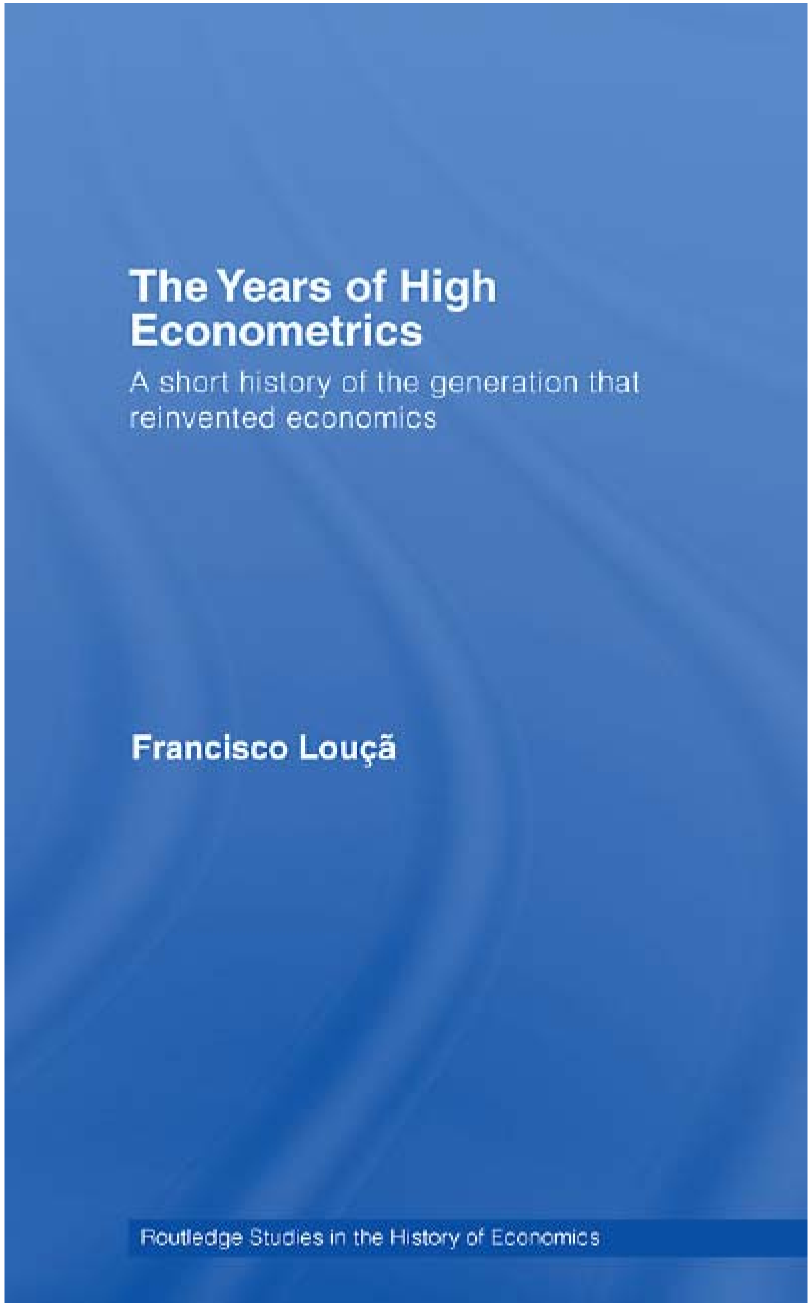

The Years of High Econometrics

The creation of econometrics was the most important transformation in twentieth-century economics and the establishment of the Econometric Society, the Cowles Commission and the journal *Econometrica* were just three of the events that would shape its development. Taking Ragnar Frisch as the narrator, Francisco Louçã presents a comprehensive history of this fascinating innovation.

Louçã includes all the major players in this history from the economists like Wesley Mitchell, Irving Fisher, Joseph Schumpeter and Maynard Keynes to the mathematicians like John von Neumann, as well as the statisticians like Karl Pearson, Jerzy Neyman and R.A. Fisher. He discusses the evolution of their thought, detailing the debates, the quarrels and the interrogations that crystallised their work. Louçã even offers a conclusion of sorts, suggesting that some of the founders of econometrics became critical of its later development.

This book will be of great interest to students, academics and researchers alike in the history of economics, not least because it contains archive material that has not been published. It would be a welcome accompaniment to students taking courses on statistics and probability, econometric methods, macroeconometrics and the history of economic thought.

Francisco Louçã is Professor of Economics at the ISEG in Lisbon. He is also a member of Parliament.

The Years of High Econometrics

A short history of the generation that reinvented economics

Francisco Louçã

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Characters

Founders of the Econometric Society (present at the 1930 Cleveland Conference)

Frisch, Ragnar (1895–1973). See Chapter 1 for a short biography.

Hotelling, Harold (1895–1973). Statistician and Professor at Columbia University until 1946, and then at the University of North Carolina. He was responsible for important developments in correlation analysis, hypothesis testing and the estimation of confidence regions. His contributions to economics include the revival of the marginalist revolution, as it came to be known in the 1930s, namely in the field of Paretian welfare economics. (Chapters 2, 5, 6, 8)

Menger, Karl (1902–85). Born in Vienna, studied physics and mathematics and attended meetings of the Vienna Circle, taking part in the discussions about science and philosophy, with another young student, Kurt Godel. He was attracted to economics by a lecture given by his supervisor, Hans Hahn, in 1925. Menger was invited by Brouwer to Amsterdam for two years but, as they quarrelled, their collaboration was soon terminated. In 1927, he was invited by Hahn to take a chair in geometry in Vienna. Moved to Notre Dame, USA, in 1938, where he developed work on probability, geometry and the algebra of functions and, with Morgenstern, on game theory. Finally, he moved to Chicago. (Chapter 2)

Mills, Frederick (1892–1964). Studied with Mitchell at Berkeley and followed him to Columbia University. Took a job at the US Commission on Industrial Relations and in 1914, as a young graduate, travelled on behalf of the Californian Commission of Immigration and Housing under an alias, working for farms and lumber camps in order to report on the work conditions there. Finished his Ph.D. at Columbia University in 1917, where he was hired in 1919 as an instructor and in 1920 as a Professor of Business Statistics. Joined the National Bureau of Economic Research (NBER) staff in 1924. Was the president of the American Statistical Association in 1934 and president of the American Economic Association in 1940. Co-editor of *Econometrica* for its first year. (Chapter 2)

Ogburn, William (1886–1959). An institutionalist sociologist, he finished his Ph.D. in 1912. He was the editor of the *Journal of the American Statistical Association* from 1920 to 1927 and taught from 1919 to 1927 at the Sociology Department of Columbia University. Ogburn then moved to Chicago for the rest of his career until 1951. (Chapter 2)

Ore, Oystein (1899–1968). Norwegian mathematician, who contributed to research in abstract algebra, number theory and combinatorics, namely graph theory. Was at Yale from 1927 to 1945 and then returned to Norway. (Chapter 2)

Rogers, James Harvey (1886–1939). Finished his Ph.D. in 1916 and took a job as a Professor at Missouri University (1923–30). Then moved to Yale (1930–9) and was an adviser to President Roosevelt. He died in a plane crash in Rio de Janeiro. (Chapter 2)

Roos, Charles (1901–58). Professor of Mathematics at Cornell (1926–8), then Professor of Econometrics at Colorado College (1934–7). Research director at Cowles (September 1934 to January 1937), secretary and member of the executive committee of the American Association for the Advancement of Science (1928–31). Founder and secretary-treasurer of the Econometric Society (1931–2) and its first secretary (1932–6). Vice-president of the Society in 1947 and president in 1948. Resigned from his position as director in January 1937 to take a business job at the Econometric Institute, Inc. (Chapters 2, 11)

Rorty, Malcolm (1875–1936). Colonel Rorty graduated as a mechanical and electrical engineer from Cornell. Vice-president (1922) and president (1930) of the American Statistical Association. Chief statistician of ATT, then vice-president of the Bell Company and later vice-president of ATT. Published works on the theory of probability applied to traffic problems. One of the founders of the NBER. In his own words, took 'an ultra-conservative viewpoint'. (Chapter 2)

Schultz, Henry (1893–1938). Born in Polish territory under the rule of the Russian Empire, he emigrated to the US and finished his Ph.D. at Columbia University in 1926 with Moore. Studied with Karl Pearson in London. Became one of the leading Walrasians in the US. After becoming established in Chicago, Schultz was the editor of the *Journal of Political Economy*. His contributions include the statistical analysis of demand and supply functions. Died in a car accident in 1938; in order to replace him, the university invited members of the Cowles Commission staff. (Chapters 2, 3, 8)

Schumpeter, Joseph (1885–1950). Born in Austro-Hungary, studied with Bohm-Bawerk. In 1919–20, Schumpeter was the Austrian Minister of Finance; in 1920–4 he was the president of a private bank. Unsuccessful in both tasks, Schumpeter returned to his academic life. He had taught at Columbia University for the year 1913–14 and had good connections in the

US academic world, so that in 1932 he obtained a job at Harvard, definitively abandoning Bonn. Schumpeter remained at Harvard for the rest of his career and developed his opposition to Keynes and his own version of neoclassical economics. He was part of the founding group of the Econometric Society. His main works were *Business Cycles* (1939) and the posthumously published *History of Economic Analysis* (1954). (Chapters 2, 3, 6, 11)

Shewhart, Walter (1891–1967). Took a Ph.D. in physics in California and in 1918 joined the Western Electric Company, a Bell affiliate, and then the Bell Telephone Laboratory created in 1925. Contributed to research into control theory and to the philosophical argument of operationalism. (Chapter 2)

Snyder, Carl (1894–1964). Studied in Iowa and then in Paris. His first job was as a journalist, and then he developed his skills as a statistician and became the chief statistician at the Federal Reserve Bank of New York. He was president of the American Statistical Association. (Chapter 2)

Wiener, Norbert (1894–1964). Born into a family of Russian Jews that emigrated to the US. His father taught mathematics at Harvard. Norbert studied zoology at Harvard, then philosophy and mathematics and took a Ph.D. in mathematical logic. He then went to Europe to study philosophy and logic with Bertrand Russell and Hilbert and thereafter maintained a close relationship with European science, namely through Fréchet, Lévy and other mathematicians. Took a job at MIT in 1919, where he made his career. Studied probability theory and Brownian motion, communication theory, cybernetics and quantum mechanics, becoming a leading figure in harmonic analysis in the 1930s. Hostile to neoclassical economics, he resigned from the Econometric Society in 1936, because of 'misgivings regarding the possibilities of employing more than elementary statistical methods to economic data' (Wiener to Roos, 12 April 1936, quoted in Mirowski, 2002: 64). (Chapter 2)

Wilson, Edwin (1879–1964). Studied with Gibbs at Yale, obtaining a Ph.D. in 1901. In 1906, he took a job at Yale, and in 1907 moved to MIT, and then to Harvard in 1922. Studied theory of relativity, vector analysis, then probability and statistics in applications to biology and astronomy. (Chapter 2)

Wedergang, Ingvar (1891–1961). A Norwegian statistician and economist, who graduated from Oslo and worked at the Central Bureau of Statistics. Took a Ph.D. in 1925 and the following year became Professor of Economics and Statistics, preceding Frisch in this post. Wedergang contributed to empirical research in economics. (Chapter 2)

Absent from the Conference but elected to the first Council

Amoroso, Luigi (1886–1965). Mathematician and Professor in Rome, he took a job at Bari in 1914 teaching financial mathematics, then at Naples University, and finally returning to Rome. Followed Pareto in economic theory. (Chapters 2, 11)

von Bortkiewicz, Ladislau (1868–1931). Born in St Petersburg, Russia, into a Polish family. Studied political economy and statistics after graduating in law and took a Ph.D. at Gottingen in 1893, joining the University of Berlin in 1901 as Professor of Statistics. A friend of Walras and a competent mathematical economist sympathetic to the Lausanne approach, Bortkiewicz nevertheless preferred the classical approach. Worked on actuarial science and political economy and proposed changes to the Marxian concept of profit and prices. Strongly opposed Karl Pearson's methods in statistics. He was contacted by Frisch in the early days of the preparation for the Cleveland conference, but died soon afterwards. (Chapter 2)

Bowley, Arthur (1869–1957). Trained as a mathematician and statistician, he was the author of the first English textbook on statistics. Bowley was the dominant personality at the London School of Economics and argued for the development of quantitative and statistical methods. (Chapter 2)

Divisia, François (1889–1964). Graduated as an engineer at the Ecole Nationale des Ponts et Chaussées, under the influence of Colson, the first mathematical economist in France. Strongly committed to liberal ideas, he worked on monetary theory and other areas of econometric application. (Chapters 2, 3, 4, 6, 7)

Fisher, Irving (1867–1947). Graduated from Yale and finished his very influential Ph.D. in 1892, under the physicist Willard Gibbs and the economist William Sumner. Focused on monetary theory, namely on interest and capital, and eventually became the first famous economist in the US. Although his influence had waned by the end of his career, namely after his failure to anticipate the 1929 crisis, he was respected as one of the founders of neoclassical economics in the US. Like some of the British statisticians and economists, Fisher was also a devout eugenicist. (Chapters 2, 3, 4, 5, 6)

Zawadzki, Wladyslaw (1885–1939). Professor at Warsaw. (Chapter 2)

Also invited, among others, but absent from the Cleveland meeting

Cassel, Karl Gustav (1866–1945). Mathematician, born in Sweden, who moved to Germany to study economics, although his major influence was British neoclassical thought. Worked on general equilibrium theory and the interest rate. Cooperated with Keynes in 1922 on a currency reform project for Germany, although they tended to be in disagreement on theoretical matters. (Chapter 2)

Clark, John Bates (1847–1938). Studied in Heidelberg, Germany, with Karl Knies. One of the founders of the marginalist revolution in the USA, he taught at Columbia University (1895–1923), where he was an opponent of the institutionalists and of Veblen in particular. (Chapters 2, 3, 11)

Clark, John Maurice (1884–1963). Took his Ph.D. at Chicago in 1910, where he taught until 1926, when he moved to Columbia University. Unlike his father, John Bates Clark, he supported the institutionalist school. (Chapter 2)

Colson, Clément (1853–1939). A French engineer, who was at school with Poincaré. Was a disciple of Dupuit and argued in favour of liberal policies. Divisia, Rueff and Roy were his students at the Ecole Nationale des Ponts et Chaussées. He contributed to research into applied economics, working for instance on the economics of transportation. Wrote a textbook on economics. Although he defended the use of formal rigour, Colson was very sceptical about the widespread use of mathematics in economics. (Chapter 2)

Ezekiel, Mordecai (1899–1974). Joined the Bureau of Agricultural Economics when it was created in 1922. Took a Ph.D. at the University of Minnesota in 1924 and moved to the Federal Farm Board in 1930. Contributed to the definition of multiple regression techniques. Was elected a Fellow of the Econometric Society in 1935, together with Marschak. (Chapter 2)

Gini, Corrado (1884–1965). Graduated in law at Bologna with a thesis on gender statistics, later developing a career in statistics, applied research and index number theory. Accepted an appointment from Mussolini in 1926 to the presidency of the Central Institute of Statistics and was supposed to report directly to the dictator. Resigned in 1932. In 1921, Gini defined the coefficient commonly used to measure inequality in the distribution of wealth. In 1939, he attacked both the Neyman–Pearson approach and R.A. Fisher's statistical methods, from a kind of Bayesian standpoint. (Chapter 2)

Keynes, John Maynard (1883–1946). Educated at Cambridge, Keynes made an impact with his work on probability theory and then became the most famous British and European economist. His polemical dispute with Karl Pearson in 1910–11 on inference and statistical estimation anticipated his later debate with Tinbergen. His major work, *The General Theory of Employment, Interest and Money*, deeply influenced economics. (Chapters 2, 3, 7, 11)

Moore, Henry Ludwell (1869–1958). Studied with Menger, a disciple of Walras, in Vienna. One of the defenders of the Lausanne school, his main research was into the empirically oriented statistical derivation of demand curves. Henry Schultz, his disciple, continued his programme. (Chapter 2)

Ohlin, Bertil (1899–1979). Studied at Lund and Stockholm with Heckscher, then at Harvard with Taussig. Took a Ph.D. at Stockholm in 1924. Opposed Keynes on the question of the war reparations. Was the Minister of Commerce in 1944–5 in a Liberal Party government. One of the pioneers of macroeconomic thinking, he developed the modern theory of the dynamics of trade. Was awarded the Nobel Prize in 1977. (Chapter 2)

Pigou, Arthur C. (1877–1959). Graduated from Cambridge in 1900 and was the heir and successor of Marshall in his chair. Pioneered welfare economics and shaped the Cambridge neoclassical version of economics, strongly attacked by Keynes in the 1930s. (Chapter 2)

Persons, Warren (1878–1937). Graduated from Wisconsin in 1899, taking his Ph.D. in 1916. Became well known for his development of descriptive statistical analysis. (Chapters 2, 3)

Alfonso di Pietri-Tonelli was also invited but was absent. (Chapters 2, 6)

Ricci, Umberto (1879–1946). Taught at Giza, in Egypt, and was one of the founders of the Faculty of Economics in Istanbul. (Chapter 2)

Rueff, Jacques (1896–1978). Simultaneously developed both a scientific and a political career: he was a member of the French delegation to the reparation conferences after the end of the Second World War, then a judge at the European Court and finally an adviser to De Gaulle. Wrote on finance and philosophy and replaced Jean Cocteau at the French Academy. (Chapter 11)

Slutsky, Evgeny (1880–1948). Studied at Kiev University and was expelled for political reasons by order of the Czarist government. He later took a Ph.D. in law but devoted his talents to the study of statistical theory. Worked on demand theory following the Lausanne predicaments. Moved to Moscow in 1920, working on probability theory at the Conjuncture Institute and teaching at Moscow University. (Chapters 2, 3, 6)

First Fellows (other than the previous figures)

Anderson, Oskar (1887–1960). Studied in St Petersburg and became Chuprov's assistant, preparing a Ph.D. thesis on variance-difference methods for time series. Also took a law degree. Was a colleague of Slutsky for three years in Kiev. Moved to Budapest and then to Sofia. Finished his academic career in Munich. Tintner described him as the most famous statistician in Central Europe. (Chapter 2)

Aupetit, Albert (1876–1943). Follower of Walras and Professor at Paris University. (Chapter 2)

Bonisegni, Pasquale (1869–1939). Follower of Pareto, who taught at Lausanne and published on general equilibrium theory. (Chapter 2)

Evans, Griffith (1887–1973). Studied at Harvard, and took a Ph.D. in 1910 on Volterra's integral equation. He had spent some time with Volterra in Rome and then with Planck in Berlin. Accepted a job in Houston and then moved to Berkeley in 1933, where he decided to revitalise the mathematics department, to which Neyman and Tarski were appointed. Wrote on mathematical economics. (Chapter 2)

Haberler, Gottfried (1901–95). Born in Austria and emigrated to the USA. A member of the Mises circle, he was influenced by the Austrian point of view, actively promoting liberal views in international trade and opposing Keynesian

policies. In 1937, he undertook a study of business cycle theories for the League of Nations, which was empirically estimated by Tinbergen. (Chapter 2)

Kondratiev, Nikolai (1892–1938). Kondratiev studied at the University of St Petersburg, attending courses given by Tugan-Baranowsky. His initial professional work was in the area of agricultural economics and statistics. On 5 October 1917, aged twenty-five, he was appointed Minister of Supply of the last Kerensky government, which only lasted for a few days. In 1920, he created and directed the Conjuncture Institute. In 1924, he visited England, Germany, Canada and the US. Kondratiev was removed from the directorship of the Institute in 1928 and arrested in July 1930; in August 1930, Stalin called for his execution but instead Kondratiev was sentenced to eight years' imprisonment. Although his health deteriorated, he still managed to continue his research and even decided to prepare five new books. But a second trial followed and Kondratiev was then sentenced to death and executed. He was forty-six at the time of his murder and was only rehabilitated almost forty years later, on 16 July 1987. (Chapters 2, 3, 4)

Mitchell, Wesley (1874–1948). Took a Ph.D. at Chicago in 1899, where he studied with Thorstein Veblen and the philosopher John Dewey. He taught at Chicago (1899–1903), Berkeley (1903–12) and then Columbia University (1913–44). Mitchell created the National Bureau of Economic Research (NBER) in 1920 and was its research director until 1945. His most important work was *Business Cycles* (1913), after which he developed an empirical approach to the economic cycles that became the main focus of the NBER for a long time. (Chapters 2, 3, 6)

Schneider, Erich (1900–70). Studied physics and mathematics. Colleague of Schumpeter while he was at Bonn. Then taught at Dortmund and Kiel. He was elected a Fellow in the first list.

Vecchio, Gustavo del (1883–1972). Studied the philosophy of history under Labriola, as well as economics. His Ph.D. (1904) was about the theory of monopoly. Researched into the theory of capital and criticised Keynes for ignoring the long-term perspective. Was a Professor at Trieste, Bologna, Boconni and Rome. Was Minister of Finance of the Italian government (1947–8) and then director of the IMF (1948–50). (Chapter 2)

Zeuthen, Frederick (1888–1959). Published on economic methods and monopolistic competition. Professor at Copenhagen. (Chapter 2)

Other characters

Akerman, Johan (1896–1982). A Professor at Lund, he researched into business cycles, namely into the synchronicity between short-term and longer cycles. Developed models of endogenous cycles. Published on epistemology and institutional economics. Was present at the Lausanne conference of the Econometric Society. (Chapters 2, 7)

Allais, Maurice (1911–. . .). Influenced by Walras and Pareto, he provided the first axiomatic and proofs of theorems on welfare. Shared many of the values of the Lausanne school, including Walras's argument in favour of the nationalisation of land and capital taxation for the redistribution of income. Was the originator of the overlapping generations model and the Allais Paradox in the theory of choice under uncertainty. Mentor of Debreu and Malinvaud. Was awarded the Nobel Prize in 1988. (Chapter 11)

Arrow, Kenneth (1921–. . .). Took a Ph.D. at Columbia University in 1950, where he studied with Hotelling and Wald. Arrow was a research associate at the Cowles Commission from April 1947, working with Marschak and Koopmans, and was still a research consultant after accepting his Stanford job in 1948, where he remained for most of his academic life. He was then made a Fellow of the Econometric Society in 1951. Worked at the Rand Corporation during that period. Arrow contributed to the impact of the Neo-Walrasian general equilibrium approach, namely through the Arrow–Debreu proof of the existence of equilibrium; in 1971, with Frank Hahn, he published a reappraisal of the general equilibrium theory. He was awarded the Nobel Prize in 1972. Later on, Arrow expressed a certain disappointment with the general equilibrium approach. (Chapter 2)

Le Corbeiller, Philippe (1891–1980). A French engineer and a specialist in radio electricity and electromagnetic waves, he was present at the first Econometric meeting in Lausanne. Professor at the Conservatoire National des Arts et Métiers, Paris. (Chapters 2, 4, 5, 6)

Cowles, Alfred (1891–1984). Graduated at Yale. A businessman and statistician, he was also the founder of the Cowles Commission. Prior to its foundation, Cowles maintained a private organisation for ten years dedicated to statistical research into capital markets. (Chapters 2, 6)

Darmois, Georges (1888–1960). Statistician and Professor at Nancy and Paris. President of the National Institute of Statistics. Researched into probability. Present at the first econometric conference in Lausanne. (Chapters 2, 9)

Davis, Harold (1892–1974). Mathematician and Professor of Mathematics at Indiana University (1923–37). Belonged to the first staff of the Cowles Commission and was its acting director from February to August 1937. Moved to the North-Western University in 1937. Associate editor of *Econometrica*, as well as of the *Bulletin of the American Mathematical Society*. Published *The Theory of Econometrics* (1941), as well as works on the history of mathematics and the philosophy of sciences. (Chapter 2)

Domar, Evsey (1914–97). Russian born, he took a Ph.D. at Harvard in 1947. Domar was a research associate at the Cowles Commission from July 1947 until July 1948. His contributions focused on growth theory and economic history. (Chapter 5)

Fanno, Marco (1878–1965). Studied international economics, monetary policy and economic fluctuations. From 1915 onwards, he taught at Parma and then Padua. During the Second World War, he was persecuted as a Jew and could not teach. His works were noticed by Edgeworth and were especially appreciated by Schultz. Was present at the first econometric conference in Lausanne. (Chapter 2)

Fisher, Ronald Aylmer (1890–1962). Graduated in mathematics and physics at Cambridge. Replaced Karl Pearson at University College, London (1933), then moved to Cambridge in 1943. His important contributions to statistical theory include the analysis of variance, the concept of likelihood, the definition of the design of experiments, applications to genetics and the theory of natural selection, the definition of sampling distributions from small samples, and considerations on the logic of inductive inference and the theory of statistical estimation. Opposed Neyman and Egon Pearson on the theory of statistics. (Chapters 3, 8)

Fréchet, Maurice (1878–1973). A student of Hadamard, he introduced the concept of metric space and made important contributions to the calculus of probabilities, differential and integral calculus and topology. He was one of the founders of the Institut Henri Poincaré in Paris. (Chapters 5, 9)

Haavelmo, Trygve (1911–99). Was a research assistant from 1933 to 1938 at the Institute of Economics, Oslo, and, in 1938–9, a lecturer in statistics at the University of Aarhus. When he was in the US during the war, Haavelmo was the commercial attaché at the Norwegian Embassy. A research associate at the Cowles Commission from July 1943 onwards, he joined Chicago University in 1946, obtaining his Ph.D. in the same year from Oslo University. He returned to Norway in 1947, as a head of department at the Ministry of Commerce, Industry and Finance, and then took up a position as Professor at the University of Oslo from 1948 to 1979. He received the Nobel Prize in 1989. Fellow of the Econometric Society. (Chapters 2, 4, 8)

Harrod, Roy (1900–78). A Professor at Oxford, 1924–67, he enjoyed a close relationship with Keynes. Argued for a dynamic rather than a static approach and made original contributions to the multiplier-accelerator model, imperfect competition, the IS-LM curve and to the theory of growth. Also contributed to the revival of business cycle theory. (Chapters 2, 7)

Hayek, Friedrich (1899–1992). Born in Vienna, was a student of Wieser and Bohm-Bawerk. Worked on monetary cycle theory and participated in 1935 in the debate on socialist calculation. Established himself at the LSE in 1931 and moved to Chicago in 1950. Was always a liberal opponent of Keynes. Was awarded the Nobel Prize in 1974. (Chapters 3, 7)

Hicks, J.R. (1904–89). Hicks was part of the LSE generation that, under the influence of Robbins, contributed to the revival of the marginalist approach. His review of Keynes's *General Theory* introduced the IS-LM and the

Neo-Keynesian synthesis. He was awarded the Nobel Prize in 1972 and later worked on causality and the irreversibility of time in economics, leading him to adopt a very critical stance in relation to the synthesis. (Chapters 2, 7)

Hurwicz, Leonid (1917–. . .). Graduated from the University of Warsaw. Joined the Cowles Commission as a research associate in 1942, and then in 1944 accepted the position of a full-time research associate and finally took over the directorship of the Commission from Koopmans in 1950. A consultant at the Rand Corporation from 1949 onwards. From 1942 to 1944, he joined the Faculty of Meteorology at the University of Chicago. In 1946, he was appointed Associate Editor of the *Journal of the American Statistical Association*. (Chapter 2)

Kalecki, Michal (1899–1970). A Polish citizen, Kalecki worked for most of his life at Warsaw University. Although he anticipated many of the concepts used later on by Keynes, his contributions remained unknown for a long time because of the fact that he published in Polish. (Chapters 2, 6)

Klein, Lawrence (1920–. . .). Took a Ph.D. at MIT in 1944, joined the Cowles Commission in 1944, moved from 1948–51 to the NBER and returned to the Cowles Commission in 1951. Fellow of the Econometric Society in 1948. Klein moved to England to escape McCarthyism and developed his work in Oxford. When he returned to the US, he was elected president of both the Econometric Society and the American Economic Association. Was awarded the Nobel Prize in 1980. (Chapter 3)

Knight, Frank (1885–1972). Together with Jacob Viner, Knight was the dominant personality at Chicago, from the 1920s to the late 1940s. He led the economics department, developing a specific brand of the neoclassical school that differed from the Walrasian approach. He criticised the Austrians, Marshallians and institutionalists. Opposed to the generalisation of quantitative techniques, Knight argued with Henry Schultz and Lange on this matter. Although defending a laissez-faire policy, he condemned capitalism and the market system from the ethical point of view, as opposed to the Chicago School of Friedman and Stigler, which came after him. (Chapter 3)

Kolmogorov, Andrey (1903–87). A Russian mathematician, who worked at Moscow University. His main contributions were made in the fields of probability theory and topology, but he also published works on turbulence, classical mechanics and algorithmic complexity theory. (Chapter 8)

Koopmans, Tjalling (1910–85). Took a Ph.D. at Leiden in 1935 under the supervision of Kramers and Tinbergen. From 1938 to 1940, he replaced Tinbergen in Geneva for the business cycle research project of the League of Nations. Studied with Frisch in Oslo before finishing his Ph.D., and presented the Neyman–Pearson approach in seminars. Met Marschak at the Oxford conference of 1938 and developed a close relationship with him. In 1940–1, was at Princeton. Joined the Cowles Commission in July 1944 and in 1946 was appointed to the Chicago faculty. Vice-president of the Econometric Society in 1949 and president in 1950. Shared the Nobel Prize with Kantorovich for their research into the optimal allocation of resources in 1975. (Chapters 2, 8, 10, 11)

Lange, Oskar (1904–65). Lectured at Cracow from 1931 onwards, joining the University of Michigan in 1936, followed by the University of California (1937–8) and Chicago (since 1938). A research associate at the Cowles Commission from September 1939 and acting editor of *Econometrica* (1943–5). Played an active part in the socialist calculation debate with Hayek in 1936–7, arguing in favour of the feasibility of socialism. In 1945, he became the Polish Ambassador in the US, and then Ambassador to the UN (1946–9). Returned after this to Poland, to Warsaw University and government responsibilities. (Chapters 2, 3, 7, 11)

Leavens, Dickson (1887–1955). Took his master's degree in mathematics at Yale (1915). Was part of the staff of the Yale college in China from 1909 to 1927. A research associate at the Cowles Commission (1936–47) and managing editor of *Econometrica* (1937–48). (Chapter 2)

Leontief, Wassily (1906–99). Born in Russia, he studied in Berlin where he took his Ph.D., and then joined the staff at Harvard in 1932. Was responsible for the development of Input–Output analysis, under both Marxian and Walrasian influences. Later, he criticised the misuse of mathematics and the lack of empirical relevance of econometric works. Was awarded the Nobel Prize in 1973. (Chapters 2, 4)

Malinvaud, Edmond (1923–. . .). A student of Allais, like Debreu. In 1950, he joined the Cowles Commission. Worked on Walrasian general equilibrium theory and on the theory of choice under uncertainty. (Chapter 11)

Marschak, Jacob (1898–1977). A prisoner of the Czar for his opposition activities, the young Marschak studied at the Technological Institute at Kiev. He then became the Minister of Labour in the Cossack-Menshevik Republic of Terek in the North Caucasus and was exiled to Berlin in 1919, where he made his first contribution to economics with an anti-Austrian paper in 1923 in the socialist calculation debate. Took his Ph.D. at the University of Heidelberg in 1922. Taught at Heidelberg (1930–3) and Oxford (1933–5). Was the research director of the Cowles Commission from January 1943 onwards, resigning in June 1948. Vice-president of the Econometric Society in 1944 and 1945 and president in 1946. Member of the Editorial Board of *Econometrica* (1943–6) and collaborating editor of the *Journal of the American Statistical Association*. Professor at Chicago and then at UCLA. (Chapters 2, 3, 7, 11)

Morgenstern, Oskar (1902–77). Replaced Hayek as director of the Austrian Institute for Business Cycle Research. Developed a critical attitude towards the Austrian theory of capital. When he was dismissed by the Nazis in 1938, he left for Princeton, where he became a close friend of Kurt Godel. Wrote the *Theory of Games* (1944) with von Neumann, a ground-breaking book. (Chapters 2, 3, 8)

Mosak, Jacob (1913–. . .). Took a Ph.D. at Chicago in 1941 on general equilibrium and international trade. Joined the Cowles Commission as a research associate in September 1939. Worked with Henry Schultz at Chicago (1935–8) and then moved to Columbia University in 1948. (Chapters 2, 7)

Nelson, William (1900–36). Born in Ottawa and graduated from Toronto University (1921). Assistant editor of *Econometrica* and research associate at the Cowles Commission from 1932 until his premature death in 1936. (Chapter 2)

von Neumann, John (born Johann, 1903–57). Born in Hungary, he took a Ph.D. in mathematics from Budapest University plus a Ph.D. in chemistry from Zurich University. Moved to Berlin in 1927 and then to Princeton in 1932. Was part of the Manhattan Project. Left immense contributions to computing, game theory and other topics, which greatly changed economics and other sciences. Follower of Hilbert, created concepts of activity analysis that were developed by Koopmans, and was the first to use Brouwer's fixed point theorem to demonstrate the existence of an equilibrium, impressing his contemporaries with the axiomatisation of choice under uncertainty. Resigned from the Econometric Society in 1947, claiming a lack of interest. (Chapter 2)

Neyman, Jerzy (1894–1981). Studied physics and mathematics at Kharkhov and was awarded a Rockefeller scholarship to study with Karl Pearson in London in 1925. He then spent one year in Paris attending lectures by Borel, Lebesgue and Hadamard. Created a biometric laboratory in Warsaw, but, in 1935, moved to University College, London, to work with Egon Pearson. Later he moved to Berkeley. His cooperation with Egon Pearson generated some of the papers in the area of statistical theory that had the greatest impact on economics. (Chapters 3, 8, 11)

Pareto, Vilfredo (1848–1923). An engineer, who studied physics and mathematics at the University of Turin. Was later given the position of director of the railway company. In 1893, he succeeded Walras at Lausanne. Proposed a theory of the elites, as well as making other contributions to general equilibrium theory. Was appointed a Senator by Mussolini. (Chapter 5)

Pearson, Egon (1895–1980). Graduated from Cambridge, where he attended lectures on the theory of errors in astronomy, with Yule and Eddington. Cooperated with Neyman in defining a new theory of estimation, statistical inference and testing. Head of the Department of Applied Statistics at University College, London, after the retirement of his father, Karl Pearson. (Chapter 3)

Pearson, Karl (1857–1936). Studied mathematics at Cambridge and then German medieval literature at Berlin and Heidelberg. He became a close follower of Galton and was introduced to biometry and evolutionary theory, contributing to the research into heredity and eugenics. The founder of *Biometrika*, a leading journal, Pearson developed important insights into linear regression and the classification of distributions. (Chapters 3, 11)

Reiersol, Olav (1908–2001). A research assistant of Frisch at Oslo (1936–7). Took a Ph.D. at Stockholm in 1945, where he sought refuge from the war and then went to Cambridge to study with R.A. Fisher (1946). Was given a job at Columbia University and then became a research fellow for Cowles in the summer of 1949. Became a Fellow of the Econometric Society from 1952 onwards. Worked on identifiability and instrumental variables, and later on genetic algebra. (Chapter 2)

Robinson, Joan (1903–83). A prominent member of the Cambridge and Keynesian school, she developed the theory of imperfect competition and became a reader of Marx. In 1950, she declined the invitation by Frisch to become vice-president of the Econometric Society, as she argued that she could not read Econometrica. Her major work is *The Accumulation of Capital* (1965), an attempt to extend Keynesianism to the long-term perspective. (Chapter 11)

Rosenstein-Rodan, Paul (1902–85). Born in Poland, he studied in Vienna under the Austrian school. In 1930, he moved to Britain and taught at the LSE until 1947, and then later to the World Bank and MIT, where he researched into economic development. He delivered a paper to the Lausanne conference of the Econometric Society. (Chapter 2)

Samuelson, Paul (1915–. . .). Took a Ph.D. at Harvard in 1941. Joined MIT in 1940. Published his thesis in 1947, which became a major influence on economics. Was awarded the Nobel Prize in 1970. (Chapter 2)

Sraffa, Piero (1898–1983). Brought to Cambridge by Keynes in the 1920s, he criticised the Marshallian theory of the firm and developed the theory of imperfect competition. Edited Ricardo's works and developed Ricardian themes, tinged with Marxism. A close friend of Ludwig Wittgenstein, he cooperated with him on the preparation of his *Philosophical Investigations*. Joined the Econometric Society, but soon withdrew, since he was not elected a Fellow. (Chapter 2)

Taussig, Frank (1859–1940). Editor of the *Quarterly Journal of Economics* (1889–90, 1896–1935) and a dominant figure at Harvard, Taussig influenced the emergence of modern economics in the US. Published on international trade and supported conservative views, adopting a critical stance towards the institutionalists, as well as in relation to some neoclassical economists. President of the American Economic Association (1904, 1905). Schumpeter was to become his successor in the chair at Harvard in 1935. (Chapter 2)

Tinbergen, Jan (1903–94). Took his Ph.D. in physics at Leiden. He was the director of the Central Planning Bureau of the Dutch government from 1945 to 1955. Founded the Econometric Institute of the Erasmus Universiteit Rotterdam. Was awarded the Nobel Prize in 1969, together with Frisch, for his work on applied dynamic models. (Chapters 1, 2, 3, 5, 6, 7, 8, 10, 11)

Tintner, Gerhard (1907–1983). Took a Ph.D. at Vienna in 1929. Was a member of the staff of the Austrian Institute of Trade Cycle Research, and then became a research fellow at the Cowles Commission (1936–7). A Fellow from 1940 onwards, Tintner was a member of the editorial board of *Econometrica* and its associate editor. (Chapter 2)

Waugh, Frederick (1898–1974). Took his Ph.D. at Columbia University in 1929. One of the most influential agricultural economists at one of the important schools of experimental econometrics, he spent one year with Frisch (1932–3), writing a joint paper with him in 1933. Frisch suggested that he should extend his stay in Europe in order to have discussions with Schneider, Divisia and Tinbergen. A Fellow of the Econometric Society from 1947 onwards. (Chapter 2)

Yntema, Theodore O. (1900–85). A student of Schultz, he took a Ph.D. from Chicago in 1929. A research director at the Cowles Commission from 1937–43 when it moved to Chicago. From 1940 onwards, Yntema was a director at the NBER. In 1949, he joined the Ford Company as vice-president for finance, and in 1950 was made director. A Fellow of the Econometric Society. (Chapters 2, 7)

Introduction

Econometrics was the most adventurous and successful innovation introduced into economics during the course of the last century. By turning economics into social physics, econometrics claimed to reach the heights of pure science. The perpetrators were a new generation of economists and statisticians, some of them immigrants from physics and mathematics, who challenged both the established references and routines of economics. As a result, econometrics proudly led to the professionalisation of the discipline, completely transforming its landscape and promising a new capacity for measurement, estimation, prediction and control.

This book is an essay in biography and its subject matter is the collective effort of that brilliant generation of economists who aspired to transform economics into a rigorous science. The powerful econometric movement took shape in the 1930s, the years of high theory – the concept that Shackle used to describe the period of the inception of the Keynesian revolution, a period that cannot be thoroughly understood unless both movements are contrasted. In a sense, both the Keynesian revolution and the econometric revolution shared the same motivation: to extend the empirical capacity of economics, broadening its analytical scope and strengthening its capacity for designing a control policy. As the story unfurls, it becomes obvious that the young econometricians with Keynesian leanings were more radically engaged in such a task than the Cambridge circle itself, and this was the profound reason for a great deal of the harsh criticism and disappointment that they faced.

Furthermore, the acceptance of the epistemological primacy of a very peculiar type of simple mathematical formalism contributed to the marginalisation of some of the major theoretical alternatives developed in the first half of the century. Evidence shows that the endorsement of the urgent political agenda for action against unemployment and the dangers of war were instrumental in determining the victory of a specific mathematical drive, and that the econometric programme as it came to be conceived in these incipient years was shaped by this movement.

As a consequence of its impact, econometrics became a tool for the reconstruction of neoclassical economics, which sought to be redescribed in the language of mathematical formalism and statistical inference and estimation, and simultaneously responsible for the decay of heterodox alternatives elsewhere. In that sense, modern economics was a tributary of that success. But the emergence of modern neoclassical theory from the convergence between the new mathematical approach and older general equilibrium theories would prove to be a difficult process, particularly in relation to the core subject of the 1930s: business cycles. On the one hand, much of the empirical and concrete research was developed under the auspices of Wesley Mitchell, who was hostile to the general equilibrium paradigm. In the late 1930s, on the other hand, the Keynesian circle was to develop a critique of 'classical' economics, hastened by the impact of the Great Depression, which challenged the concept of equilibrium as an accurate description of reality and provided the dramatic argument in favour of new policies. The years of high theory were the fascinating period in which all these arguments were being constructed and disputed, a time when adherence to different schools did not raise barriers against cooperation and dialogue and no canon was imposed.

I have chosen Ragnar Frisch to serve as the narrator for this drama, although this biography is less about his life than about the movement that he played such a crucial role in creating, since he, better than anyone, embodied the intellectual ambiguities and motivations of econometrics, as well as the courage and devotion that were shown to the cause. Indeed, rather than being simply another character in the plot, Frisch wrote the play himself. He conceived, proposed, conversed, gathered his fellow-thinkers together, instructed, challenged and edited: for a decade, Frisch was the centre of econometrics. The Econometric Society, the Cowles Commission, *Econometrica* and the first conferences were all the fruit of his intense efforts.

Furthermore, since, with the exception of rare conferences, letter writing was the only real means of communication during those years, the construction of the institutions and the definition of the research programme were discussed at length in the correspondence between many of the older and younger econometricians. This epistolary exchange bears testimony to the doubts, divergences, quarrels, innuendoes, alliances and strategies of the econometricians, as well as their inspiration and devotion to the objectives of their cause, so that the biography of the movement itself and of this generation of economists can be convincingly written from the point of view of both what was never published and what was not intended to be published in journals. In reading their intense correspondence, we can feel econometrics in the making.

This is also the biography of an idea, that of the assumption of the primacy of mechanics as the criterion for scientific legitimacy. This influence was not new in economics; in fact, it was a replica of the intellectual movement that had generated the previous neoclassical revolution. But physics itself had dramatically changed by the end of the first quarter of the twentieth century and, as econometrics emerged as a specific movement, the ideal types of natural science were being radically modified.

During the eighteenth century, the model of models in mechanics was the clock. The wonders of the mechanical precision of the clock had captured the

imagination of both ordinary people and scientists alike: it managed to describe time, it required specialised manufacture and it was proof of the correct application of mathematical rigour. In the ensuing nineteenth and early twentieth centuries, the model was to become the engine for exact sciences, providing domesticated movement from an external source of power on which an internal regulation had been imposed. In economics, business cycles were discussed for a long time with a view to determining the precise nature of that *deus ex-machina*, the driving force, albeit using very simple devices, such as levers, pendula and other metaphors. Mechanics was seen as the authoritative model of causality in order to unveil the preordained order established in nature. But mechanics could not provide guidance for economic research, since social data relate to complex agents, institutions, rules, strategies and consequently to uncertain and changing dynamics. Therefore, the evolution of economics during the decades under scrutiny, these years of high theory, is that of the difficult transposition from the mechanical model of models to the mathematical analogy to be imposed both on the will to discipline formal reasoning and on the very nature of the research.

The difficulties of imposing the mechanical mode of thought and subsequently exploring these metaphors became particularly obvious when statistical inference and estimation were introduced. In physics, the concepts of sample and population were clearly established; consequently, the notion of error had been well-defined in astronomy and in the laboratory in relation to both general laws and controlled experiments. As economics and other social sciences lacked this framework of general laws that precisely governed the behaviour of nature, the econometricians strove to define an operational concept that could somehow unite structure and randomness, order and change. Frisch and Slutsky provided a bridge between the concept of errors in measurement (just as in astronomy, meaning errors in variables) and the concept of errors as stimuli driving the dynamics of the system (errors in equations). But it was only with Haavelmo that the bridge was finally crossed and a strategy was established for redefining econometrics as probability inference, at precisely the same time as the epic battles were being fought between R.A. Fisher's approach and the Neyman–Pearson alternative.

Yet, there was some ambiguity in this conception and application of probability theory, which was explored by another group of physicists, mathematicians and economists, who, being in close contact with the Cowles Commission – and with some of them transferring from one circle to the other – were developing another model of models at the Rand Corporation, namely that of the computer. Their model conceived of the world as inherently stochastic and not as the result of shocks impinging on stable structures. This fascinating story was recently recounted by Mirowski (2002) and it is obvious that the first econometric generation was not interested in such developments.

Finally, this is also the story of misunderstanding, disillusion and dissidence: after the Second World War, when communication was re-established between Europe and the USA, it became obvious that some of the founders of econometrics no longer followed the change of direction that the Cowles Commission was imposing under Marschak and Koopmans. Frisch never considered that he had abandoned econometrics; on the contrary, he felt that econometrics was failing him, since it ignored the priority of developing a sound mathematics of planning. Since the econometric revolution undeniably reshaped economics, it is useful for us to turn our attention back to the past and learn how these founders conceived, discussed and proposed their various alternatives.

Part I presents the central elements of the biography of this generation of young mathematical economists and the events that led to the foundational conference of the Econometric Society at the end of 1930. Part II shows how this society was constructed through arguments, themes, conferences and complicity, as well as through the creation of the new structure, composed of both the society with its journal and the Cowles Commission. Part III argues that the debates defining economics in this period were also instrumental for the definition of the field of econometrics. Finally, Part IV explores the doubts, second thoughts, problems and contradictions of some of these scientists.

This research began some years ago and, as time went by, my debt grew in relation to a number of referees and editors for the encouragement that they gave me to publish and for their comments on very preliminary versions of what would later become some of the chapters of this book (Louçã 1999a, 1999b, 2000a, 2000b, 2001, 2004). Many colleagues discussed some of these papers and chapters, and I am grateful to all of them: David Acheson (Jesus College, Oxford), Olav Bjerkholt (Oslo University), Mauro Boianovsky (Brasilia University), José Luis Cardoso (ISEG, Technical University of Lisbon), Dave Colander (Middleburgh College, Vermont), James Collins (Department of Biomedical Engineering, Boston), Guido Erreygers (Antwerp University), John Foster (Queensland University), Harald Hagemann (Hohenheim University), Hooshang Hemami (Department of Mechanics, Ohio University), Arjo Klamer (Rotterdam University), Albert Jolink (Rotterdam University), Judy Klein (Mary Baldwin College, Virginia), David Lane (University of Modena and Santa Fe Institute), Marji Lines (Udine University), Alfredo Medio (Venezia University), Stan Metcalfe (Manchester University), Philip Mirowski (Notre Dame University), Mary Morgan (London School of Economics and Amsterdam University), D.P. O'Brien (Durham University), António Sousa Ramos and José Taborda Duarte (IST, Technical University of Lisbon), Geert Reuten (Amsterdam University), Jorge Santos (ISEG), Boaventura Sousa Santos (Coimbra University), Esther-Mirjam Sent (Nijmegen University), António St Aubyn (Faculty of Agronomy, Technical University of Lisbon), Rui Vilela-Mendes (Mathematical Physics Institute, Lisbon) and Roy Weintraub (Duke University). Although in the book those preliminary sketches are now replaced by substantially different texts, the initial impulse from these discussions and comments was essential for the definition of the project. The final version was thoroughly discussed with João Ferreira do Amaral (ISEG, Lisbon), Marcel Boumans (Amsterdam University), Chris Freeman (Sussex University), Bruna Ingrao (La Sapienza, Rome) and

Stefano Zambelli (Copenhagen University), who provided numerous suggestions and criticisms. The usual caveat applies to all these contributors.

Back in 1996, Jens Andvig, Olav Bjerkholt, Kare Edvardsen (who organised the comprehensive bibliography by Frisch) and Tore Thonstadt (who organised the Frisch and Haavelmo Archives at Oslo University) accepted to be interviewed on their recollections of Frisch and I am grateful to them as well, since this story could not have been told without the emotions of those who knew and shared part of their lives with the main character in the construction of early econometrics. Pieter de Wolff kindly provided important clarifications on his past correspondence with Frisch on the project of a book on nonlinear dynamics. So did Maurice Allais and Edmond Malinvaud on their debates with Frisch in the 1960s. Mrs Marie Ragna Frisch Hasnaoui graciously gave her permission to quote from her father's work and to publish parts of his personal archive. I also wish to thank Sofia Terlica for providing the translation from Norwegian of many documents and John Elliott for helping to establish the definitive version of the English text.

Part of the research was hosted by the National Library and by the Oslo University Archive, where Frisch's documents are held, as well as by the Harvard Archives, where Schumpeter's documents are. I am grateful for the cooperation that I received from these institutions, as well as from other archives, such as that at UCLA on Marschak, the Yale Archive on the Econometric Society, the Columbia archives on Mitchell and other staff, and the archive at Adelaide University on R.A. Fisher, as well as that at the Institut Henri Poincaré in Paris. Greg Woirol and Lionello Punzo kindly furnished information on Frederick Mills and Marco Fanno respectively.

Finally, Jan Reijnders, from the Utrecht School of Economics and the Tjalling Koopmans Research Institute, invited me to stay for some time, and both his constant encouragement and the calm of Utrecht proved to be instrumental for the completion of my writing. My colleagues at ISEG and its research unit on complexity in economics (UECE) provided the best possible environment for this research. The Foundation for Science and Technology (Lisbon) funded part of this research, which is also duly acknowledged.

Part I

Foundation

1 'Not afraid of the impossible'

Ragnar Frisch (1895–1973)

Ragnar Anton Kittil Frisch was born on 3 March 1895.[1] His father, Anton Frisch, was a jeweller from an old family of mining specialists,[2] very active in local politics as a member of the Liberal Party and an elected member of the executive committee of the city council of Oslo. His mother was Ragna Fredrikke Kittilsen.

In 1913, the young Ragnar completed the normal examinations taken after secondary school. But then he suspended his studies and worked as an apprentice for some years in a firm owned by David Andersen, in order to follow his father's career in jewellery: there, Ragnar completed his probationary period as a craftsman. He was now a goldsmith, but then considered going back to school, under the influence of his mother.

Consequently, this twenty-one-year-old professional applied for admission to Oslo University. Much later, in an autobiographical note written to mark his acceptance of the Nobel Prize, Ragnar Frisch explained that facility and rapidity had been the sole criteria for his choosing the course of economics: 'we perused the catalogue of Oslo University and found that economics was the *shortest* and *easiest* study' (Frisch, 1970a: 211).[3] Economics, a recently established two-year course (1908) at the Faculty of Law, was considered to be an easy topic (Bjerkholt, 1995: xiv). Yet, Frisch excelled as a student: as an active member of the faculty, he was chairman of the educational programme of the Oslo Students Union; as an undergraduate, he prepared himself for crossing over the boundaries of economics. The year after graduation, in 1920, Ragnar married Marie Smedal.[4] Professional life was beginning for this goldsmith turned apprentice economist and mathematician: whilst he managed the family's jewellery business, he would become one of the most influential economists of the century. Not bad for someone who had chosen economics for the simplicity and rapidity of the course.

In a youthful manifesto written as an examination report, Ragnar claimed that 'Man must not be afraid of what seems impossible to do. History has shown that human beings possess a wonderful gift of being able to obey the saying of Aristotle: "Measure the unmeasurable"' (quoted in Andvig and Thonstad, 1998: 6). More than fifty years later, Frisch recapitulated this Aristotelian assertion in his Nobel lecture: 'deep in human nature there is an almost irresistible tendency to concentrate physical and mental energy on attempts at solving problems that seem to be unsolvable' (Frisch, 1970b: 214). Solving what seems to be unsolvable, not being afraid of what seems impossible: measuring the unmeasurable – that could have been Frisch's motto in life.

Measuring the unmeasurable

In the early 1920s, Oslo was a pleasant place to be: the foundations of the economics course were just being laid and there was not too much competition for its leadership, the city was close to Sweden where important economic research was being conducted and Russian mathematicians were also within contactable range. But it was not an important research centre: from 1811 until 1930, only six or seven doctoral dissertations were approved in economics and one in statistics – that written by Frisch himself (Bjerkholt, 2005: 493). Ragnar wisely decided to seek better qualifications and, from spring 1921 until 1923, he moved to Paris in order to study mathematics and prepare his dissertation. He then travelled until the spring of 1924 in order to discover more about the state of the art in economics: Britain, Germany and Italy were his next destinations.

The very first paper that Frisch ever wrote was on numerical computation (1923), a subject he remained fascinated about throughout his life. During that period, he was also confident enough to prepare other papers on different topics related to mathematics and statistics. His main themes of interest were time series analysis, the measurement of the marginal utility of income and mathematical studies aimed at different applications.[5] His first paper on economics, 'Sur un Problème d'Economie Pure', was published in 1926 by the Norwegian Mathematical Association of Oslo University and includes the inaugural reference to the concept of 'Econometrics':

> Econometrics has as its aim to subject abstract laws of theoretical political economy or 'pure' economics to experimental and numerical verification, and thus to turn pure economics, as far as is possible, into a science in the strict sense of the word.
>
> (Frisch, 1926a: 3)

According to an unpublished manuscript from 1925 or 1926, econometrics should correspond to a very well defined research programme based on the following priorities:

1. To continue the work established by Cournot, Jevons, Walras, Fisher,[6] Pareto and others. That is, to construct a general mathematical analysis of statistical phenomena, without specifying the relevant functions or achieving numerical results.
2. To extend the analysis by using this method to dynamic phenomena.
3. The statistical-econometric task: to specify the relevant functions and to determine the values of the parameters by a rational use of economic statistics, thereby gaining numerical results.

(quoted in Andvig, 1981: 703)

The last task was the one that Frisch embodied in his own doctoral dissertation, implicitly admitting that both the other two would also be complied with. Written in French, as was most of his work in that early period,[7] the thesis dealt with 'Semi-invariants et Moments d'Ordre Supérieure' and was published in Oslo in 1926.[8] The introductory pages present an overview of the current state of the art of statistics. According to Frisch, the calculation of probabilities followed one of two possible paths, being either (i) a rational, a priori form of statistics, or (ii) a stochastic, a posteriori form of statistics, as Bernoulli and then, later on, Bortkiewicz suggested. The author noticed that the second form was beginning to emerge: 'Since some years, this part of the probability computation knew such development that it almost constitutes a new science, intermediary between mathematics and statistics' (Frisch, 1926b: 1).

But that development was not the relevant one for Frisch, since his own point of interest lay elsewhere: the essential problem, he argued, was the determination of the laws of distribution followed by a concrete series describing a process. This was, of course, another way to approach the second task of his programme, the analysis of dynamic phenomena. Taking a series, 'we first try to determine the most likely type of scheme from which the series is created. Then we try to determine the "presumptive" value of the parameters defining the scheme' (ibid.).[9] Frisch admitted to being under the spell of Tschuprow's 1924 lectures at Oslo University, but it is quite clear that the preparation of his dissertation was well underway when he first heard the mathematician speak, and he took care to emphasise that new theories and innovative approaches were necessary for complying with his own research programme. This was Frisch's aim in life: to measure the unmeasurable.

The dissertation deals with this problem: in analytical terms, how to interpret the results from empirical observation – or how to describe the process generating the data. This was what Frisch referred to as the inversion problem, as is usual in physics:

> The inversion problem: how to go back from an empirical distribution to the scheme originating this observed distribution, it is a very different problem. In order to deal deeply with it, we cannot avoid discussing philosophical questions, in particular questions related to the theory of knowledge. We believe that the critical interpretation of the functioning and methods of statistics did not follow the technical development and extension of the field of application of our discipline both in the domain of social sciences and in that of natural sciences. This interpretation quite often suffered from the refusal of statisticians and mathematicians to discuss philosophical questions in order to limit themselves to discussing exclusively technical questions.
>
> (ibid.: 101, or 86 in the published version)

This is an accurate description of what Frisch endeavoured to do from the first moment of his entry into the world of economics in 1920 until the outbreak of the Second World War.

Figure 1.1 Portrait of Ragnar Anton Kittil Frisch sitting by his desk. Taken in 1968 (source: Frisch Archive, University of Oslo).

In 1925, Frisch was appointed *universitetsstipendiat* (junior assistant professor) of economics and defended his dissertation the following year. In 1928, he was nominated docent (associate professor) and, following the creation of a new chair by a special government decision, effective from 1 July 1931, full professor.[10] Economics had been part of the faculty of law since the creation of the course and it would remain so until 1963; from 1935 onwards, the course was extended to five years. At the same time, the Økonomisk Institutt (Institute of Economics) was created; although installed on the University premises, the Institute was autonomous. It became a centre for research into national accounting and other topics, always headed by Frisch, the research director and *alma mater*.

From the beginning, the Institute received a yearly grant of $5,000 from the Rockefeller Foundation and the guarantee of a further 5,000 if some local money was added to the sum. Yet, this interest quickly waned, since the Oslo Institute was mostly interested in 'highly abstract mathematical theory', as John Van Sickle, from the Rockefeller office in New York, put it. The office consulted other economists in order to get their assessment of what was going on in Oslo. Hayek, for one, stated that Frisch was 'more of a mathematician than an economist and is not convinced of the soundness of his economics',[11] but could eventually 'develop techniques that in another generation might prove highly useful' (Bjerkholt, 2005: 523). In short, he was neither understood nor admired, but clearly not disregarded by that generation of economists for whom mathematics was a conundrum.

Looking beyond these borders, Frisch established his leadership of the new programme of mathematical economics. In 1947, just after the war ended, four professors taught the economics course. Frisch was the most influential among them.

Crossing the ocean

As soon as he received the faculty appointment, Frisch decided to establish new working relationships with other economists and to look for kindred spirits, consequently crossing the ocean in 1927 and staying in the US until 1928, again with the convenient support of the Rockefeller Foundation.

Ragnar Frisch disembarked into the midst of a small although effervescent and expanding milieu of social scientists. He already knew, either by name or in person, some of the most distinguished European economists, but only a few shared his ambition of creating a new breed of economics, which he defined as econometrics, the science of measuring the unmeasurable. Never afraid of the impossible, he addressed the most prominent of the economists and immediately received their understanding and cooperation: Wesley Mitchell and the Rockefeller Foundation kindly offered to distribute a manuscript in which Frisch summarised his views on how to measure business cycles.

The paper was prepared for Frisch's lectures and was highly critical of the dominant methods defined by Warren Persons, especially since the young economist rejected the assumption of the constant period and shape of cycles (see Chapter 4).[12] Frisch launched a vigorous attack against the uncritical use of regression analysis, considered Mitchell's periodogram too mechanical and suggested a geometric framework to study collinearity. By that time, he was already aware of the dangers of spurious regression and the illusory effects of averaging over time, as established by Yule and Slutsky in their 1927 papers.

The same month in which he finished that text, December, Frisch presented another paper at the joint meeting of the American Economic Association and the American Statistical Association in Washington, at a round table on the 'Present Status and Future Prospects of Quantitative Economics'. The paper, which has no title, still survives in the Oslo University Archive, and is constructed as an argument for the development of quantitative economics, which should include 'that part of economic theory which is concerned with *the logic of our quantitative notions*', as opposed to simple economic statistics. In other words, Frisch joined forces with those theoretical economists who had embraced the neoclassical approach, the likes of Irving Fisher and John Bates Clark, the introducers of that vision of economics into the US, and his acquaintance François Divisia, a specialist in monetary theory: 'For lack of a better term we might call this part of economic theory the axiomatic part of quantitative economics or simply axiomatic economics' (Frisch, 1927a: 2). Declaring his allegiance to axiomatics, Frisch placed himself at the centre of the mainstream, fighting for an operative analogy with the empirically oriented sciences, physics above all. But this was not enough for him, since most of the cultivators of this very canon still came from a literary and non-mathematical tradition in economics: Frisch provided guidance for the econometric generation in imposing their new method, although he followed a very peculiar version of axiomatics, as argued below.

Immediately after the meeting and eager to obtain the widest possible dissemination of his ideas, Frisch submitted the paper to the NBER (National Bureau of Economic Research) for publication. His argument in favour of axiomatic economics as opposed to, or as an explanation for, the results of empirical economics was alien to the NBER and the institutionalist tradition. At that time – and for many years afterwards – Mitchell and his collaborators were engaged in rather successful research into the measurement and explanation of business cycles, and either tended to ignore or were critical of mainstream equilibrium concepts. Some years later, these differences would ignite a fierce debate, but in the late 1920s they did not prevent Mitchell from providing his young colleague with a list of addresses of relevant economists and encouraging him to distribute his paper on business cycles, although the NBER could not find the space to publish his methodological remarks. Consequently, Frederick Mills[13] wrote to Frisch announcing, after consultation with Schultz and Burns, the rejection of his round table paper for reasons of space.[14]

Frisch did not give up and remained busy looking for other companions who could share his ambition and faith. The project for the creation of a new association grew from contacts and discussions with very different people, including

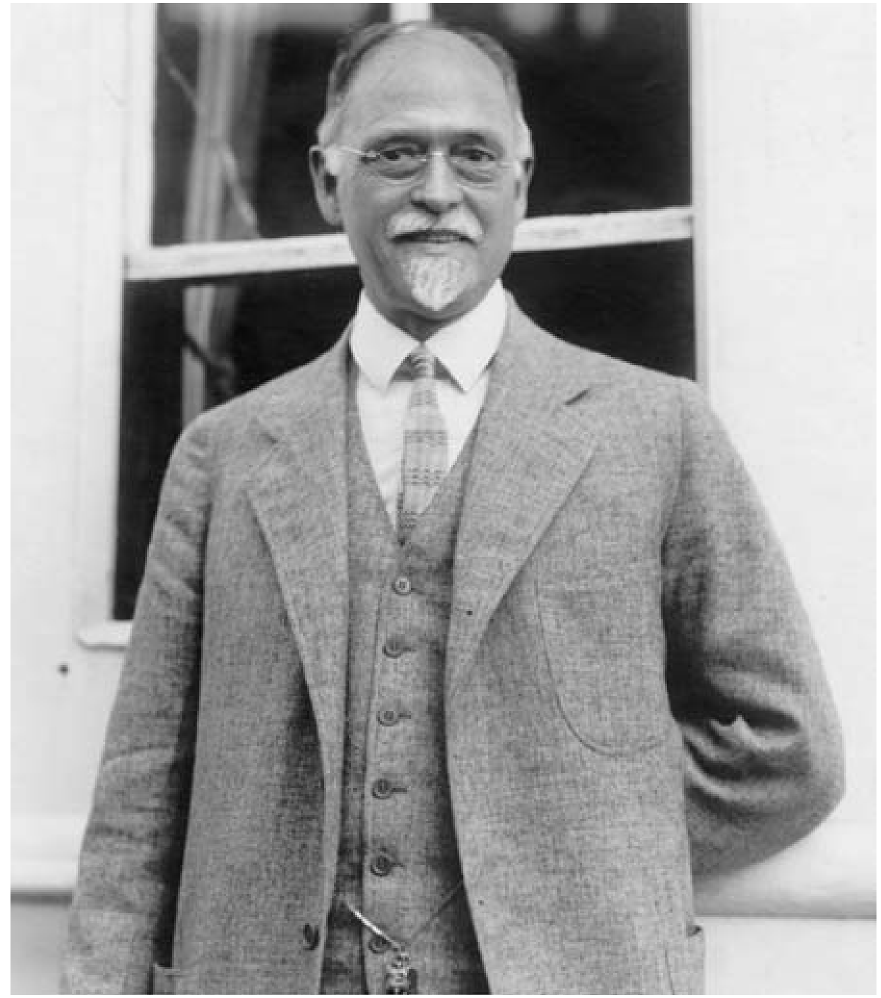

Figure 1.2 Irving Fisher, the first president of the Econometric Society (source: Manuscripts and Archives, Yale University Library).

those who would later split into diverse schools of thought and method. After discussions with Charles Roos, a meeting was set for the next year, at the Colonial Club in Harvard: on 29 February 1928, Frisch met Haberler and Schumpeter in order to draw up a list of 'econometric people'. They found seventy-seven names and proceeded to contact as many as they could, inviting them to join the new econometric movement. The Colonial Club meeting initiated a round of letters and contacts all over the world in a search for like-minded thinkers, and led eventually to the creation of the Econometric Society. At the very end of December 1930, a meeting was held at the Statler Hotel in Cleveland, Ohio, at which the new Society was founded. That story is the main theme of Chapter 2 and covers most of Frisch's activity during the 1930s, the period in which he devoted most of his time to the consolidation of the newborn econometric movement.

In 1930, Ragnar returned to the US to spend a year and half there, first at Yale, at the invitation of Irving Fisher, and then at Minnesota University.[15] The students were engaged in strenuous numerical exercises designed to check the methods of cycle identification that Frisch was devising, namely in constructed series with superimposed noise. The first sketches of the mechanical impulse-propagation interpretation were put to the test and were very soon presented in public, at a conference in Stockholm in June 1931.

In 1931 (published 1932), Frisch delivered the inaugural lecture for a chair that had been created for him at Oslo University. The title was itself a programme: 'New Orientation of Economic Theory. Economics as an Experimental Natural Science'. The lecture presented a vast overview of economics, evaluating the neoclassical revolution and the emergence of the 'subjective side of valuation activity (1870–1890)', including the Austrians, Walras and Jevons. According to Frisch, as neither classical nor neoclassical theories were suitable for statistical checks, the price to be paid was the emergence of different anti-theoretical schools: the German historicists (Schmoller) and the institutionalists (Mitchell), 'the fundamental starting point of both these schools is the same, namely an emphasising that the economic laws are strictly bound to time and place', although the institutionalists engaged in 'exact statistical investigation', related to 'theoretical lines of thought', whereas the German historicists remained anti-theoretical (Frisch, 1932a: 4).

As a consequence and according to Frisch, the necessary transformation of economics into an experimental science had been delayed, but it was time to enter upon a new phase. Some months after the foundation of the Society, Frisch made the argument for econometrics in Norway:

> One of the most important sides of the development of economics in an experimental direction has been the quantification of the economic conceptions, that is to say, the effort to make the conceptions measurable.
>
> [. . .] This is most plainly brought out in thinking of the final object of economic theory, namely, to elucidate the interactive relation between the various factors and to do so in such a manner as to provide the basis on which to determine what practical measures are best suited to promote definite economic social objectives.
>
> (ibid.: 6)

Referring to the example of Hilbert in modern geometry, Frisch endorsed axiomatics as the disciplinary structure for theoretically oriented statistics. This became the common approach for econometricians.

In the autumn, the first European conference of the Econometric Society met at Lausanne, in September 1931 (see Chapter 2). Unstoppable, Frisch travelled across Europe to present the new idea; in 1933, he delivered a series of lectures at the Institut Henri Poincaré, part of Paris University, summarising the new approach and constituting the first ever series of lectures to be named 'econometric' (Chapter 9); in 1934, he lectured at the LSE, in London. In the meantime, he corresponded with almost everybody in economics. He was the decisive man behind the creation of the journal, *Econometrica*, and an important voice in the construction of the Cowles Commission, as well as in the leadership of the Society itself – internal politics, the choice of presidents, decisions regarding the election of Fellows, invitations to new members, the organisation of conferences, editorial policy decisions in relation to *Econometrica*, the dissemination of ideas, the promotion of debates, all this could not proceed without Frisch's approval (see Chapter 2). This impressive rhythm of work continued for the whole decade, centred on the same topics and aims.

Tinbergen, a privileged witness during that period, aptly considered his close friend Ragnar as one of the 'founding fathers' of the Econometric Society and, to say the least, 'for the first decade of its existence, its recognized leader in Europe' (preface to Frisch, 1976: vii), and the 'soul of the group', the 'inspiring leader for decades' (Tinbergen, 1974: 4). Much later, Tinbergen would still insist: 'Our [ES] European meetings were very pleasant because there were only about thirty people participating, so you could actually have profound discussions. Frisch was the leading man there; he was automatically recognized as such' (Tinbergen, 1987: 124). Cowles and Nelson asked Frisch to prepare a book, *Introduction to the Statistical Theory of Econometrics*: 'If Frisch were to edit a book which was a definitive symposium on all the phases of econometrics, this would undoubtedly be a more authoritative exposition than anything we could produce at present' (memorandum sent by Cowles to Frisch, 31 March 1933). This appreciation was confirmed by other colleagues, although Schultz, who toured Europe from summer 1933 to summer 1934 and attended the Econometric conference that year, wrote a somewhat critical appreciation in his diary: 'Frisch has a wonderful command of technique and almost inexhaustible energy. Would that he had better statistical and economic judgement' (entry 20 October 1933, Yale Archive). The younger colleagues were impressed as well: 'Ragnar Frisch dominated analytical economics from the early 1930s founding of the Econometric Society to his wartime internment in a Nazi concentration camp' (Samuelson, 1974: 7).

Indeed, this work was brutally halted by the outbreak of the world war, which prevented communications and travel across the ocean and separated the European and North-American econometric communities. In 1940, Norway was occupied by German troops and normal university life was disturbed as a consequence. The Rector was imprisoned in 1941, although normal teaching went on until 1943, when the faculties were closed. By that time, Professor Ragnar Frisch was the Dean of the Faculty of Law (1942–4), and bore special responsibility for its functioning. This, at least, was the opinion of the Nazi authorities, who decided to arrest the Dean: Ragnar was held at Bretvvedt from 17 October until 22 November 1943, then transferred to Berg on 8 December and finally to the camp of Grini, outside Oslo, where he re-encountered many other university professors, remaining there from 8 October 1944 until the end of the German occupation (May 1945). After more than one and a half years in prison, the war ended for Ragnar Frisch and peace and freedom were brought to Norway.

Yet, there was no return to the past and nothing would ever be the same again: Frisch still retained the nominal editorship of *Econometrica* until 1955, but indeed he no longer played a decisive role in the production of the journal, in contrast with his immense efforts and contributions in the pre-war period. In fact, after the war, he actually abandoned the econometric movement and followed it at a distance with growing scepticism and even hostility (see Chapter 11).

Shortly after the end of the war, in 1947, Frisch was appointed chairman of the United Nations Economic and Employment Commission and used this position to promote his vision of economics: the aim of science should be to prevent unemployment and conflict and, consequently, to ensure a rational distribution of resources and wealth. Extended Leontief input–output matrices were then being used as a tool for that sort of rational planning. From 1948, as Haavelmo returned to Norway and took part of the burden of lecturing, Frisch devoted his professional skills to decision models.

Ragnar, a devout Lutheran Christian and by then a supporter of the Labour Party, had always been particularly upset by unemployment and war, the scourges of the 1920s and 1930s. Within this context, his concept of econometrics flowed directly from his commitment to social justice: rigorous economic thinking, modelling and estimation were supposed to be the only adequate tools for introducing those reforms needed to prevent new wars and fresh waves of unemployment and despair.

Although this view had been quite widespread in the previous decades, after the end of the Second World War its influence waned and finally it ceased to influence the core of the community of economists. Among econometricians, this alternative vision of the aim of mathematical economics became quite isolated: Frisch and Tinbergen, who roughly followed the same critical movement, as well as some Norwegian collaborators, were practically the sole survivors remaining faithful to the earlier conception. This isolation seemed to be indifferent to Frisch, who applied theory to practice and was active in the establishment of economic planning both in India and in Egypt. His travels to India were limited to 1954, but his work in the United Arab Republic, Egypt, was far more extensive: he travelled there in 1957–8, 1958–9, 1959–60 and later in the 1960s.[16] This work was absolutely at odds with mainstream econometrics, and both Frisch and the tenants of the Econometric Society knew it.

As a consequence, some fairly major skirmishes affected Frisch's relationship with the Society: at a seminar in the Vatican (1963), at the first World Congress of Econometrics (Rome, 1965), Frisch was 'rather outspoken, so much that some of the audience may perhaps have found it a bit embarrassing', about the current work of most of his colleagues (Frisch, 1970c: 152; see Chapter 11). Yet he considered it was his duty to make his opinions clear and remained unrepentant years later: 'However, at that juncture of econometric development, I believed I could render a better service to the econometric fraternity by being

critical and outspoken than by sugar-coating the pill. I still hold that view today' (ibid.).

In this sense, Frisch was coherent throughout his life. The editorial of the first issue of *Econometrica* stated that 'the policy [of the journal] will be as heartily to denounce fertile playing with mathematical symbols in economics as to encourage their constructive use' (Frisch, 1933c: 3). For the editor, that meant there was a social responsibility obliging econometricians to make full use of their knowledge. Some decades afterwards, it still meant 'a social and scientific responsibility of high order in the world of today' and

> I would like to add that the time has now come when mathematics and statistics may be and should be applied ever more intensively in economics, thus building up econometrics as a respectable science. But I must also, and most emphatically, add the proviso that we must work for genuine econometrics – not for playometrics.
>
> (Frisch, 1970b: 165–6)

Constructing, not playing, that was the mission of econometrics – Frisch was almost alone in this critique of his colleagues.

After the Second World War, Frisch did not write any more on statistics or econometric methods and dynamic programming, and economic planning became his priority: 'Shortly after 1930 and perhaps even earlier, Frisch had come to consider economic planning, guided by scientific insight, as an appropriate tool to counter the failures of the economic system, and, hence, as one of the overall aims of economic research' (Bjerkholt, 1995: xxiii). Although his methods were ignored in Norway and just used incidentally in other cases, Frisch had an immense faith in his own commitment to economics as a tool for preventing unemployment and unrest. This legendary enthusiasm marked the profession and the birth of econometrics.

Isolated as he was and not afraid of the impossible, Frisch was still the respected founding father of econometrics, and because of this he was granted the first Nobel Prize to be awarded in the field of economics, in 1969, together with Tinbergen. Since he suffered from a broken leg at that time, he only received the prize on 17 June 1970, at a ceremony in which he delivered a speech remembering his whole career and insisting on his own alternative for econometrics – a defeated view, at odds with what had been going on in the field for at least thirty years by that time, but still a proudly stated vision of his science, the coherence of a lifetime.

Frisch died in 1973, aged seventy-eight.

Memorabilia

Ragnar Frisch produced an immense number of books, papers and written lectures and memoranda[17] throughout his life, and was famous for his eagerness to explore new paths in science – he was most of all a devoted researcher, with a productivity matched by very few others.[18] The participants at the first econometric meetings recall how Ragnar was able to keep discussions going for hours, take up new challenges, prepare overnight new papers on a debated topic and present them the next morning (Tinbergen, 1974: 3). His passion for rigour and clarity was contagious, as was his enthusiasm. As Frisch recalls in his Nobel lecture, the 'Lausanne people' – the econometric people assembled at the very first conference of the Society – were fully engaged in non-stop work:

> We, the Lausanne people, were indeed so enthusiastic all of us about the new venture, and so eager to give and take, that we hardly had time to eat when we sat together at lunch or at dinner with all our notes floating around on the table to the despair of the waiters.
>
> (Frisch, 1970c: 152)

Haavelmo refers to one occasion on which Ragnar worked for fifty-six hours without a break on a mathematical problem (Haavelmo, 1974: 147). This workload contaminated the Institute and every student was mobilised: in 1933, Frisch had twenty to twenty-five students working in two shifts, 8am–3pm and 3pm–8pm, in order to perform the necessary computations for solving a problem.[19] The computations involved in the project set up in the area of 'Circulation Planning', his 1934 paper on a barter economy, required, for a model with just fifty variables, approximately 600 weeks of work (two weeks if the services of 300 'computors' were enlisted), which could eventually be reduced if 'special labor saving devices' were invented and made available (Frisch, 1934a: 320f.). His research partners frequently complained about the immense burden of work imposed by Frisch's requirements: Cowles, for instance, protested that the 'computation of coefficients for all possible combinations of twelve variables (including subsets)' would require 13,000 hours of work,[20] although this did not move his correspondent.

Consequently, Frisch was quite notorious among his colleagues for his competence with numerical computation and his knowledge of the appropriate machines: for instance, Marschak consulted him on the choice of calculation machines to be bought for his own centre.[21] During his last decades of work, Frisch followed and used the first generations of computers as much as he could (Bjerkholt, 1995: xxxviii).

This devoted work always met his own demanding criteria. Kenneth Arrow, a younger colleague who met him at the first econometric activities conference in the US, witnesses how he tried to avoid 'sterile Byzantinism' and to prevent the danger of 'valuing mathematical technique over economically meaningful results' (Arrow, 1960: 175): to measure the unmeasurable but surely to measure the existing world in order to act as an economist. This was a lifelong commitment of Frisch's, which was etched in marble in the first editorial of *Econometrica* (1933), where he railed against 'futile playing with mathematical symbols'. The application of the vast programme that Frisch had announced since his younger days – to continue the work of Cournot and Walras, to extend economic

analysis to dynamic phenomena and to create and use adequate econometric tools – required suitable modelling skills and an immense amount of work, challenging the limits of the available mathematical formalism.

Frisch was himself, above all, a model-builder, a problem-solver and not a theoretical economist: this explains the ambiguous relationship he had with neoclassical economics, which will be discussed later on in this book. Concerned as he was with the risk of faulty reasoning and with the logical coherence of models, Frisch paid less attention to the use of models and their application to data, and frequently abandoned a model when it was completed as a solution to a problem, notwithstanding its relevance for empirical research. Together with his devotion to hard work, this explains the immense field he covered, always generating very provocative and innovative models and solutions, but it also explains why so many of his proposed methods and models did not prevail.

There are two main reasons for this failure. The first is that Frisch did not generate a school around his own work: although most of his efforts were addressed towards the creation of a new institution, and indeed the Econometric Society was very successful, he did not endeavour to form an Oslo school. Furthermore, when he moved away from what was becoming the dominant agenda of econometrics, he rarely voiced his objections and did not conduct any campaign in relation to that choice. Shortly after the war, Frisch restricted his work to Oslo and, although continuing to follow the developments in world economics, the sole international relations he cultivated with enthusiasm were those he enjoyed with his rare companions in planning – Tinbergen or, for a brief period, Joan Robinson – and those providing him with opportunities to apply his thoughts to Egypt and India. He was internationally isolated and most of his work was simply ignored in mainstream economics.

But Oslo could not provide an alternative environment to compensate for this isolation, since the faculty was too small and too eccentric, particularly after the shift of the centre of gravity of economics to the US during and after the war. It must be added that Frisch impressed a number of his assistants with his thorough knowledge and the way in which he struggled against difficulties, but not his students: he was a researcher, not a popular professor. For Frisch, faculty lectures were not very important, being considered more as a part of the research effort, and his students, as well as his assistants, suffered from that option:

> Frisch was not always well prepared for his lectures. At times the students were more like observers in his study than listeners to a formal lecture. But in return they got the fascination and, not the least, the inspiration of watching the genius at work. His research assistants might have to wait until the end of office hours before he turned up. Then he sometimes kept them busy in meetings until late at night. When they presented him a draft paper, he might return it full of comments on the first couple of pages, without having read the rest. [. . .] His professional enthusiasm was pervasive.
>
> (Bjerve, 1998: 549)

Thonstadt, who was one of his students and later his assistant, bears witness to the fact that the lectures were difficult and few students used to attend: a lecture could last eight hours with just a short break (Andvig and Thonstadt, 1998: 21–2).

Nevertheless, some of the most promising economists were attracted to Oslo: this was the case with three Dutch academics, Jan Tinbergen, Tjalling Koopmans, both physicists turned economists, and Pieter de Wolff, in the 1930s, and, just after the war, with Lawrence Klein. Yet, only with Tinbergen did Frisch engage in a deep and long-term cooperation.[22] Frisch did not even seek to construct a network for imposing or proposing his own views on econometrics.

There is also a second important reason why so many of his models and techniques did not prevail. Frisch was not a theoretical economist and felt free to propose for each problem the solution that his intuition counselled, even when it was at odds with the dominant views. It is also true that the period of his work corresponds to the emergence – but not yet the stabilisation – of the canon in neoclassical economics, and Frisch was sufficiently eclectic not to feel obliged to follow its prescriptions to the letter. During the 1920s, he set himself the programme of completing what Cournot and Walras had begun and yet he looked for a dynamic representation of a moving economy; during the 1930s, his main collaborators in the econometric enterprise were Irving Fisher and Joseph Schumpeter, and yet he closely followed Keynes's preparation of the *General Theory*, only to be disappointed by his timid view on public regulation and employment policies.

For most of the time, Frisch placed himself outside the canon and his work could not be recognised by mainstream economists. Some, if not most, could not even understand what he was talking about: his 1933 paper on cycles, developing a model based on a mixed difference and differential equation, was too difficult and its computation demanded an extremely demanding simulation. Although it gained respect and was considered to lay the foundations for a new theory of cycles, providing a model for future models – and indeed it did – the paper was read by many, but understood only through its rhetorical tools, the rocking-horse and pendulum metaphors explaining impulse and propagation in cycles (see Chapter 6). This was certainly the most successful model developed by Frisch.

In other cases, his work was quite simply opposed to the dominant intellectual strategy and was unable to change its course: for instance, Frisch measured the variation of the marginal utility of goods, using a cardinal utility function based on the assumption that individuals are able to rank changes in their consumption preferences, following Pareto and Fisher, a concept that was precisely denied by most neoclassical economists (Strøm, 1998: 165). Frisch tried to develop this method of measuring variations in marginal utility with Fisher in 1930–1, when at Yale, but did not achieve any publishable results. He did not give up, since he believed that it was otherwise impossible to estimate autonomous relations and causal processes: the correct statistical measurement should be consequently based on interviews in order to estimate the preference

function. But this approach was not popular and, furthermore, when Samuelson published his 1947 dissertation, a severe blow was dealt against cardinal measurement, which soon fell out of fashion. Only some decades later was there a recovery of the cardinal approach with the reconsideration of the von Neumann–Morgenstern utility functions.

In general, Frisch did not trust anything other than his own intuition, and fought for solutions and techniques that he believed in, notwithstanding the scepticism of his colleagues. His work was highly original and opened up new ground for econometrics: this was the case for instance with the use of confluence analysis for the estimation of economic relations between stochastic variables, which constituted the first generally usable tool for econometricians. According to Bjerkholt, this was indeed his central contribution: 'Frisch's main contribution to statistics and econometrics was not his time series analysis, but his development of tools for determining the interrelations between stochastic variables, in particular his "confluence analysis" which developed from ideas nurtured in the 1920s' (Bjerkholt, 1995: xxxviii). Although his approach to 'errors in variables' remained out of fashion for a long time (see Chapter 8) and Frisch was not able to extend it any further, he always believed it was a promising method.

There is no doubt that several methods and models presented by Frisch did not prevail. Yet there is perhaps one rare exception, that of the model of impulse and propagation for cycles, which constituted the *vade mecum* of the main models of cycles for generations – and still does. As a whole, his work produced several dead ends: his statistical techniques were superseded by the adoption of the probabilistic approach, which he could not fully accept; his view of econometrics as a tool for planning was vanquished; his mistrust of the simultaneous equations approach that was the expression of general equilibrium put him at odds with most of the theoretical work; and he flirted with neoclassical economics, but looked with eagerness to the announced Keynesian revolution, which finally showed him to be at fault. Throughout his life, Frisch either initiated or transformed a number of important fields in economics: production theory, time series analysis, cardinal utility measurement, business cycle and dynamic modelling, econometric methodology and procedures, national accounts, planning and optimal programming. Last but not least, he constructed many of the institutions that came to dominate economics in the second half of the century.

In most of these intellectual challenges, Frisch was defeated and, yet, he paradoxically prevailed in the most difficult one of all, that of revolutionising the landscape of economics. This is the Frischian heritage: a mode of work and research, a committed scientific spirit, but also, and above all, a humanitarian radicalism – science has a moral purpose, since he vindicated it as political economy, as his classical predecessors defined it. In this mood, his last published contribution was a speech to a conference of Norwegian scientists on the desirable cooperation between politicians and economists. In a very game-theoretical approach, Frisch describes a non-profit maximisation utility function through the example of a concrete decision in which altruistic behaviour and cooperation predominate, the choice of how to share two cakes with his wife:

> Assume that my wife and I have had dinner alone as we usually do. For dessert two cakes have been purchased. They are very different, but both are very fine cakes and expensive – according to our standards. My wife hands me the tray and suggests I help myself. What shall I do? By looking up my own total utility function, I find that I have a strong preference for one of the two cakes. I will assert that this introspective observation is *completely irrelevant* for the choice problem I face. The really relevant problem is: which one of the two cakes does my wife prefer? If I knew that, the case would be easy. I would say 'yes, please' and take the *other* cake, the one that is her second priority. But here a problem of *reliable* data emerges. If I know exactly what she prefers, the case is resolved, but what if I am in doubt about that? The problem cannot be solved by asking her: 'Which do you prefer?' She would then say: 'I am completely indifferent, take which one you prefer'. Neither is the case resolved by saying: 'You help yourself first', because the same problem will arise for her. Hence, the simplest thing I can do is to utilize earlier experience and make the decision on that basis. In some cases my assessment of her preferences may be so vague and indeterminate that I to some degree must rely on my own total utility, i.e. make some compromise between the two preference scales.
>
> (quoted in Bjerkholt, 1995: xl–xli)

Decision theory, games, ethics, experiments, utility functions, maximisation procedures, so many theoretical economic themes echo in this example. None as important as wisdom. Frisch knew, perhaps better than anyone else did at that time, that wisdom is the only way to measure the unmeasurable.

2 The emergence of social physics

The econometric people are assembled

In 1922, while preparing his own dissertation, Frisch received a copy of Irving Fisher's 1892 thesis. Fisher, who some years later was to play an important role in the 'years of high theory' of the econometric decade, discussed in detail the history of mathematical economics and in particular the contribution of Cournot. The book fascinated Frisch. Cournot and the neoclassical economists who followed him, most of all Walras, believed they could and should integrate economics into the newborn world of pure science, adopting a rigorous language and logic (mathematics) and following a pattern of scientific research that mimicked physics. Fisher certainly shared this goal – and so did Frisch. Some argue that this project, centred on the empirical estimation of marginal utility, was the prime motivation for the young economist and mathematician: 'Ragnar Frisch was drawn into econometrics not so much out of interest in policy or economics reform but a curiosity to test empirically the fundamental postulates of neoclassical utility theory' (Epstein, 1987: 36) – and that was certainly one of his first challenges.

In any case, econometrics became the necessary instrument for an empirical approach, providing the flesh and bone of that pure economics to be. Its development was Ragnar Frisch's lifetime aim, the idea he had actively promoted and campaigned for ever since the early twenties. But this variety of econometrics corresponded to a peculiar concept defined by two characters: it was conceived of as a new genetic code for economics defined or redefined as a rigorous science, and simultaneously as a tool for a useful applied science committed to the solution of the afflictive problems of humankind. When, in 1926, Frisch defined econometrics for the first time, his aim was the transformation of economics into a positivist science: econometrics should transform 'pure economics, as far as possible, in a science in the strict sense of the word' (Frisch, 1926b: 1). Without empirical verification, economics could not be a science in the full sense of the word, Frisch thought, and he devoted all his efforts to the making of that science.

The first steps: promoting the idea

In September 1926, Frisch approached François Divisia with a bold proposal: the creation of an *Association Internationale d'Economie Pure* and a new journal, as a consequence of their previous correspondence having highlighted their convergence of views on the future of economics. Divisia was a highly respected French economist working on monetary theory to whom, earlier that year, Frisch had sent a copy of his dissertation and a letter outlining his views on the future of mathematical economics. In June, Divisia replied, sharing with Frisch his own ideas on these topics:

> First of all, I believe, as you do, that economic studies cannot today be restricted to the vague reasoning that the classical economists have offered, and that the help of mathematics is necessary; I even believe that economic studies must resort to more complicated mathematical notions than those generally used in sciences for which experimentation is possible.[1]

Although Divisia did not feel at ease with the more advanced mathematical methods, he nonetheless believed that they represented the way forward:

> Mathematical economics has very few supporters in France; myself, I don't know much about it; nevertheless, I am to be counted among those who consider that economic phenomena must be studied by methods as precise as those used in the other more advanced sciences.
>
> (ibid.)

This was hardly an encouragement and even less a commitment, but Frisch only wanted not to be opposed. Divisia, like so many economists of that period, was not exactly a neoclassical economist and suspected the methods of Walras and Pareto.[2] Yet, like an even larger number, he was ready to accept the epistemological predominance and guidance of 'pure sciences' and consequently he sympathised with Frisch's move towards a thorough mathematisation of economics in order to create an empirically based science, although he felt himself to be in some danger in those deep waters. Consequently, Frisch rightly interpreted the letter as an invitation to proceed. The letter sent from Frisch to Divisia in September 1926 took up the challenge and assumed that new steps could follow immediately and, moreover, that he would lead the effort:

> I enthusiastically welcome the idea of a list or some other form of communication between mathematical economists of the whole world. Myself, I had thought of creating an association with a journal discussing these questions. [. . .] I know quite a few mathematical economists in different countries, and I consider writing one of these days a letter to each of them in order to get their opinion about the possibility of an '*Association Internationale d'Economie Pure*' and the possibility of a journal. What do you say to an *Econometrica* (the sister of *Biometrika*)?
>
> (Frisch to Divisia, 4 September 1926)

And so he did: on 1 November 1926, Frisch wrote to four colleagues, Ladislau von Bortkiewicz, Charles Jordan, Arthur Bowley and Eugene Slutsky – no one from the US. Slutsky, whom Frisch had already met in Oslo, was the most enthusiastic about the new association (Bjerkholt, 1998: 31–2), although later on he never adhered to it.[3] The same day, Frisch informed Divisia of the initiative of this letter.

In spite of these early efforts, the decisive steps in the creation of the econometric movement were not taken until it became a European–American enterprise: when Frisch arrived in the US, he immediately found a like-minded thinker in Charles Roos, then at Cornell University, and together they prepared a five-page memorandum, which Frisch recapitulated at his Nobel lecture (Frisch, 1970b: 225). The memorandum argued in favour of rigorous quantification and an empirically based science:

> Two important features in the modern economic development are the application of mathematics to abstract economic reasoning [. . .] and the attempt at placing economics on a numerical and experimental basis by an intensive study of economic statistics. Both these developments have a common characteristic: they emphasize the quantitative character of economics. This quantitative movement in our estimation is one of the most promising developments in modern economics.
>
> (October 1927, Frisch–Roos memorandum)

This argument was championed by Frisch two months later, in his presentation to a round table at the joint meeting of the American Economic Association and the American Statistical Association:

> Quantitative economics is something more than economic statistics. There is a quantitative aspect of economics which is rational and in one sense more fundamental than the empirical manipulation of numerical data on economic phenomena; namely, that part of economic theory which is concerned with the logic of our quantitative notions (. . .). We speak of one statistical procedure as giving a better result than another. (. . .) But I cannot get rid of the impression that we engage (. . .) in target shooting without any target to shoot at. The target has to be furnished by axiomatic economics. Clearing the ground in axiomatic economics is a job which will certainly not be accomplished within the first few years to come.
>
> (Frisch, 1927b)

Clearing the ground was indeed a job that would not be accomplished in the years to come, and yet the author was ready to begin, even though his enthusiasm was not widely shared. The responses to the memorandum and the call for a new movement were similar to Divisia's: curiosity and sympathy, but only tentative support. It was not until a couple of well-respected economists adhered to the movement that it became a force: this was the case with Schumpeter and Fisher. Schumpeter, who was by then preparing to leave Germany for his American exile, was twelve years older than Frisch, and Fisher was his elder by twenty-eight years. Both were established and leading economists and the idea was not new for them: Fisher had already unsuccessfully promoted the project of a new association of mathematical economists in 1912 (Darnell and Evans, 1990: xv, fn.). Yet, the next initiative would not fail. In the autumn of 1927, Frisch met Schumpeter for the first time, at Harvard: their friendship and complicity in matters of the Society became a driving force behind the emerging movement. The *American connection* was to be the core of econometrics.

In February of the next year, Frisch continued with his tour in support of econometrics and visited Irving Fisher at Yale, and then Charles Roos once again at Princeton: both would soon form part of the Society's first managerial board. Later that month, on 29 February, Frisch met Schumpeter and Haberler at the Colonial Club in Harvard: the abstract of the conversation, drawn up by Frisch, indicates that they prepared a new 'list of [77] econometric people' and discussed a name for the projected *International Circle for the Promotion of the Econommetric* (sic) *Studies*, suggesting *Eranos Oekonommetrikos*[4] – a scientific corpus under the name of a student club.

The answers from the 'econometric people' were quite prudent. As we saw, Slutsky adhered to the idea but not to the Society. Georges Lutfalla wrote back to Frisch, advising him not to expect crowds at the door: he had had the experience of being unable to find 400 subscriptions to create a journal of mathematical economics in France.[5] Despite following the movement since its incipient days, Divisia remained very prudent: 'I believe the formula "Economic Science" [in the title of the journal] would be too dangerous: it would mean we want to monopolise economic science. It may well be the essence of our thought, but I believe it is still not the moment to announce it.'[6] Norbert Wiener, who attended the foundational meeting, was very pessimistic about the whole enterprise, as Frisch recalled later on (Frisch, 1970a: 164n.). Others thought the same.

But Frisch did not give up: back in Europe, he went to Italy in June and discussed the matter with Corrado Gini, while developing an intense correspondence with many others about the future Society. Returning to the US at the beginning of 1930 as a visiting professor at Yale, Frisch drew up a list of invitees to the foundational meeting of the Econometric Society, which was to be held in December, and sent out a circular letter to twenty-eight people in the name of Roos, Fisher and himself (17 June). The invitees were: Hans Mayer in Austria; Harald Westergaard in Denmark; Umberto Ricci in Egypt; Clément Colson, François Divisia, Jacques Moret and Jacques Rueff in France; Ladislau von Bortkiewicz and Joseph Schumpeter (who would come to Harvard a couple of years later) in Germany; Luigi Amoroso, Corrado Gini, Alfonso de Pietri Tonelli and Gustavo del Vecchio in Italy; Ragnar Frisch in Norway, Gustav Cassel and Bertil Ohlin in Sweden; Wladislaw Zawadzki in Poland; Arthur Bowley, John Maynard Keynes and Arthur Pigou in the UK; Thomas Carver, John Bates Clark, John Maurice Clark, Griffith Evans, Mordekai Ezekiel, Irving Fisher, Henry Moore, Warren Persons, Charles Roos and Henry Schultz in the US; and Eugene Slutsky in Russia. Jordan was no longer on the list, in spite of having been one of the first to be contacted after Divisia. Considering their answers,[7] the promoters of the Society decided to go ahead with the inaugural conference.

The conference met as scheduled on 29 December 1930 at the Statler Hotel in Cleveland, Ohio. The meeting was held under the presidency of Schumpeter. Sixteen men, including some added to the preliminary list of invitations, decided upon the foundation of the Econometric Society: from the US, the meeting was attended by Harold Hotelling, Frederick Mills, William Ogburn, J. Harvey Rogers, Roos, Malcolm Rorty, Henry Schultz, Walter Shewhart, Carl Snyder, Norbert Wiener, Edwin Wilson, and from Europe by Frisch, Oystein Ore (who was then at Yale), Ingvar Wedervang, Karl Menger and Schumpeter. In spite of their heterogeneity, this small number of economists, sociologists and mathematicians, some of them neoclassical, others institutionalists, reunited to lay the foundation of one of the societies that would reshape economics.

The first selection of the 'econometric people'

The conference elected ten men to the first council of the Econometric Society: Fisher, Roos and Wilson, from the USA; and Frisch, Schumpeter, Luigi Amoroso, Ladislau von Bortkiewicz, Arthur Bowley, Divisia and Zawadzki from Europe. Fisher, who was not present at the meeting, was elected president of the Society and Divisia vice-president.

The task this small group was setting itself was immense in three different fields. First, they endeavoured to create a new discipline inside economics: quite originally, the Society was created precisely in order to define its own subject. Second, they wanted to emulate physics and established a constitutional goal to create social physics, 'to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences' – as stated by the Constitution of the Econometric Society, drawn up by a committee composed of Frisch, Mills and Roos.[8] Third, they intended to provide new solutions, rigorous and quantified as they should be, to the traditional economic problems. This was a challenging agenda.

From the very first day that the Society was created, the members of the council understood that this agenda was too demanding, since they just shared some general ideas and not a concrete response to any of these three goals. In fact, not only did each of them pursue their own research agenda with little connection with each other's, but they also had different visions as far as the future of the Society was concerned. In the first year, 1931, differences emerged over the criteria for choosing new members: Roos and Fisher argued for an open society, whereas Schumpeter, Frisch and Bowley preferred a closed centre of excellence. As a consequence of his different view of the nature of the Society, Schumpeter opposed a number of names proposed by Fisher (Bjerkholt, 1998: 39–40). Fisher even complained that mathematics was too emphasised: 'I notice a tendency in the society to stress mathematics and forget economics.'[9]

The disagreement over the criteria for membership was resolved by the statutory definition of two types of members: ordinary members and Fellows. In spite of these discussions, the Society had grown by the next year: after the meeting of the sixteen founders in 1931, 153 new members joined the association, and, fundamentally, some of the most influential economists of the time were among that number. Irving Fisher had drawn up a list of 261 mathematical economists and many of them were approached by the founders of the Econometric Society (ibid.: 31).

The inevitable result was that the problem was consequently translated into the choice of Fellows, and the first years of the Society were indeed dominated by the definition of criteria both for that selection and for the election process itself, which finally took place for the first time in February 1933. It took some months for the election to be held, and some members expressed their anxiety about it: the immediate election of Fellows was instrumental in preventing the discontentment of young members, since:

> with the policies of the Society guided by a group of Fellows comprising outstanding econometricians, to exclude from ordinary membership those of lesser attainment in this field, who are nevertheless interested in furthering the aims of the Society, could only result in impeding the progress of econometrics. The creation of a group of Fellows should meet all the requirements of those who crave an esoteric atmosphere.[10]

Finally, in the first days of 1933, an agreement was reached on the criteria for the election and Fisher formulated the requirements for the choice of a Fellow, as recapitulated by Frisch:

1 The candidate must be an economist acquainted with economic theory.
2 He must have a mathematical foundation.
3 He must have some knowledge of statistics.
4 He must have done some original work.
5 Some of this original work must have been in economic theory.

(Frisch to Fisher, 11 January 1933)

Using these criteria, the votes were cast and consequently twenty-nine Fellows were elected to the Council and notified in the following August: Amoroso, Anderson, Aupetit, Boninsegni, Bowley, Colson, Gini, Haberler, Hotelling, Keynes, del Vecchio, Divisia, Evans, Fisher, Frisch, Kondratiev, Mitchell, Moore, Ricci, Roos, Rueff, Schneider, Schultz, Schumpeter, Tinbergen, Vinci, Wilson, Zawadzki and Zeuthen.[11]

The diverging concepts about the nature of the Society were expressed in the discussion about the appointments of candidates. Frisch wanted to include Tinbergen, 'an absolutely charming personality', but also Vinci, Gini, Weinberger, Kuhne and 'perhaps' Leontief and Marschak.[12] Divisia opposed Aftalion.[13] Fisher favoured E. Cannan ('one of the first to distinguish between a stock and a flow'), Thomas N. Carver ('the only one who has developed certain points in regard to the coordination of distribution'), E. Kemmerer ('has used a little bit of mathematics'), William Ogburn ('familiar with the application of correlation to mathematics'), a description that indicates the fairly unimpressive state of the art of mathematical economics at that time.[14] Schumpeter suggested Volterra and preferred Taussig to Carver (both from Harvard),[15] although Frisch opposed both of these and proposed John Black ('certainly has the *econometric attitude*, even if he does not master much of the mathematical technique').[16] The final list resulted from multiple compromises among these opinions and eventually expressed the prestige and influence of the main candidates: Mitchell received the maximum number of votes possible, fifty-seven, in spite of not being involved in the workings of the Society; Fisher, Frisch, Schumpeter, Divisia and Roos, the founders, received fifty-four, whereas Keynes received fifty-two.

Nevertheless, as the result of the election of Fellows was unsatisfactory for many, a new list was drawn up the same year. Frisch presented just one candidate, Marschak, since he had previously abandoned this proposal in view of a remark made by Divisia: according to Divisia, Marschak would not know a partial derivative,[17] but Frisch rapidly understood that this just was an unfair insinuation. Taking into account other suggestions, a list was put to the vote of the current Fellows, after eliminating some of the possible candidates, among others Hicks, Sraffa, Hayek and Morgenstern. The list included four who were elected (Allen, Bresciani-Turroni, Marschak and Ezekiel), and thirteen who were rejected (Darmois, Pietri-Tonelli, Fanno, Furlan, Hansen, Hawtrey, Leontief, Mills, Giorgio Mortara, Snyder, Otto Weinberger, E.J. Working and Holbrook Working). Consequently, the election as Fellows of two of those who had been present at the inaugural conference of the Society, Mills and Snyder, was rejected for the second time.

The next list of Fellows was only established four years later, in 1937: Cowles, Hicks, Mortara, René Roy and H. Staehle were all elected. In 1938, it was the turn of Lange, Leontief, J.C. Stamp and T.O. Yntema.[18] By the end of the first decade of the Society's existence, forty-two Fellows had represented the Olympus of the 'econometric people'.

This was an immense success: a couple of years after its creation, the Society attracted already some of the most prominent economists and mathematicians: in 1935, Emile Borel, Constantino Bresciani-Turroni, Jacques Hadamard, Friedrich Hayek, William Jaffe, Otto Kuhne, Emil Lederer, Erik Lindhal, Fritz Machlup, James Meade, Ludwig von Mises, Gunnar Myrdal, Lionel Robbins, Arthur Spiethoff, Sven Wicksell and Vito Volterra, were members, among many others. Not many distinguished economists were absent from the econometric gathering.

The success of the enterprise was matched by the uniqueness of the convergence of different approaches and schools in economics, which were involved by this innovative programme: the Econometric Society was born under the project of reuniting all available capabilities in economics, notwithstanding their divergences. Its pluralistic nature is highlighted by the careful choice of the invitations as well as by the composition of the organs: at the foundational conference the Columbia school was conveniently represented and, as the editorship of *Econometrica* was attributed to Frisch, Frederick Mills was also involved as associate editor as a de facto representative of the institutionalists. He was also asked to contribute to the redaction of the constitution of the Society. Mitchell was one of the five members of the Advisory Council of the Cowles Commission, when it was formed. It was because of Mills's other obligations that he resigned from the post of associate editor in 1934, and not because he did not feel comfortable with the editorial choices of the journal for its first year. On the contrary, at least during the first years of the Society, he played an active role advocating the virtues of affiliation to the new movement: as Edmund Day, who worked for the Rockefeller Foundation, hesitantly approached Mills in order to weight the arguments for membership, he was convinced by a battery of reasons, including the certainty that the Society would not cultivate mathematical esoterism or any kind of separatism.[19]

This would soon change. When the Cowles Commission moved to Chicago in 1939 and the influence of Schultz and Yntema – who were at war with the Columbia institutionalists – was affirmed, the days of the happy convergence were over (Mirowski, 1989b).

The econometric edifice

The Society was moving forwards. But it was self-centred in regard to a number of internal quarrels over vague concepts; diplomacy abounded, but no important steps were taken to establish the dominance of mathematical economics, which still remained ill-defined. The fact was that Mitchell was the most popular economist among the selected audience of the econometric people, but, despite being engaged in empirical work like very few others, he could not be taken as the promoter of a mathematically based 'pure' economics, and still less so of a science that aimed at achieving the higher grounds of the positivist realm of social physics: he did not have the appropriate 'econometric attitude'. At the same time, the econometric people were scarcely prepared for a battle for the reconstruction of economics: according to President-elect Irving Fisher, the ability to distinguish between a stock and a flow, the 'use of a little bit of mathematics' and 'familiarity with the application of correlation' were sufficient recommendations for membership.

Indeed, the development of the Society required the creation of two instruments: an intense network of cooperation, emulation and competition, such as that provided by regular conferences, and the publication of a journal. Both instruments were at the centre of the preparatory discussions among the founders of the Society, although one was easier to establish than the other: conferences required enthusiasm, but a journal required financing, and the large endowment of the former could not compensate for the scarcity of the latter. Consequently, the priority was to set up the assemblies of econometricians. The Society organised an intense schedule of regular meetings both in Europe and in the US: each September–October in Europe, whereas the US meetings were held

in December–January and June, frequently in association with other academic meetings.

The first meeting was held in Lausanne, on 22–24 September 1931, and it was conceived of as an evocation of Walras: the econometric people vindicated both 'pure' economics and its necessary mathematical formalism. Divisia, the vice-president, was in charge of drawing up the programme for this conference, but at the last minute could not make it and Frisch replaced him: not only did he organise the programme, but he also gave three of the nineteen papers, plus the opening address (replacing Schumpeter)[20] and the closing address. Akerman, Boninsegni, Darmois, Fanno, Le Corbeiller, Marschak, Rosenstein-Rodan, Roy, Sraffa, Tinbergen, Staehle, Del Vecchio and Weinberger, among others, also presented their work.

The US econometricians met at Washington and New Orleans later the same year.[21] The following European conference was organised in Paris the next year, and Frisch again replaced Divisia, who was supposed to be the organiser of the programme of the conference in his own town. The next conferences were held at Leiden (1933), dealing with business cycles with papers by Tinbergen, Frisch, Marschak, Kalecki, Hicks, Wisniewski and the presence of Divisia, Zeuthen, Lange, Schultz and others (see Chapter 6). The cities of Stresa (1934),

Figure 2.1 Second Econometric Conference in Europe (Paris, 1–4 October, 1932). In the first row: Tinbergen and Bolza may be the first and third from the left to the right. In the second row: Olegario Baños, Marschak, Bowley Colson, Frisch may be the first, third, fourth, fifth and sixth from the left to the right, and Divisia the second from right to the left (source: National Library, Oslo).

The Econometric Society

An International Society for the Advancement of Economic Theory
in its Relation to Statistics and Mathematics

A l'occasion du centenaire de la naissance de

Léon Walras

les membres soussignés de la Société Internationale d'Econométrie tiennent à exprimer leur reconnaissance à l'Université de Lausanne pour le service qu'elle a rendu à la Science, en dotant d'une chaire l'enseignement de la théorie de l'équilibre économique, et en lui imprimant un éclat qui a largement contribué à son rayonnement dans le monde.

Irving Fisher

16 DÉCEMBRE 1934 — PRESIDENT

OTHER OFFICERS

FRANÇOIS DIVISIA · VICE-PRESIDENT — CHARLES F. ROOS · SECRETARY — ALFRED COWLES 3RD · TREASURER

COLORADO COLLEGE · COLORADO SPRINGS · COLORADO · U.S.A.

OTHER FELLOWS

LUIGI AMOROSO, OSKAR N. ANDERSON, ALBERT AUPETIT, PASCAL BONINSEGNI, ARTHUR L. BOWLEY, CLÉMENT COLSON, GUSTAVO DEL VECCHIO, GRIFFITH C. EVANS, RAGNAR FRISCH, CORRADO GINI, GOTTFRIED HABERLER, HAROLD HOTELLING, JOHN MAYNARD KEYNES, WESLEY C. MITCHELL, HENRY L. MOORE, UMBERTO RICCI, JACQUES RUEFF, ERICH SCHNEIDER, HENRY SCHULTZ, JOSEPH A. SCHUMPETER, J. TINBERGEN, FELICE VINCI, EDWIN B. WILSON, WL. ZAWADZKI, F. ZEUTHEN

OTHER MEMBERS

Figure 2.2 Homage of the Econometric Society to Walras, by the centenary of his birth (published in 1935 in *Econometrica* 3(1): 128). It includes a list of members of the Society (copyright: the Economic Society).

Namur (1935, see Chapter 10) and Oxford (1936, see Chapter 7) hosted the next meetings.

Although few econometricians attended, these conferences are landmarks in the history of economics. At Leiden, under the organisation of Tinbergen, both Hicks and Schultz presented papers for the first time, Frisch presented his 'Propagation Problems and Impulse Problems in Dynamic Economics' – discussed by Machlup, Koopmans, Kalecki, Divisia and Schultz – and Kalecki presented the first outline of his cycle model. Ehrenfest, a physicist who had been Tinbergen's supervisor, was supposed to lecture on harmonic oscillations, but died just before the meeting. The next meeting, at Oxford, was described by Frisch as being 'the best so far' and presented the opportunity for Meade, Hicks and Harrod to draw up models based on Keynes's *General Theory*; on that occasion, Neyman and Haavelmo presented papers for the first time at a gathering of econometricians.

In spite of its having been in existence since December 1930 and having elected a president and vice-president, much of the work of the Society – correspondence, preparation of the conferences, day-to-day management – was concentrated in the hands of only a few members of the council, namely Frisch and Roos. Indeed, the choice of the president and vice-president was a matter of diplomacy, defined by the need for equilibrium and rotation between Europe and the US. Irving Fisher, who did not attend the Cleveland foundation conference, expected Schumpeter to become the first president and was chosen *in absentia*, with Divisia as vice-president and Roos as secretary and treasurer. The mandate of this presidency was extended until the end of 1935.

In fact, the prolongation of Fisher's mandate was imposed by a palace conspiracy designed to prevent Divisia from becoming the next president. Indeed, Frisch strongly opposed the candidacy of Divisia, but could not convince Schumpeter to make a move and to present himself as a candidate. Consequently, Frisch favoured keeping Fisher as president for year after year, then insisted with Schumpeter and desperately suggested Amoroso.[22] The consequence of his resistance was the postponement of the election until 1935. In a telegram, Frisch argued against Fisher's retirement as president, and repeated 'Divisia [is] not recommendable'.[23] Later on, he explained his resistance in rather mysterious words: Divisia 'uses many words to express his meanings'.[24]

Although Divisia had been the first economist to be approached by Frisch for the creation of the econometric movement, it is obvious that certain differences had arisen between them, either because of divergences over criteria for the management of the Society or because of differing personal or scientific points of view.[25] Other econometricians shared the rejection of the natural candidacy of Divisia, the current vice-president. In the same mood as Frisch, Roos prepared a tentative slate for the next ten years and sent it to Fisher, Frisch, Keynes and Bowley, excluding Divisia: Bowley and Mitchell should become president and vice-president in 1936.[26]

In spite of Frisch's insistence, Schumpeter, who by then was living and teaching at Harvard in the US, merely accepted the prospect of a presidency in the future,[27] and the case for avoiding Divisia collapsed. Finally, out of respect for the Europe–US rotation, Frisch accepted Divisia as president and Schumpeter as vice-president.[28] Two years later, Frisch would strongly insist again with Schumpeter that he should accept the job.[29] Schumpeter did not become president until 1940.

Afterwards, the presidency was decided on a yearly basis: Divisia was the next president (1936), with Schumpeter as vice-president; then Hotelling took over the presidency and Bowley the vice-presidency (1937), with Cowles – to be introduced in the next section – as the secretary; in 1938 and 1939, Bowley ascended to the presidency and Schumpeter was made vice-president; finally, in 1940, Schumpeter accepted the presidency and Tinbergen was his vice-president. With some irregularities, the operational scheme was to alternate the presidency and the vice-presidency between the representatives of the two continents and to give the presidency to the previous vice-president.

An angel suddenly fell from the sky

Since the early days of the 1920s, when Frisch first tried to convince Divisia of his plans for the creation of a new international association, the proposal of publishing a journal had been constantly evoked. Indeed, it was intensely discussed, mostly in respect of its title. Indeed, a curious feature of this early correspondence is the discussion about the name of the future journal: Frisch favoured *Oekonometrika* (admitting the influence of *Biometrika*), Divisia suggested *Oeconometrika* or *Oeconommetrika* whereas Slutsky's choice was *Economometrika*. Fortunately, the option turned out to be the first choice, the more pedestrian *Econometrica*.[30]

Some years later, with the Society structuring itself around the conferences and the permanent activity of some of the council members, *Econometrica* was badly needed in order to project the new methods, discipline the field, impose the prestige of the Society and its programme and attract new researchers and capacities. Without the journal, the Society could organise the econometric people, but could not reach the 'economist people'.

The journal was a central piece in the project: indeed, since its conception, the association of econometricians had been supposed to be defined both by the organised corpus of membership and attendance at the conferences and by *Econometrica* as the expression of their research. But it was much harder to create the journal, since it required more than just intense work, devotion and imagination – it demanded financing in that disturbed period of the 1930s, right in the midst of the general depression and under the pressure of the imminent outbreak of war. Financing was even harder to attain given the general ignorance of econometrics and the widespread dismissal of its potentiality: even later, when the Society was beginning to impose itself, the treasurer, Roos, noted the difficulties in obtaining funding from public or other sources, since the referees of projects were very sceptical – mathematicians were rather critical and, if asked, Jacob Viner and Carl Snyder could be 'quite unfavourable', whereas Mitchell and Taussig's attitude was unpredictable.[31] The paradox of the situation was obvious: Snyder, Mitchell and Taussig were members of the Society and yet were suspected of not favouring the financing of its projects and activities.

A miracle was needed in order to publish *Econometrica*, and it came in the form of a complete surprise: Alfred Cowles III, the son of a millionaire, the president of an investment counselling firm, Cowles and Co., and a competent statistician interested in stock market predictions, offered to pay $12,000 a year for the journal.[32] 'An angel suddenly fell down from the sky', announced Fisher to Frisch, asking for his opinion since, as the 'original founder', the decision was up to him.[33] Cowles had tried to compute a multiple correlation for twenty-four variables and sought advice from Davis, a mathematician from Indiana University, since he had bought a Hollerith computer from IBM. Davis suggested that he approach the Econometric Society (Christ, 1952: 7–8). Since Cowles had discontinued his forecasting services in 1931, he was totally dedicated to statistical research and embraced the suggestion with great enthusiasm. It was just the beginning of many years of collaboration with the Econometric Society.

Although Frisch's first reaction was to suspect and resist the deal, he decided to travel to Colorado Springs in order to meet Cowles. After one week, he was convinced (ibid.: 9) and wrote back to Schumpeter commending Cowles, 'a very conscientious young man'.[34] *Econometrica*'s first issue appeared in January 1933 and it has been published ever since. Frisch proposed Hotelling as editor, in order to avoid his own appointment, but he could not prevent it from happening: he finally took over editorial duties from 1933 until 1955. Associate editors were also appointed: first Alvin Hansen (replaced, in 1938, by Schumpeter), Frederick Mills (resigned in 1934) and Harold Davis: one economist, one statistician and one mathematician. The function of the assistant editors was to accompany Frisch in most decisions and practical tasks, but managing editors were also appointed to fulfil the practical tasks: William Nelson took the position in Colorado Springs until his early death in 1936 (aged thirty-six) and then Dickson Leavens replaced him at the editorial office until 1948. In 1942, when the war interrupted communications with Europe, Oskar Lange was appointed acting editor (although Frisch preferred Tinbergen, Schumpeter or Hicks, in that order),[35] and Frisch resumed his position in 1946.

At least for the first decade of the journal's existence, Frisch was the sole driving force behind its publication: he set the agenda, corresponded with the authors,[36] asked for articles, was the referee in most cases, discussed the papers and made suggestions, and, finally, decided on publication, changed the notation for coherence and even corrected the galley proofs. He worked immensely hard and it was his efforts that determined the survival and development of the journal; at the same time, this concentration of decision making generated delays, since the papers and proofs had to cross the Atlantic twice before each issue, and, worse still, created new editorial problems.[37] The editor was the journal.

Furthermore, Frisch's ways were absolutely centralist and very self-centred: he abundantly published his own work (seventeen papers plus many notes) but also included references to the journal in footnotes to other papers. In one famous case, Frisch's appreciation of Kalecki's dynamic model appeared prior to the paper itself (April 1935, pp. 225–6, and July 1935, pp. 327–44, respectively). Frisch included personal endnotes to papers by Bowley (6, 1938, pp. 83–4), Mendershausen (ibid., pp. 285–6) and footnotes to Tinbergen (6, 1938, pp. 29–33) and Wald (7, 1939, pp. 319–21), among others. Of course, some of the authors reacted against this practice, even if fewer than might have been expected:[38] Hotelling reacted against Frisch's announcement of the publication of a comment along with his paper, prepared after years of research, writing that 'I do not think it likely that any criticism conceived of within a few days and published immediately is likely to have much force';[39] the editor simply postponed publication of the comment until the following issue.

In other controversial cases, Frisch's editorial choices were also challenged. The publication of a biographical essay by Amoroso on Pareto (*Econometrica* 6, January 1938), which some considered to be apologetic of Pareto's political choices, including that of his accepting honours from Mussolini, gave rise to a chorus of criticism. Jerzy Neyman was particularly virulent,[40] although the editor defended the paper as a mere description of Pareto's work.[41] In order to avoid further controversy, Frisch sent a circular letter to the Fellows, based on a draft by Schumpeter, presenting his explanation for the incident.[42]

But none of these decisions provoked such a loud roar as that of the publication of a paper of his own: 'Circulation Planning' (1934), the longest paper ever published in *Econometrica*, occupied ninety-three pages. The uneasiness felt by the Council members became quite obvious when Fisher reported different critiques of the editorial policy by Gini and Bousquet, and Roos condemned the publication of the paper (as well as of Tinbergen's paper on business cycles, with more than sixty pages). In fact, the discussion over this issue is one of the most enlightening about some of the tensions existing between Frisch and his fellow econometricians.

Roos was the main contender: he criticised the self-promotion of the editor and his previous practice of adding bibliographical notes on his own work, but also the content of the paper itself, since it is:

> not a very vital contribution to economic knowledge. It seems to me especially unfortunate that by far the longest paper so far to appear in *Econometrica*, of a length greater than is customary in any scientific journal, should come from the editor, particularly since, in my opinion, it does not read like a very important piece of research. I do not mean that it should not have been published, but I do mean that a pair of scissors could have been used to advantage. I assume, however, that, for your own protection, you had it refereed and, in that case, evidently the referee did not agree with my judgement. There has been some criticism to the effect that you have failed to recognize the difference between mathematical exercises and contributions to economic knowledge by means of mathematics.
>
> [. . .] Several people have commented on the frequency with which authors' papers have been documented with references to your own work especially when reasons for these references have not been particularly obvious.
>
> [. . .] In this country an editor elected by a scientific society invariably tends to deemphasize his own work. It is generally held that he should be careful to avoid all suspicion of using his position to promote his own scientific reputation.
>
> (Roos to Frisch, 13 February 1935)

In the same vein, Roos also thought that the empirical content of both this and several other papers was open to question:

> If it [the journal] is to be devoted to econometrics, the postulates of the papers should at least be related to economic reality. Perhaps you, as editor, ought to examine papers even more critically in this mind, for if the journal is to be devoted principally to mathematical 'theories', without regard to their relationship to economic reality, there is no limit to its possible size.
>
> (ibid.)

Some days later, Roos played down the importance of the issue of editorial policy but still insisted on priority being given to tested and testable papers rejecting equilibrium theories:

> I think you are doing a good job with *Econometrica* and the only criticism I have is that there is not enough attempt to test theories with factual matter. I, of course, believe that most of the theoretical work that assumes equilibrium is worthless, but that is a personal point of view.[43]

Frisch acknowledged the criticism and conceded 'this is the kind of way a friend writes to a friend', even accepting that 'Circulation Planning' should quite possibly not have been published. But he stood by it: 'If the paper has any merit it is of a technical sort', the rest being dismissed as mere political opinions – the paper argued for a system of direct exchange in order to prevent the inner tendency towards the collapse of the market, a recurrent theme in Frisch (see Chapter 10), a theory that was not, of course, popular among many econometricians.[44] The editor also agreed to try to keep the size of the papers down and to diminish his self-references: 'I am sorry if somebody feels that authors in Econometrica have given too frequent references to my own works. [. . .] I recognise that I, as editor, ought to deemphasize my own work.'[45] In the same 'friendly spirit', Roos replied and stated that Frisch should not look for alternative places for publication, but that at least he should restrict the size of the accepted papers as a general rule.[46] Nevertheless, in spite of this amiable mood, there is evidence that after this episode Roos concluded that Frisch should possibly be replaced and he argued for such renovation some years later.

Since Frisch's mandate as editor expired in 1940, the pressure to substitute him had mounted by that time. Cowles voiced some of the opposition in his correspondence to Schumpeter:

> Charles Roos was in Chicago recently and intimated to me that he was aware of considerable opposition to the re-election of Frisch. The criticism seems to be that the material in *Econometrica* is not of practical interest to industrialists and also that Frisch has made too much use of his position to publicize his own work through articles and footnotes in *Econometrica*. I think there is more basis for the latter criticism than for the former.[47]

But Schumpeter strongly opposed any change and evoked the danger of a schism opposing the Europeans and Americans if Frisch were replaced: 'with the possible exception of Keynes, all the Europeans will, I think, vote for him'.[48] Finally, Cowles accepted this position, but established that provisions should be made for a temporary replacement if the occlusion of peace prevented continuation of the editorial work, which happened to be the case.

After the end of the war, a new solution was looked for. In 1953, a Committee was established to evaluate the editorial policy of *Econometrica* and to make suggestions: Samuelson, Koopmans and Stone published their findings and proposed that more space be made available for empirical research (*Econometrica* 22, 1954, pp. 141–6). The next year, Malinvaud was appointed European co-editor, 'as part of the package for solving the tension between the Chicago office on one side and Frisch and some other European members of the society on the other' (Malinvaud, 1998: 560, fn.). Finally, Frisch's editorship was brought to an end in 1955. At that time, he was appointed chairman of the Editorial Board and then, in 1967, he became the first chairman of the newly created Editorial Advisory Committee, a position he filled until July 1969.

The uniqueness of *Econometrica*

Frisch's activity is indistinguishable from the development of *Econometrica*, at least for the first decade of the journal's existence, and cannot of course be evaluated solely from the point of view of the conflicts it generated. There is, on the other hand, a story of immense success, since *Econometrica* became one of the leading journals in the profession and fully accomplished its role as a pillar for the development of the econometric movement. Looking back, one can only be surprised by the depth and seminal influence of so many papers, by the diverse and far-reaching strategies of publication, by the attempts to motivate young colleagues, and by the strenuous efforts to combine historical memory with the promotion of technical expertise.

Compared to the *Economic Journal (EJ)*, an older and well-established journal, *Econometrica* exhibits some revealing differences during this period (Appendix, Table 2.a.6). The number of general theoretical papers published in both journals is not significantly different, the editorial strategies diverge notably in regard to the

publication of articles about statistical theory and mathematics (irrelevant for *EJ*[49] and quite substantial for *Econometrica*, with approximately twice as many empirical studies). The other relevant differences were the publication of papers on the history of thought (*Econometrica* publishing twice as many as the *EJ*) and the publication of papers on empirical studies (*EJ* publishing three times as many as *Econometrica*). *Econometrica* was definitely more inclined to publish papers on mathematically based theoretical and applied research and on the history of economic thought, and less on empirical applications.

The numbers are telling, but they do not reveal the discussions taking place on editorial strategies, in particular those that decided the shape of the newborn journal. In fact, in spite of its dedication to both mathematics, the powerful formal logic that was at the epicentre of econometrics, and statistical theory, *Econometrica*'s editorial policy was not insensitive to the difficulties of affording technical treatment to abstract topics, and several of its prominent leaders frequently emphasised the importance of there being an empirical counterpart in the choice of the papers. The publication of a difficult paper by Frisch, 'Changing Harmonics', an attempt to substitute or refine traditional spectral analysis (1934), proved that the question was open to discussion: E.B. Wilson, the referee, did not rate the paper very highly and proposed condensing some parts and omitting others, in order to make it more accessible, receiving some support for his suggestion. As Nelson, the managing editor put it:

> The paper will not appeal to economists. It is really a mathematical paper, whereas the economist is interested in the *application* of mathematics to his economic problems; and the paper gives no indication of such application.
>
> [. . .] Since the paper is mathematical, and will be read by mathematicians and not by economists, it can be condensed
>
> [. . .] A journal is not an 'educational' project, and 'educational' material is generally not read by those for whom it is intended. The inclusion of 'educational' material, therefore, is a waste and a mistake.
>
> [. . .] My own résumé of Wilson's attitude is: no economist who is not a mathematician would attempt to work through your paper.
>
> (Nelson to Frisch, 17 October 1933)

Frisch disagreed about both the form and the content,[50] but abided by the suggestion of the referee and agreed not to print the paper. As the editor of the journal, Frisch accepted the suggestion of giving priority to the formal treatment of real data, the 'inversion problem' he had mentioned earlier in his career – indeed, this was all he asked econometrics to be. And this required accessible papers: very shortly afterwards, he invited Georges Bousquet, a young economist (thirty-four years of age at that time), to prepare a paper for *Econometrica* in order to build a bridge between the 'more mathematically oriented men in our group and the broader group of general economists who are interested in the econometric approach but who do not have the time or the background to follow the technicalities of our work'.[51]

On the other hand, the journal was not supposed to be just a repository of difficult mathematical papers. One of its most impressive early features was the editorial concern with the recuperation of history. This highlighted the nature of the movement, which was looking for legitimacy as the heir to the ideas of giants: in its very first issue, *Econometrica* surveyed Cournot and Wicksell; in 1934, it included papers on Von Thunen, Edgeworth, Jevons and Walras; in 1938, the famous paper on Pareto and another on Cournot, then the next year on Schultz and again on Cournot; in 1946, a memorial on Keynes. In looking at the past, *Econometrica* was pointing to the future.

More good news falling from the sky

At the same time as the Econometric Society was discussing the miracle of Cowles's generous offer to finance *Econometrica*, another initiative was also under consideration: the creation of a research centre at Colorado Springs, the Cowles Commission for Economic Research. Indeed, the key to the success of the econometric movement was this virtuous combination between the various workings of the Society, namely its conferences, the publication of the journal and the creation of the Cowles Commission as a permanent research facility.

At first, the Commission was a centre with only a few permanent scientists, working in close connection with the econometric movement and managing both resources and contacts in order to develop useful research projects. The advisory board of the Cowles Commission was later appointed by the Society, in 1937, and its members were Fisher, Bowley, Mitchell, Frisch and Snyder.[52] In a booklet outlining the aims of the Commission in the year that it was created, it was boldly stated that the econometric movement was bringing the triumphant spirit of natural sciences to economics:

> Happily the materials for a fresh approach are at hand. The disclosure of the shortcomings of the older theories has coincided with a steady strengthening of a movement to deal with the problems of economics in a more precise and adequate way. It consists of an attempt at working out a new system of economic theory, more realistic, more precise and wider in scope than the orthodox theory – a new theoretical economic system that is built with the specific purpose of being brought into immediate contact with and thus effectively utilizing the huge mass of economic-statistical data that are now rapidly becoming available. In this way it is hoped to bring into economics some of the constructive and precise spirit – at the same time emphasizing theory and factual measurements – that has come to dominate in the natural sciences, and which has been one of the chief factors of the amazing progress of these sciences in the last generations.
>
> An essential element in this new movement in economics is the application of mathematics in the handling of the more realistic and therefore tremendously complex situations that are considered in the new setting of economic problems. Many of these situations cannot be discussed consistently and safely without the aid of mathematics. This combination of profound economic theory with statistics and mathematics offers a very promising means of real progress.
>
> (Cowles Commission, 1932)

These claims about the future of the 'new theoretical economic system' were thoroughly discussed and the first draft was not accepted, since it was criticised as promoting a far too one-sided vision of the role of mathematics. The published version, after the corrections that Frisch had imposed, was a mirror of the movement's links with both mathematical economics, following the path of physics, and the empirically oriented science in the economic realm, social physics. For theoretical and for practical reasons, Frisch could not agree to limiting econometrics to abstract theorising under the requirements of formal beauty:

> There are however certain parts which, as I see it, cannot stand as they are now. I am in particular referring to the exclusive, and indeed rather mechanistic, emphasis that is put on the mathematical aspect of the new movement. The first paper of the new draft, and also other passages, gives the impression that the econometricians, and in particular the Cowles people, say something like this: All you dumb economists have done so far is all bunk, because you have forgotten to press the correct button: marked mathematics. Now just watch and see, we are going to press it and – *click* – you will have the solution ready made.
>
> [. . .] econometrics is not synonymous with the application of mathematics to economics. This is only one aspect of econometrics.
>
> (Frisch to Cowles, 22 August 1932)

Consequently, during its first years, the Cowles Commission used its resources to work intensely on practical projects, namely in business cycle analysis, generating models and developing a practical and empirical approach to mechanical representations by creating prototypes inspired by electrical engineering models and other domains of physics, in order to meet Frisch's demands. At the beginning, the Commission was dominated by the omnipresence of Frisch and technical capacities were placed under the auspices of his own hunches. A collection of booklets was designed to publish those articles that were either too large for inclusion in *Econometrica*[53] or too difficult to be accepted by other institutions.[54] A small resident research staff managed the Commission: for the period 1932–5, it included only Harold Davis, William Nelson and Forrest Danson. In 1936, a Herbert Jones, Edward Chapman, Dickson Leavens and Gerhard Tintner also joined this staff.

The Commission soon set its own course, under the directorship of Charles Roos, who was both able and prepared to oppose Frisch if necessary, as demonstrated in the previous pages over the case of the editorial choices for *Econometrica*. Roos largely influenced the first years of the US econometric movement but abandoned it in 1937 when he accepted a comfortable job at the

Mercer-Allied Corporation in New York. The choice of the new director proved not to be an easy one: Cowles first invited Marschak, but there was no agreement on the wages to be paid.[55] Consequently, Cowles consulted Frisch about alternatives, Bresciani-Turroni or Tinbergen, and also noted the difficulty in securing the services of Lange, since he already had a position at California University and was also a visiting professor at several other universities.[56] Frisch invited Bresciani-Turroni, who rejected the offer,[57] as did Tinbergen.[58] Indeed, no one was seduced by the idea of a transfer to Colorado Springs.

Finally, Lange was selected and given the job in 1937. Meanwhile, as Cowles's father had died, he was forced to move to Chicago: with his attorney Laird Bell, a trustee of the University, Cowles negotiated the inclusion of the Commission in its structure, in spite of the hostility of the economics department. Yntema, at the time a professor at the Business School, was the next director, but apparently was frequently on leave during his 1939–42 tenure. Anyway, as the Commission was now established at a major university and its prestige had grown, the post of director became a highly desirable position: Marschak succeeded Yntema in 1943 and stayed until 1948; then Koopmans took the job from 1948 to 1954.

Marschak was responsible for a major change in direction in the development of the Cowles Commission. He recruited Haavelmo (July 1943) and Koopmans (July 1944) as research associates, as well as Arrow, Klein, Domar, Modigliani, Patinkin and Simon (Hildreth, 1986: 8). This new team drove the Commission towards structural estimation, following the seminar Marschak had previously held at the New School of Social Research in New York, which was attended by Haavelmo, Schumpeter,[59] Leontief, Modigliani, Wald, Koopmans, Samuelson and Arrow from 1940 until his move to Chicago, and developing Schultz's programme. For the first time, the elite of US econometrics was directly involved in the day-to-day research of the Commission. A witness of that period, Lawrence Klein, marvelled at the intensity and diversity of the working of the Commission:

> it was the most unusual group of people there. To think of having Marschak, Koopmans, Haavelmo, Hurwicz, Anderson, Patinkin and eventually Arrow, Herman Rubin, Roy Leipnik and Herman Chernoff, with many visitors like Jan Tinbergen and Ragnar Frisch. It was just a tremendous number of people who were unusually talented, and they all congregated in that one place.
>
> (Klein, 1987: 413)

Yet, this programme for generalised use of probabilistic theory for structural estimation was soon exhausted. In spite of massive efforts and the elaboration of sophisticated techniques, these did not lead to very different estimation results in relation to standard OLS. As a consequence, although since 1932 the motto of the Commission had been 'Science is measurement' (Christ, 1952: 61), structural estimation waned away over the course of the 1940s. Consequently, empirical research in structural estimation and 'econometrics started to become a

secondary interest of the Cowles staff as the 1940s ended' (Epstein, 1987: 110). In fact:

> structural estimation was founded on the belief that Tinbergen's approach to empirical economic could be adapted to yield decisive tests of different economic theories and to design effective policies for changing an economic system. It was an ingenuous extension of standard statistical methods for the analysis of laboratory experiments. The 'endogenous' variables under study were assumed to be generated by an equal number of co-acting 'laws' that operated in the aggregate.
>
> [. . .] The early researchers at the Cowles Commission, as well as Tinbergen and Frisch by the early 1950's came to view these other issues [the methodological problems] as posing fundamental obstacles to the realization of their original goals.
>
> (ibid.: 223–4)

Simultaneously, Frisch's parallel research programme on business cycles was also paralysed: 'By 1939 Frisch's original research program for the Institute was in shambles, as his high-profiled business cycles/time series project had fallen apart' (Bjerkholt, 2005: 520).

Furthermore, the relation with the economics department at Chicago was tense and deteriorated. The department had always been the centre for intense scientific innovation: during its first period, it was dominated by Veblen, Mitchell and J.M. Clark, who had moved to Columbia; then, the duo Frank Knight and Jacob Viner transformed it into the counter-institutionalism headquarter. The Chicago School of the 1920s was a very peculiar mixture of atypical neoclassical economists of Austrian and Marshallian inclination but suspecting the efficiency of laissez-faire, supporting government intervention in recessions but later hostile to the Keynesian movement, attracting mathematically trained economists such as Lange, Schultz and Paul Douglas, who were more of the Lausanne abeyance, but Knight himself did not bet on mathematics. When the Cowles people, with Marschak and Koopmans, came to the University, the tension was unavoidable. Koopmans had replaced Marschak in June 1948: it was time for the Commission to turn to developments in General Equilibrium theory and models, to the discontent of some econometricians. Koopmans generated a 'metamorphosis' of the Cowles Commission, both in structure and in research (Mirowski, 2002: 249). In 1955, the Commission moved to Yale.

As the next chapters will show, these notable differences as to the preferred approach to econometrics had already surfaced in several episodes. In any case, the econometric edifice was already built with the Society, *Econometrica* and the Commission, and how imposing it was: as US science and academia benefited from forced immigration from Europe – Neyman, Morgenstern, Von Neumann, Schumpeter, Haavelmo, Koopmans, Marschak – the professionalisation of economic mathematics and statistics was the achievement of the econometric movement in a matter of a dozen years.

Appendix

Table 2.a.1 List of presidents and vice-presidents of the Econometric Society (1930–40)

	President	*Vice-president*
December 1930	Fisher (USA)	Divisia (France)
1931	Fisher (USA)	Divisia (France)
1932	Fisher (USA)	Divisia (France)
1933	Fisher (USA)	Divisia (France)
1934	Fisher (USA)	Divisia (France)
1935	Fisher (USA)	Divisia (France)
1936	Divisia (France)	Schumpeter (USA)
1937	Hotelling (USA)	Bowley (UK)
1938	Bowley (UK)	Schumpeter (USA)
1939	Bowley (UK)	Schumpeter (USA)
1940	Schumpeter (USA)	Tinbergen (Netherlands)

Table 2.a.2 Presidents of the Econometric Society after 1940

Year	*President*	*Year*	*President*
1941	Joseph Schumpeter (USA)	1974	Don Patinkin (Israel)
1942	Wesley Mitchell (USA)	1975	Zvi Griliches (USA)
1943	Wesley Mitchell (USA)	1976	Hirofumi Uzawa (Japan)
1944	John Maynard Keynes (UK)	1977	Lionel McKenzie (USA)
1945	John Maynard Keynes (UK)	1978	Janos Kornai (Hungary)
1946	Jacob Marschak (USA)	1979	Franklin M. Fisher (USA)
1947	Jan Tinbergen (Netherlands)	1980	John D. Sargan (UK)
1948	Charles Roos (USA)	1981	Marc Nerlove (USA)
1949	Ragnar Frisch (Norway)	1982	James A. Mirrlees (UK)
1950	Tjalling Koopmans (USA)	1983	Herbert Sarf (USA)
1951	R.G.D. Allen (UK)	1984	Amartya Sen (UK)
1952	Paul Samuelson (USA)	1985	Daniel McFadden (USA)
1953	René Roy (France)	1986	Michael Bruno (Israel)
1954	Wassily Leontief (USA)	1987	Dale Jorgenson (USA)
1955	Richard Stone (UK)	1988	Anthony B. Atkinson (UK)
1956	Kenneth Arrow (USA)	1989	Hugo Shonnenschein (USA)
1957	Trygve Haavelmo (Norway)	1990	Jean-Michel Grandmont (France)
1958	James Tobin (USA)	1991	Peter Diamond (USA)
1959	Marcel Boiteux (France)	1992	Jean-Jacques Laffont (France)
1960	Lawrence Klein (USA)	1993	Andreu Mas-Colell (USA)
1961	Henri Theil (Netherlands)	1994	Takashi Negishi (Japan)
1962	Franco Modigliani (USA)	1995	Christopher Sims (USA)
1963	Edmond Malinvaud (France)	1996	Roger Guernerie (France)
1964	Robert Solow (USA)	1997	Robert Lucas (USA)
1965	Michio Morishima (Japan)	1998	Jean Tirole (France)
1966	Herman Wold (Sweden)	1999	Robert B. Wilson (USA)
1967	Hendrik Houthakker (USA)	2000	Elhanan Helpman (Israel)
1968	Frank Hahn (UK)	2001	Avinash Dixit (USA)
1969	Leonid Hurwicz (USA)	2002	Guy Laroque (France)
1970	Jacques Drèze (Belgium)	2003	Eric Maskin (USA)
1971	Gerard Debreu (USA)	2004	Ariel Rubinstein (Israel)
1972	W.M. Gorman (UK)	2005	Thomas J. Sugent (USA)
1973	Roy Radner (USA)	2006	Richard Blundell (UK)

Table 2.a.3 List of Fellows (1933–8)

Year	*Fellows*
1933a	Amoroso, Anderson, Aupetit, Boninsegni, Bowley, Colson, Del Vecchio, Divisia, Evans, Fisher, Frisch, Gini, Haberler, Hotelling, Keynes, Kondratiev, Mitchell, Moore, Ricci, Roos, Rueff, Schneider, Schultz, Schumpeter, Tinbergen, Vinci, Wilson, Zawadzki, Zeuthen
1933b	Allen, Bresciani-Turroni, Ezekiel, Marschak
1937	Cowles, Hicks, Mortara, Roy, Staehle
1938	Lange, Leontief, Stamp, Yntema

Table 2.a.4 Research Directors of the Cowles Commission

Alfred Cowles III 1932–4 (Colorado Springs)
Charles Roos September 1934 to January 1937 (adviser 1937–9)
Harold Davis 1937 (adviser 1937–9)
Oskar Lange 1937–9
Theodore Yntema 1939 to November 1942 (Chicago)
Jacob Marschak 1943–8
Tjalling Koopmans 1948–54
James Tobin 1955–62, 1964–5 (Yale)
Tjalling Koopmans 1961–4, 1965–7
Herbert Scarf 1967–71, 1981–4
William Brainard 1971–3 1976–81
Martin Shubik 1973–6
Alvin Klevorick 1984–96
John Geanokoplos 1996–2005
Philip Haile 2005 to present

Table 2.a.5 Conferences of the Econometric Society

European meetings	*US meetings*
	Cleveland 1930
Lausanne 1931	Washington, DC 1931
Paris 1932	New Orleans, Syracuse, Atlantic City, Cincinnati 1932
Leiden 1933	Chicago, Philadelphia, Boston 1933
Stresa 1934	Berkeley, Chicago, Pittsburgh 1934
Namur 1935	Denver, Indianapolis, Colorado Springs 1935
Oxford 1936	St. Louis, Chicago 1936.
Annecy 1937	
Krakow 1938	Detroit 1938
Elsimore 1939	Philadelphia 1939
	Chicago, New Orleans 1940
	Chicago, New York, Dallas 1941
	Cleveland 1944, 1946
	Atlantic City, Washington DC, Chicago 1947
The Hague 1948	Madison, Cleveland 1948
Colmar 1949	Bolder, New York 1949
Varese 1950	Berkeley, Cambridge, Chicago 1950

Table 2.a.6 *Econometrica* compared to the *Economic Journal* (1933–55) (number of papers by type of subject)

Year	Econometrica					Economic Journal				
	G Th	*St Th*	*Math*	*History*	*Em St*	*G Th*	*St Th*	*Math*	*History*	*Em St*
	Editor: Frisch					Editor: Keynes				
1933	13	6	2	2	5	17	0	0	0	11
1934	12	2	1	4	4	19	0	0	0	7
1935	15	2	1	1	6	14	1	0	2	10
1936	16	2	1	1	4	12	1	0	4	7
1937	6	4	1	2	4	15	0	0	1	14
1938	20	0	4	3	3	14	0	0	0	9
1939	21	0	2	3	4	18	0	0	0	8
1940	16	1	0	2	2	15	0	0	0	5
1941	12	3	2	1	1	10	0	0	1	7
1942	14	4	3	0	0	2	0	0	4	5
1943	11	3	2	0	2	4	0	0	0	12
1944	10	2	1	0	2	7	0	0	1	7
						Editor: Harrod				
1945	15	3	0	0	3	8	0	0	1	8
1946	20	1	0	1	0	13	0	0	1	10
1947	16	3	0	3	0	7	0	0	1	5
1948	11	1	1	2	1	10	0	0	0	11
1949	5	3	4	0	1	13	0	0	0	7
1950	17	1	4	2	2	18	0	0	0	11
1951*	18	0	1	3	1	20	0	0	1	9
1952	23	1	3	0	2	17	0	0	1	13
1953	21	0	2	4	1	21	0	0	0	7
1954	20	2	7	1	4	25	1	0	0	5
1955	18	3	4	0	3	27	0	0	0	5
Totals	350	47	46	35	55	326	3	0	18	193

Notes
* first book reviews in *Econometrica.*
G Th, general theory; St Th, statistical theory; Math, mathematics; History, history of thought; Em St, empirical studies.

3 The years of high theory

The year 1933 was an *Annus Horribilis* for the world but an *Annus Mirabilis* for economics. Frisch delivered the final instalment of his model of economic oscillations, a mathematical and analytical *tour de force* that marked the culmination of his meanderings through business-cycle analysis and established for a long time to come the authoritative pattern for mechanical representations in economics, juxtaposing stochastic shocks with a deterministic process. The same year, Kolmogorov provided the first rigorous axiomatic basis for stochastic theory, whilst Neyman and Pearson presented the complete model for the adaptation of probabilistic inference to decision making. Each of these contributions had a tremendous impact and generated waves of transformation which, during the 1930s, would lead to the creation of econometrics and its incorporation of modern stochastic theory. This chapter deals with the institutional framework of that intellectual evolution.

In the 1920s and early 1930s, when the econometric movement was being built, statistical research, and in particular the theoretical and applied analysis of business cycles, which was the most fashionable application of statistics in economics, was concentrated in a rather small number of small institutes. In continental Europe, at least six institutes were engaged in this research: the Institute of Economics in Oslo with Frisch, the Central Bureau of Statistics and the Netherlands Economic Institute, where Tinbergen was working, the German Institut fur Konjunkturforschung led by Wagemann since 1925, the Institut fur Konjunkturforschung created in Vienna by Mises and Hayek in 1927 and directed by Morgenstern in the early 1930s, the Belgian Business Cycle Unit at Leuven under the direction of Dupriez and the Conjuncture Institute in Moscow with Kondratiev and Slutsky, created in 1920 and active during that decade. In Britain, apart from the Cambridge Keynesian group that was researching into anticyclical policy, there was the Oxford Institute of Statistics with Marschak.

In the US, three separate lines of research were developed. At Harvard, W.M. Persons pursued research providing rough descriptive indexes on economic fluctuations, whereas, under Wesley Mitchell's guidance, the NBER researchers sought to obtain rigorous measures of business cycles, creating for that purpose a generally applicable method based on non-inferential statistics. In Columbia, working separately from the institutionalists, Henry Moore, Henry Schultz and

Harold Hotelling also contributed to stochastic theory and, in the case of the latter two, to neoclassical theory. At the same time, an influential research was developed at the Bureau of Agricultural Economics, created in 1922 and which was dominated by Mordecai Ezekiel and Frederick Waugh.

This research echoed the important developments in statistical theory that had been taking place for some time at London University College and the Galton Laboratory under Karl Pearson and at Rothamsted Agricultural Experimental Station under Ronald Aylmer Fisher. In the US, the Bureau of Agricultural Economics had provided training in modern statistics since the 1920s and, just as R.A. Fisher had been doing in Britain, established new patterns for experimentation and randomisation protocols. In Paris, Borel and other researchers at the Poincaré Institute were also contributing to the emergence of a new paradigm in statistics. A revolution was taking place, with new methods of sampling and inference in the framework of a defined stochastic theory. In a sense, this was part of the major leap forward inspired by the development of quantum mechanics and the redefinition of physics but, as this chapter shows, there were substantial differences and important peculiarities in economics, the most important being the heated discussions on the epistemic relevance of the analogy drawn from mechanics.

In spite of such diversity and pluralism, all these researchers – Table 3.1 indicates the structure of this generation as photographed in 1933 – considered themselves to be part of an emerging and challenging world of new thoughts and techniques, certainties and doubts. A hurricane of change blew through the sciences and econometrics was born in the midst of it all. Consequently, throughout the 1930s, rampant discussions established the contours of new methods and approaches to the social sciences: this chapter presents some of the institutions and scientists involved in this process and their position in relation to the analogy with mechanical models as the basis for applying statistical tools.

Table 3.1 Age of economists, econometricians and statisticians in 1933

	Europe	*USA*
The elder	K. Pearson 76 A. Bowley 64	I. Fisher 66 H. Moore 64
The references	M. Fréchet 55 E. Slutsky 53 J. Schumpeter 50 J.M. Keynes 50 F. Hayek 44	W. Mitchell 59
The founders	F. Divisia 44 R.A. Fisher 43 N. Kondratiev 41 J. Neyman 39 E. Pearson 38 R. Frisch 38 J. Marschak 35	A. Cowles 42 H. Schultz 40 H. Hotelling 38 C. Roos 32
The young	R. Harrod 33 O. Morgenstern 31 A. Kolmogorov 30 J. von Neumann 30 J. Tinbergen 30 O. Lange 29 T. Koopmans 23 T. Haavelmo 22	W. Leontief 27 M. Friedman 21

The mechanical representation inducing a mathematical analogy lay at the heart of Irving Fisher's early contribution to economics and became the condition for the adaptation of neoclassical economics as he understood it. But, in different ways, many of the other prestigious scholars of the mature generation either opposed or resisted the spread of this mechanical metaphor for a new mathematical language: this was the case with Schumpeter and, most particularly, with both Keynes and Mitchell. In contrast, the younger generation in Europe and the US enthusiastically adhered to the model of models that mechanics induced and provided: the rise of econometrics represented their victory, in spite of some later afterthoughts. In any case, these differences of attitude moulded the shape of institutions, as well as the research programmes and strategic choices of econometricians in this period of high theory.

Britain, the eugenic connection to statistics

Until sampling inference was developed, statistics was simply regarded as a tool for the measurement and appreciation of abstractly framed hypotheses. Originally, the Gaussian curve was conceived to describe observational errors in relation to a precise law in astronomy and, as it was extended into the field of description in the social sciences, it revealed the wonder of the *homme moyen*, being understood as a language of Nature that established the pattern for humankind. Consequently, as it was used in physics, in biology or in the social sciences, the law of errors was at first simply interpreted as a confirmation of perfect determinism, affected to some extent either by human errors of measurement or by perturbations such as those occurring in biology, which were Nature's errors in any case. Consequently, variation was simple perturbation, as it was for Quetelet (Gigerenzer *et al.*, 1997: 55).

This situation would not last. By the end of the nineteenth century, new analyses of the essential nature of variation as part of dynamic processes challenged the previous interpretation of errors. One such interpretation emerged from thermodynamics, from the understanding of entropic degradation as a form of variation. On the other hand, although the exact nature of indeterminism and determinism remained an ongoing subject of dispute between Einstein and other scientists, quantum physics defined a new concept of stochastic processes. In both cases, the model of mechanics still prevailed and Boltzmann dedicated his work to the central aim of reducing thermodynamics to mechanics.

One disciple of Boltzmann, Paul Ehrenfest, played a major role in the events relevant for this narrative. A professor of theoretical physics at Leiden, Ehrenfest was part of the contemporary revolution in physics, was a friend of Einstein, Bohr, Heisenberg and Pauli and, more important, was interested in social sciences. In a letter to Schumpeter, he stated that 'I am particularly interested in theoretical economics because it conforms to mathematical physics and because, methodically, it has several points of contact especially with a theoretical physics discipline which has kept me occupied for a long time, thermodynamics.'[1] Notwithstanding his interest, Ehrenfest lamented the decision of his outstanding student, Jan Tinbergen, to turn to economics. Tinbergen benefited from his knowledge in physics and mathematical expertise, but did not import, use or even consider physical analogies for the description of social phenomena as such – unlike the neoclassical revolution had extensively done – but he certainly generalised mathematical methods and statistical procedures conceived as mechanical counterparts of social processes (Boumans, 1992). In any case, explaining the mechanism of evolution was only a pale alternative to explain variation. Mechanics did not qualify in explaining the strange and the unpredictable events.

Furthermore, for a long time, many social researchers were looking not in physics but in biology for radically new thinking on the nature of variation. Indeed, the concept of error remained ill-defined in biology until the understanding of genetic inheritance and mutation became available and, afterwards, even though it was now well-defined, it still remained indeterministic: in biology, error is also perturbation in the meaningful sense of the word and it is the error itself that drives the dynamics. This conundrum mirrored the difficulties of the social sciences in defining their method of analysis and, given their envy of the quantified and exact sciences, suggested the use of statistics as the most suitable improvement: from physics, the social sciences incorporated the requirement of exhaustive measurement and therefore the need for the statistical treatment of data; from biology, they took the concept of the nature of data. And statistical tools were available, even when these were not produced for the specific purposes of each science, such as factor analysis, which was generated as part of the development of psychology, and the analysis of variance, which was conceived as a part of agronomy (Gigerenzer *et al.*, 1997: xiii). Statics, the measurement of the supreme mechanics of Nature, reconciled social sciences, biology and physics.

Yet, the analysis of errors and variation was not simply motivated by the evolution of theory and science itself but also from one of its applications, and a most debatable one. The conscience of genetic inheritance and of variation produced an intellectual wave, which had been highly influential since the rise of modern biology and whose importance has generally been overlooked in the history of both statistics and economics: the eugenic movement. In Britain, eugenics was a major motivation for the development of applied statistics, since it could not only highlight the processes of inheritance and variation, but also indicate the tools that could be used for the betterment of humankind. The movement was created around Francis Galton in 1907 when the Eugenics Education Society was formed. It valued inheritance more than variation, proposing the promotion of an elite for the betterment of society, through methods of selection and support of the fertility of those sharing elevated 'civic worth' and moral values (Klein, 1997: 184).

It was followed by Karl Pearson, later appointed Galton Professor of Eugenics. R.A. Fisher chose statistics for his early involvement with eugenics (ibid.: 58–9, 91). Eugenics, at that stage, involved strong claims for national and social segregation, as Fisher clearly stated in 1914:

> Eugenics is not inherently associated with nationalism; but in the world of nations, as we see it, nationalism may perform a valuable eugenic function. The modern nation is a genetic, a territorial, and an economic unity, and the modern tendency is to emphasise its essential unity, the community of interests of its individual members; European nations are grouping themselves along ethnic lines, and the individual finds himself more and more closely engaged in serving the greater interests of his race. . . . The socially lower classes have a birth rate, or, to speak more exactly, a survival rate, greatly in excess of those who are, on the whole, distinctly their eugenic superiors. It is to investigate the cause and cure of this phenomenon that the eugenic society should devote its best efforts.
>
> (Fisher, 1914: 310–11)

Biometrics was generated as a tool for eugenics and, from that point of view, one of the roots of modern statistics in social sciences is the segregation connection. In particular, it was intensely developed in order to measure characteristics and correlations for the analysis of inheritance, and produced ingenious geometric procedures renovating statistics; Charles Spearman, for instance, designed factor analysis for the justification of theory of physical structure of inherited intellect and capacities. One of those influenced by Pearson across the Atlantic, Irving Fisher, also an eugenist and member of the society, called for government control of the reproduction of humans for the improvement of society. His own concept of the 'dance of the dollar' was indeed inspired from Pearson's essay on the statistics of the rates of death (Klein, 1997: 241fn.). Keynes, whom we will see briefly in conflict with Pearson, was also a convinced supporter of eugenics.[2] Indeed, for the *fin-de-siècle* statistical generation, British eugenics was what social engineering later became for the econometrics generation.

In spite of such motivation, the heterogeneity of this movement as far as the choice of the concepts of statistics was outstanding. This section briefly presents the triangular disputes taking place between Karl Pearson and R.A. Fisher, and then between the latter and Jerzy Neyman and Egon Pearson, as well as Keynes's objections to their work.

Karl Pearson

Karl Pearson enjoyed an impressive career in science, although his contributions to literature and cultural history are undeservedly ignored today. Since a detailed biography has recently done him some justice (Porter, 2004), the next lines refer exclusively to his contribution to statistics, his main topic of research from 1892 onwards, when he embraced the ideal of universal quantification. A few years earlier, in 1884, Pearson had been appointed Professor of Mathematics at University College, London, but essentially divided his time between mathematics and cultural history.

Pearson's devotion to cultural history, greatly nurtured during his formative years in Germany amidst a whirlwind of artistic innovation and social conflict, heavily influenced his thinking and determined his preference for historical analysis. For this reason, he defended historicism and loathed neoclassical economics based on utilitarian values, since he could only conceive of history as the basis for the political economy of social choices (ibid.: 70). Nevertheless, when he made his contribution to the discussion of scientific methods, with his 1892 best-seller *The Grammar of Science*, Pearson concentrated on other subjects, such as the explanation of his idealism and staunch positivism. A follower of Ernst Mach,[3] he took the extreme view that we cannot understand or make definite statements about reality, but that science is still possible since the mind is a machine organising our sensory experiences, the only accessible path to reasoning (ibid.: 203). Consequently, even if there is no sense in imputing causality, the method – the positivist method – is the sole characteristic discriminating sciences from other forms of knowledge, and it is universal. The fabric of the mind highlights the mechanical structure of the world. Porter called this period the British 'high baroque phase of the mechanical world view' (ibid.: 179).

This 'ostensible positivism' allied to radical anti-materialism produced a pugnacious devotion to statistics: as science is description and cannot be explanation, then statistics is the best description available since, furthermore, law and order are the expression of the mechanic of the universe: 'Our concept of chance is one of law and order in large numbers; it is not that idea of chaotic incidence which vexed the medieval mind' (Pearson, 1897: 15).

The recourse to probability was seen by Pearson as being equivalent to the rejection of the concept of causality, which was deemed to be unattainable and therefore useless. Instead, he proposed the computation of correlation as the core concept for biometrics, in order to establish an undisputed number encapsulating a complex description of the relationship between sets of data that can be either totally independent or totally dependent, or have some measurable dependence – correlation – thus fully replacing the concept of cause (ibid.: 187, 212, 257, 261). Pearson's eugenics was based upon biometrical research with exhaustive quantification: correlation ruled everything.

A strict positivist, Pearson defined chance as the result of human ignorance and ignored variation in nature: the errors of measurement were described by the commonly found bell (Gaussian) curve (ibid.: 237, 259). But he was simultaneously mostly interested in departures from normality, as his eugenic philosophy suggested: the application of probability calculus to biological data was meant to establish the actuarial basis for eugenics, and therefore irregularities and the end of symmetry as proof of evolution were his cherished topics. For Pearson, evolution was the quintessential field for statistics, away from normality (ibid.: 237, 255, 254; also Gigerenzer *et al.*, 1997: 113). By the same token, Pearson was hostile to the application of mathematics to the social sciences, given their complexity (Pearson, 1897: 215).

In spite of this approach, John Maynard Keynes violently challenged Pearson, who was 'one of [his] long-standing *bête noires*'. The occasion for presenting his challenge arose in May 1910, when Pearson published a report on the influence of parental alcoholism on children, concluding that heredity and not the parents' habits influence the offspring, since he had found no evidence of statistical difference in the samples differentiating between those children with alcoholic parents and the others (Skidelsky, 1992: 223f.; Moggridge, 1992: 205f.). The conclusions provoked a storm. Keynes was one of the voices raised against Pearson, whom he accused of being 'actually misleading'. Two methodological objections were raised by Keynes: statistical inference may be deceptive whenever there is no homogeneity in the population, and the nonrepresentativeness of the samples is not established beyond reasonable doubt. The Cambridge establishment supported Keynes: Pigou and Marshall stood by him and contributed to the debate, which was spreading from the letters section of the London *Times* to the scientific journals. Although the debate was not fully developed, Keynes seemed to deny the use of *ceteris paribus* conditions and certainly expressed his hostility towards the use of mathematical methods in the framework of the social sciences and, in particular, towards the use of unsubstantiated probability theory.[4] Both arguments would re-emerge later on in his survey of Tinbergen's econometric work.

A different controversy opposed Karl Pearson to R.A. Fisher, who was not subject to the same sort of philosophical constraints when approaching statistics. The two men disagreed about the definition of modern statistics: whereas Pearson's work was based on correlations of observations, for Fisher the core of statistics was the analytical capacity for testing significance applied to experimental data. Their divergence turned into a bitter conflict without any cessation of hostilities, an epic struggle punctuated by angry criticisms and an institutional dispute.

Ronald Aylmer Fisher

R.A. Fisher studied mathematics and physics at Cambridge, where he graduated with distinction in the mathematical tripos of 1912, but he was also interested in biology and eugenics. His dedication to statistics was determined by his interest in the theory of errors, applicable both as a general mathematical subject and as a specific theme for biological research. His mathematical expertise did not go unnoticed, and in 1919 Karl Pearson invited him to be the chief statistician at Galton, although he imposed the condition that Fisher should submit publications for his prior approval. Fisher instead chose to take a job at the Rothamsted Agricultural Experimental Station, where he developed his experiments, working on the protocols of randomisation and defining the analysis of variance. In 1933, when Karl Pearson retired from University College, R.A. Fisher was invited to replace him both in the Galton Chair and as Head of the Department of Eugenics, in spite of the irreconcilable disputes between the two men.[5] In any case, the department was divided between Fisher and Egon Pearson, Karl's son, who took over the Department of Applied Statistics, with there being barely any contact between the two parties.[6] In 1943, Fisher moved to Cambridge and then, when he retired, to Adelaide in Australia.

By 1925, when he published *Statistical Methods for Research Workers*, a book which had a large impact, Fisher's modern theory of estimation and significance tests was already complete from his agricultural experiments. He worked on the determination of the sampling distributions of small samples, defined the significance tests and introduced the concept of maximum likelihood, all seminal contributions that changed the way in which statistical inference was carried out. Fisher was rather optimistic about the impact of his methods, since he believed he could tame chance: 'The effects of chance are the most accurately calculable, and therefore the least doubtful, of all the factors in the evolutionary situation' (Fisher, 1953: 511). Moreover, statistics could provide us with the essence of the process of knowledge, since 'inductive inference is the only process known to us by which essentially new knowledge comes into the world' (Fisher, 1935a: 7).

Seen in perspective, it is fair to state that it was the impulse provided by Karl Pearson and the developments and improvements introduced by R.A. Fisher that transformed statistics into a branch of mathematics and therefore changed its scientific status (Gigerenzer *et al.*, 1997: 69). In a sense, Pearson paved the way for the new method, but he was also fascinated by eclectic topics, namely irregularities and departures from Gaussianity, unlike the dominant view shared by Galton, Fisher and many others, who created the technology of the new concepts.[7] Their approach was certainly different, since Pearson used large samples from natural populations, whereas Fisher used small samples from controlled experiments and the latter were more easily generalised. Consequently, it was Fisher who dominated the immediate developments of statistics.

Establishing the function and procedure of significance tests, Fisher did not concede anything in relation to their use and abuse and claimed that the tests were unable to assign a degree of probability to the hypothesis (ibid.: 93). Furthermore, the tests could only provide information and not confirmation: 'In fact, logically all the tests of significance are means whereby the facts are allowed to disprove some definite hypothesis, and not means whereby they are ever allowed to prove such an hypothesis.'[8]

Fisher's experimental approach did not leave much space for interest in the social sciences and he never directly addressed the difficulties of the subject. Yet, at least once Fisher considered the problem, when he offered the theme as the topic for a lecture organised by Schultz in Chicago:

> I am, as you know, much interested in the logic of experimentation, and it might be interesting to try and broach the special statistical difficulties which pervade sociological enquiries, and the extent to which they can be circumvented without experimental control of the material.[9]

Although nothing came of it, Fisher followed the development of statistical applications in the social sciences, since he was in close contact with Harold Hotelling, an American mathematician turned economist.

Hotelling had spent the year 1929–30 as a volunteer at the Rothamsted farm, studying with R.A. Fisher (Gigerenzer *et al.*, 1997: 118), and they had a very close personal relationship from then on. Yet their intense correspondence of the 1930s and 1940s concentrates on mathematical problems and theoretical developments in statistics, and not on concrete applications to economics. Schultz, the other American economist who had been given specific statistical training, enabling him to match Hotelling, had previously studied with Karl Pearson in London, and was also aware of the controversy and the available techniques. In any case, Fisher certainly produced the most influential indirect contribution to the development of probabilistic inference in the 1930s and was highly regarded among the early econometricians, who looked on him as a source of inspiration.[10]

Nevertheless, there was a reason for the alienation of economics from the statistical world: its non-experimental nature. Fisher was very demanding about the design of experiments and this strict view of the scope of statistics was to bring him into conflict with Jerzy Neyman.

Jerzy Neyman

Neyman, born in Russia from Polish parents, was a younger man, a powerful mathematician and polemicist, who chose statistics under the influence of his reading of both Lebesgue on the theory of measure and then Karl Pearson's *The Grammar of Science*. After travelling to Britain in 1925, where he came into contact with the statistical intelligentsia, including both Pearson and Fisher (only to discover to his dismay that Pearson was unaware of modern mathematics), Neyman attended Borel, Lebesgue and Hadamard's lectures in Paris and then returned to his post in Poland, a country that would soon come under threat from Hitler. By the end of 1933, Egon Pearson, a close friend with whom he enjoyed an intense intellectual collaboration,[11] had arranged a job for him at University College.

Neyman was trained in the statistics of agricultural experimentation, since one of his first jobs was at the National Agricultural Institute in Poland, and this was the subject for his 1924 Ph.D. dissertation on probability. Then he was given a job at Warsaw University, where he excelled in teaching and research: one of his students was a certain Oskar Lange (Reid, 1998: 76), who had a role to play in Neyman's future career. But it was in theory that he excelled in particular: when Neyman came to London, he had already published a number of papers with Egon Pearson, presenting a new theory of estimation.

The initial initiative for such cooperation, and indeed the original idea for the new theory, came from Egon Pearson, who developed it from a curious insight he discovered in a personal letter from Gosset (a statistician who signed himself 'Student', his well-known alias, and worked at a brewery). Egon succeeded his

Figure 3.1 Jerzy Neyman (source: courtesy of University of California, Berkeley).

father but had a number of disagreements and breaks with him (ibid.: 55, 63, 81, 98), and his cooperation with Jerzy Neyman provided him with the opportunity to develop his own career and thinking. The first instalments of the new theory, the method of statistical inference for the testing of hypotheses, were published from 1928 onwards. By 1933, the Neyman–Pearson theory had been stabilised.

In spite of the fact that the authors initially claimed R.A. Fisher's influence, more than Karl Pearson's, the fact was that they diverged from his view of statistics. And Fisher, who had behaved paternalistically towards them at first, soon remarked and emphasised the differences, completely rejecting the Neyman–Pearson (N–P) approach. In particular, the N–P method required the definition of a rival hypothesis and repeated samples in order to define the error of the first and second types – a condition Fisher could only reject. Furthermore, this procedure determined the size and power of the test, whereas for Fisher the significance level could only be a property of the sample itself and not of the actual test (Gigerenzer *et al.*, 1997: 98). Consequently, Fisher scorned the N–P strategy and used whatever opportunity he found to belittle it, as in 1945:

> In recent times, one often-repeated exposition of the tests of significance by J. Neyman, a writer not closely associated with the development of these tests, seems liable to lead mathematical readers astray, through laying down axiomatically, what is not generally agreed or generally true, that the level of significance must be equal to the frequency with which the hypothesis is rejected in repeated sampling of any fixed population allowed by hypothesis. This intrusive axiom, which is foreign to the reasoning on which the tests of significance were in fact based, seems to be a real bar to progress.
>
> (Fisher, 1945: 130)

Neyman, who was also a fierce polemicist, frequently showed his disagreement in both tone and substance, for instance with Fisher's theory of fiducial influence, which he confronted with his own theory of confidence intervals, as he had done before: 'In this light, the theory of fiducial inference is simply nonexistent in the same sense as, for example, a theory of numbers defined by mutually contradictory definitions' (Neyman, 1941: 393).

The confrontation between the two statisticians was gigantic. The clash of personalities was intense and their divergence of views was irreconcilable. Fisher, who could not accept a generalised concept of inverse probability based on inference from samples to populations and favoured the selection of controlled experiments for that purpose (Fisher, 1935b, 1951), strongly opposed the Neyman–Pearson strategy and its assumptions:

> If Professor Neyman were in the habit of learning from others he might profit from the quotation he gives from Yates, for Yates is there warning his readers against a pitfall into which it would seem Neyman has himself fallen. I refer to the fallacy that a statement of fiducial probability about the population from which an observed sample has been drawn, refers not

> simply to that population, but to an imaginary, or hypothetical, aggregate of populations from which, at some anterior stage, it was picked at random. Even if such an aggregate existed, which is scarcely axiomatic, it is obvious that nothing could be known of it, beyond what we can learn about that one of its members which has been sampled.
>
> (Fisher, 1957: 179)

Although this divergence is too frequently ignored, since, from this point on, statistics was based upon a combination of Fisher's and Neyman–Pearson's strategies – hybridisation, as Gigerenzer and his colleagues call it (Gigerenzer *et al.*, 1997: 106f.) – the fact is that the premises of both approaches differed quite considerably. The non-experimental character of most of the data in social sciences was indeed ignored through recourse to the Neyman–Pearson strategy, which – and this was an innovation – provided tools for the comparison of alternative hypotheses, i.e. for decision making. Fisher's significance tests could not deliver such a result and the author resisted any such use. This was the crucial reason for the quite remarkable success of the N–P strategy in economics, beginning from the moment when Haavelmo provided the rationale for its generalisation.

But, for that generalisation to occur, Neyman still had to go to the US, which he did when he accepted an invitation to move to Berkeley in 1938. And the situation also required the war to begin and attract, or keep, Haavelmo, Koopmans, Marschak and so many others in the US. Interested in the social sciences, unlike R.A. Fisher, Neyman held a seminar on economics the year after his arrival, which was attended by Larry Klein (Reid, 1998: 168), whilst Haavelmo spent some months – the decisive months for his conversion to stochastic theory – studying with him in California.

John Maynard Keynes

So far in this narrative, we have encountered major innovations, rivalries and challenges, in a period when lineages were being traced and institutions were being built: in short, evolution proceeded fairly smoothly. Karl Pearson taught Henry Schultz, R.A. Fisher received Harold Hotelling, Pearson's son Egon established a certain distance from his father, Neyman became disillusioned with the father and cooperated with the son, both of them quarrelled with Fisher. Eugenics, suggesting biometrical research and correlation techniques, was being replaced by experimentally controlled tests in agriculture as the focus of attention shifted from abstract measurements to practical applications leading to generalised methods. Finally, there were two alternative concepts for the testing of hypotheses: one proposed by Fisher and restricted to the computation of ratios of significance; the other propounded by Neyman and Pearson and based upon deciding between two alternative hypotheses. In economics, this statistical technology was the obvious candidate for exact quantification and yet also able to remain sensitive to the very nature of probabilistic decisions in social processes.

Nevertheless, all this was rejected by no less a figure than the most influential British economist, John Maynard Keynes. Indeed, Keynes was one of the best prepared economists, well acquainted with probability theory, but he ignored most of the new developments in statistical theory and rejected at least what he could understand or would follow. In fact, his whole approach to economics was based on extended philosophical arguments, and for that reason he was criticised by Fisher, who accused Keynes of lack of acquaintance with statistics, as a branch of applied mathematics (Fisher, 1923). In his first major work, *A Treatise on Probability* (*TP*), Keynes dealt extensively with two relevant epistemological categories: induction or the logic of accumulation of knowledge, and analogy or the logic of construction of knowledge. His first argument against statistics was precisely a version of the Hume paradox challenging general claims made through the process of induction:

> To argue from the *mere* fact that a given event has occurred invariably in a thousand instances under observation, without any analysis of the circumstances accompanying the individual instances, that it is likely to occur invariably in future circumstances, is a feeble inductive argument, because it takes no account of analogy.
>
> (*TP*: 445)[12]

In that sense, analogy was considered a precondition for induction, even if it was not demonstrative (ibid.: 74, 264–5). Keynes's concept of the construction of knowledge was based on these two correlative operations: the non-demonstrative and non-conclusive induction, and the analogy that allowed for a meaningful growth of knowledge from induction, the appropriate forms of thought in an organic world.

Two examples of these qualitative and organic features are the definition of both probability and expectations. Probability itself was for Keynes an 'organic unit', and he denied that it could be reduced to a physical unit or to an empirical frequency: 'A degree of probability is not composed of some homogeneous material, and is not apparently divisible into parts of like characters with one another' (ibid.: 32). Consequently, probability was considered to be mostly non-numerical, just as most instances in science are qualitative and non-measurable. In such a case, probability may not always be reducible to quantitative measurements, and the qualitative analysis – of probability or the 'weight' of the argument – is imposed by the organic nature of the process. And since one of the organic features of this world is the very formation of expectations (ibid.: 238), uncertainty is a building block used in all types of economic action and process.

In this sense, Keynes took an opposite view to Pearson's Machian philosophy: reality is knowledgeable, but it comprises uncertainty given its organic and predominantly non-mechanical character, whereas, for Pearson, the ultimate inaccessibility of reality was matched by the certainty of the mechanical cerebral representations. In fact, one of the main consequences of Keynes's organic vision was his profound distrust of mathematical methods as the last resort of truth, as they were introduced and generalised by mechanics. This was how Keynes replied in 1936 to a letter from Shove complaining about the difficulty of establishing very precise results:

> [You] ought not feel inhibited by a difficulty in making the solution precise. It may be that a part of the errors in the classical analysis is due to that attempt. As soon as one is dealing with the influence of expectations and of transitory experience, one is, in the nature of things, outside the realm of the formally exact.
>
> (Keynes, XIV: 285–321)

As a consequence, for Keynes, the mathematics of the econometric revolution of the 1930s was highly suspect,[13] even if he himself was a distinguished member of the Econometric Society and became its president in 1944 and again in 1945. In particular, he opposed the Walrasian theory and argued that equilibrium does not necessarily imply optimality, as there are many possible equilibria with severely different social consequences: the selection between them depended on the unpredictable behaviour of the internal forces of the system.[14] As a consequence, he generally emphasised the historical nature of social processes[15] and rejected mechanicism in economics, as he emphatically did with one of his amusing analogies:

> The pseudo-analogy with the physical sciences leads directly counter to the habit of mind which is most important for an economist proper to acquire. I also want to emphasise strongly the point about economics being a moral science. I mentioned before that it deals with introspection and with values. I might have added that it deals with motives, expectation, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worth-while falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth.
>
> (Keynes, XIV: 300)

The Keynes–Frisch correspondence, which is particularly relevant for this book, adds some new information about this resistance to mathematical methods and statistical inference. Keynes, then editor of the prestigious *Economic Journal*, rejected all offers of papers submitted by Frisch without exception. The first was a paper on the 'Pitfalls' debate opposing Frisch to Leontief, which the editor deemed too technical and only accessible to half a dozen readers. Pressing for changes in the paper's tone and content, Keynes offered 'to act as a midwife between your ideas and the average economist'.[16]

In 1935, Frisch and Keynes again engaged in editorial correspondence, and once again the paper, on utility measurement, was rejected. Some months before, Keynes had acknowledged a book offered to him by Frisch, despite indicating his own lack of training in mathematics: 'It looks to be a very interesting piece of work but, alas, though once qualified to taste such things, I am afraid that I should now find myself out of depth if I were to try to embark on critical discussion of this difficult branch of a subject which I have long neglected.'[17] This was again the reason for the rejection of the paper: 'I am unfamiliar with the methods involved and it may be that my impression that nothing emerges at the end which has not been introduced expressly or tacitly at the beginning is quite wrong.' Yet, Keynes was under the impression that the readers would not be able to understand it, given the 'mass of symbolism which covers up all kinds of unstated special assumptions'. He concluded that:

> I cannot persuade myself that this sort of treatment of economic theory has anything significant to contribute. I suspect it of being nothing better than a contraption proceeding from premises which are not stated with precision to conclusions which have no clear application.[18]

Politely, Frisch thanked his correspondent for his frankness and, again, Keynes commented on the task of the econometricians:

> I think it vitally important that econometricians should avoid using an elaborate symbolic language and pretentious mathematical formulae unless they intend to really bring something out at the other end. It has to be admitted, I think, that at the present time these methods are proving disappointing and in risk of falling into general discredit.

Frisch opposed him by saying that, although statisticians should avoid any pretentious symbolic language, they differed as to 'what a fruitful application of the mathematical apparatus is and what is not'.[19]

Consequently, when in a couple of years Keynes was to raise his disagreement with Tinbergen's methods, Frisch and other econometricians were entitled to conclude that this was just a fresh invocation of his general opposition to the use of mathematics and formal models. As a consequence, the core intuitive arguments presented by Keynes remained unnoticed and undiscussed among econometricians, largely due to his own fault.

Tinbergen only met Keynes once more, after their discussion on method, and witnessed his deep mistrust of mathematical and statistical methods even for confirmation of conjectures:

> I had the privilege of meeting him later, just once in 1946. On that occasion I told him that we had done quite a bit of research on the price elasticity of exports and we had really found that the elasticity is about 2, the figure that he uses in his famous book about German reparation payments. I thought that he would be very glad that we found that figure, and 'that he had been right'. But he only said: 'How nice for you that you found the right figure'. That was a most funny experience.
>
> (Tinbergen, 1987: 129)

The figure was for Keynes a mere index of his reasoning, and a non-demonstrative one – a funny experience for Tinbergen, as he nicely put.

The US, the fortress-to-be of econometrics

By the 1930s, when Britain was experiencing a leap forward in statistical theory in spite of Keynes, American academics were concentrating on applied statistics: at the NBER, Mitchell and Burns worked out a new descriptive statistical approach to business cycles; at Harvard, Warren Persons used composite indices for the same purpose. The exception was Henry Moore's tradition in Columbia, followed by Henry Schultz (who by the 1930s had already moved to Chicago), leading to a more theoretically oriented approach. Previously in the 1920s, the US Bureau of Agricultural Economics, in parallel with Rothamsted and other institutions, was a reference institution for statistical research. But the difference was that American economics was dominated by the dispute between the dominant Institutionalism and the emerging neoclassical economics, whereas in Britain the influence of the Marshallian tradition and the central importance of Keynes prevented the generalisation of the mechanical model.

Consequently, the US became the homeland for econometrics. John Bates Clark provided the first marginalist theories, but it was left to Irving Fisher to act as the guide for the neoclassical economists – he was also the first to embrace econometrics.

John Bates Clark

The first American neoclassical economist was John Bates Clark. After graduation, Clark spent three years studying in Germany at Heidelberg, under the supervision of Karl Knies, a distinguished representative of the old German historical school. But he also studied the works of Jevons and Menger and inherited those combined influences of historicism and marginalism. Hollander described the evolution of the group of young American 'historical economists' returning from Germany, some of them, namely Clark, being 'more inclined to deductive analysis' (Hollander, 1927: 2–3). In any case, back in the US, Clark opposed the institutionalist school and became the preferred antagonist of Veblen.

Although concerned with standard marginalist theory and therefore with static systems, Clark lengthily discussed the problem of the status of statics and dynamics: the real world was only describable by dynamics. But statics was 'imaginary' not because it did not describe real trends but because it was incomplete: 'The description of the purely static state, in fact, deals with realities. It is imaginary only by its omissions; for it presents an essential part of the forces that act in the real, dynamic world' (Clark, 1899: 401), and furthermore:

> All natural societies are dynamic. . . . In the actual world unceasing changes thrust labour and capital, from time to time, out of one occupation and into another. In each industry they change, again and again, the modes of production and the kinds and quantities of the goods produced. Yet this does not invalidate the conclusions of a static theory; for static laws are nevertheless real laws. The forces that would work in a world that should be held in a fixed shape and made to act forever in a fixed manner still operate in a changing world of reality.
>
> (ibid.: 30)

Since 'all real knowledge of the laws of movement depends upon an adequate knowledge of the laws of rest' (ibid.: 442), the economy could be modelled as a set of forces of 'organization', defining the equilibrium prices and quantities, and of 'progress', impelling the system towards a new level of equilibrium (ibid.: 30, 32, 429). Consequently, Clark deduced from dynamic principles that the tendency towards equilibrium prevailed. This implied an important conclusion: statics was defined as a particular case of dynamics but, what is even more significant, it was supposed that dynamic processes would converge towards equilibrium. In other words, equilibrium was supposed to be the final state of the system and, as a consequence, dynamics was reduced to statics. This crucial conclusion, which was opposed by Marshall and, later on, by his son John Maurice Clark,[20] represented the crucial influence by Clark on Schumpeter's paradoxical programme.

Indeed, a 1906 review of Clark's book, *The Distribution of Wealth*, written in 1899 and generally considered to be one of the important building blocks of marginalist economics,[21] was the theme for Schumpeter's first paper and, although generally unnoticed, it demonstrates the impact that this author had on the formation of Schumpeter's thought. This influence is particularly obvious in another deviation from Walrasian economics, precisely that which would constitute Schumpeter's main preoccupation in economics: the definition of the role of the entrepreneur, since the 'mechanical inventions' imply 'new kinds of goods [and] call for new industrial groups to make them' (ibid.: 61). Entrepreneurship, the ability to generate adventurous and heroic enterprise, was also a product of the cultural influence from Germany, common to Clark and Schumpeter and defined as a function distinct from those of capital and labour, once again relating dynamics to statics:

> Dynamic science deals with profits in their original state, as normally created by improvements in industry, in the proceeds of which the entrepreneurs have a share; while static science deals with them in their later and permanent state, as they are transmuted into increments of wages and interest.
>
> [. . .] Dynamic theory has to account for the whole of that friction on which the entrepreneur's share depends.
>
> (ibid.: 410)

Schumpeter's review was a short paper presented as a mere summary, 'abstaining from any criticism' (Schumpeter, 1906: 325). In fact, some of the main theses were presented without any detailed discussion: the 'natural' character of economic laws, the sea and tempest analogies for the equilibrium state and real economic processes, the social judgement implicit in the marginalist theory of distribution,[22] the concept of capital and the organic nature of social life. Although Clark's interpretation of entrepreneurship was ignored in this survey, later on, in *History of Economic Analysis* (HEA), Schumpeter emphasised this contribution:

> he made a great stride toward a satisfactory theory of the entrepreneur's function and the entrepreneur's gain and, in connection with this, another great stride toward clarification of all economic problems that must result from a clear distinction between stationary and evolutionary states.
>
> (HEA: 868)

Moreover:

> Clark's contribution was the most significant of all: he was the first to strike a novel note by connecting entrepreneurial profits, considered as a surplus over interest (and rent), with the successful introduction into the economic process of technological, commercial or organizational improvements.
>
> (ibid.: 894)

Clark – who was not one of the 'ten great economists' according to Schumpeter's list – was presented in HEA as the 'architect of one of the most significant theoretical structures [of marginalist analysis]' (ibid.: 868).

These influences synthesise the essential impact of Clark's work on Schumpeter's future research: all the major questions were indeed present in this rough sketch by Clark, including an anticipation of the idea of what Schumpeter would call the Kondratiev long waves of approximately forty-five years as specific periods of development of the dynamic forces of capitalism (ibid.: 429). In fact, the review of Clark's work was the essential step for the early definition of Schumpeter's whole research programme and he followed that agenda throughout his life.

Veblen's review of *The Distribution of Wealth*, written shortly after Schumpeter's, strongly opposed his 'hedonistic' programme and therefore the concepts of utility or the marginalist methodology, and in particular challenged his equilibrium metaphysics, themes that did not provoke any opposition from Schumpeter. According to Veblen, Clark's book was just another attempt to reduce dynamics to statics (Veblen, 1908: 189). From that point of view, Veblen argued that Clark's effort was a failure: 'All that it covers [Clark's concept of dynamics] is a speculative inquiry as to how the equilibrium re-establishes itself when one or more of the quantities involved increases or decreases' (ibid.: 188). Veblen accused Clark of being unable to account for change and mutation, i.e. for dynamics:

> Economics of the line represented at its best by Mr. Clark has never entered this field of cumulative change. It does not approach questions of the class which occupy the modern sciences – that is to say, questions of genesis, growth, variation, process (in short, questions of a dynamic import) – but confines its interest to the definition and classification of a mechanically limited range of phenomena.
>
> (ibid.: 192)

In any case, after this initial encounter, American economics was still under the spell of Institutionalism. Indeed, Clark's paradoxes could not win the battle and, moreover, his own doubts about mechanical or organic representations alienated the necessary resources for the imposition of the new paradigm.

Irving Fisher

From the moment that Irving Fisher published his dissertation in 1892, American economics gained its most emphatic plea for mechanics and was actually shaped by this dictionary translating the concepts of thermodynamics into economics. From theory to practice, Fisher included in the successive editions of his book photos of the physical models representing his thermodynamic toy economy. Some decades later, the mathematician Harold Davis, one of the recruits to the Cowles Commission in its early days, would still acknowledge that 'the famous table of mechanical analogies published by Irving Fisher in 1892 furnishes a powerful guide to explorations and generalization in economics' (Davis, 1937: 11). Indeed, mechanics had shaped neoclassical economics in the US right from its inception, and this was thanks to Fisher.

Fisher was a driving force for the introduction of concepts and methods copied from physics, and this was quite well accepted by his peers. As the most senior economist to become deeply associated with econometrics, his understanding of the subject was absolutely dependent on such appropriation. In a speech to one of the first econometric conferences, that held in Cincinnati in 1932, Fisher presented econometrics as a revelation of new methods fortunately invading the province of economics:

> And today, we suddenly wake up to realize that the methods which have made physics a science have at last taken a vigorous hold on the rising generation of economists. To illustrate, I need only mention among many others Frisch, Divisia, Rueff, Schumpeter, Keynes, Bowley, Amoroso, Gini, Haberler, Leontief, Zawadzki, Kondratieff, Hotelling, Moore, Schultz, Roos, Crum, Ezekiel and Rogers.

Fisher went on to present his programme: 'What we need in economics, in that branch of economics which we now call economic theory, is more of the old, old method which made astronomy, physics, chemistry and recently biology, into true sciences' (Fisher, 1933: 209, 210).

It is true that, later on, Fisher had some afterthoughts to share with his econometrician friends, since he considered the 'over-use' of mathematics to be dangerous. Recapitulating his own career from the days of studying with a distinguished physicist, Josiah Willard Gibbs, Fisher indicated how difficult it had been to define the proper use of mathematics:

> I started off in mathematics, having been a student of J. Willard Gibbs, the greatest scientific mind ever produced in the Western world, I suppose. I nearly went into his field, mathematical physics, but decided that economics presented an opportunity to build from foundations.
>
> My first publication, my doctor's thesis, was on 'Mathematical Investigations into the Theory of Value and Prices'. I soon found, however, that there was little market for my wares so that until recently I have curbed the old desire to use mathematics, and have relegated it to the appendices of my books lest I should have no readers. I have also deplored, as you have, every effort merely to parade mathematics or to use it where it did not really help materially. You did doubtless remember that Professor Marshall of Cambridge did the same.
>
> [. . .] I still deplore the over-use of mathematics, and to that extent welcome your article as wholesome in ridiculing over-use. On the other hand, I should be very sorry if the effect of your article should be in general to discourage the proper use of mathematics, for I believe that for certain problems it is all but absolutely necessary.
>
> (Fisher to Stephen Leacock, 4 January 1937)

Eager to gain academic recognition and status, econometricians welcomed Fisher's foundational participation in the Society and read his contribution as a powerful argument in favour of exhaustive quantification and modelling.[23]

The distinct brands of neoclassical economics

After Fisher, the American civil war between marginalists and institutionalists never stopped – and the thesis of this book is that the conditions that changed the balance of forces were both the rise of econometrics and the institutional changes associated with the Second World War. Econometrics provided what neoclassical economics missed most: the empirical capacity and technical resources generally, but not always, based on the acceptance of mechanical models.

In fact, as time went by, two of the main neoclassical schools in the US fully adopted the epistemics of the mechanical representation for the sake of measurement and inference: the MIT with Paul Samuelson and the Cowles General Equilibrium approach, later developed by Kenneth Arrow and Gerard Debreu. The third brand, however, that of Chicago, and particularly Frank Knight, was deeply hostile to mechanicism and the analogy with thermodynamics: 'There is no direct analogy with equilibrium between objects stationary in a field of force. The true physical analogy would require an elaborate construction hardly undertaken so far in the literature' (Knight, 1944: 309). Knight, who had transformed Chicago after the 1920s into an anti-institutionalist fortress, went so far as to scorn quantitative methods and empirical research (Mirowski, 2002).

This was challenged by the recruitment of Henry Schultz, who moved from Columbia to Chicago in 1926. Schultz had learned statistics at the London School of Economics and at the Galton Laboratory with Arthur Bowley and Karl Pearson. He then returned to Columbia and took his Ph.D. with Henry Moore, himself a student of Carl Menger's in Vienna. Moore's main project was the statistical analysis of demand and supply and, although he later developed some hostility towards his fellow neoclassical economists, he is sometimes listed as the founder of econometrics, given his dedication to this statistical research. Moore was followed in this work by his students, Schultz and Paul Douglas.

Moore, who was established since 1902 at Columbia, had also been largely influenced by the work of Karl Pearson, whom he had frequently visited, and had been one of the first to use harmonic analysis applied to economic series (Klein 1997: 249, 252). He was rather sceptical of the physical analogies and of neoclassical economics, as he clearly stated that 'no mathematical economist, as far as I am aware, has ever attempted to pass from this or any similar representation of a statistical, hypothetical equilibrium to a realistic treatment of an actual, moving equilibrium' (Moore, 1929: 106). Consequently, Moore did not endorse the Walrasian–Paretian approach, which was followed by the most distinguished of his disciples, namely Schultz.

Schultz was the first to engage in large-scale experiments following R.A. Fisher's methods, long before Cowles created the proper environment for theoretical and statistical econometrics, and became one of the most distinguished advocates for general equilibrium economics – and for the mechanical representation of economic models. In Chicago, he became the mentor of the young Milton Friedman – both of them discovering Slutsky's 1915 paper on demand theory, which had a major impact.

In a paper presented on 30 June 1930, to the joint meeting, held in Chicago, of the American Society of Mechanical Engineers, the American Society for Testing Materials and the American Institute of Electrical Engineers, Henry Schultz presented a paper summarising quite conveniently the mood of American econometricians. Under the title 'Engineering and Economics',[24] this paper is one of the large cohorts of essays dealing with the conditions for establishing the connection between economics and mechanics. And although Schultz recognised the difficulties of the endeavour, just as many of his colleagues did at the time, this did not prevent him from working hard in this direction.

Schultz assumed that the condition for the metaphorical redescription of economics[25] was to equate human agents with particles, just as Irving Fisher had previously done and as Moore rejected. Consequently, Schultz argued that:

> The dynamic problem of a physical system may be stated as follows: I know that I have a set of bodies (whether atoms, billiard balls or planets) placed in such and such places, and moving in such and such ways now; where will they be and how moving at any later time? The dynamic problem of demand may be stated in similar terms: We have a number of

> individuals with such and such desires (utility functions), subject to such and such obstacles, and consuming and producing and saving at such and such rates now. What will be their consumption, and how will their demand curves be moving at any later time?
>
> (Schultz, 1930: 3)

The answer, according to Schultz, may be provided by the equations of motion and the conservation of energy. Yet the author added a curious footnote: 'Economic equilibrium is probably more akin to chemical or biological than to mechanical equilibrium. But the latter is simpler and its laws have been more fully worked out. That is why it is generally used as a basis of comparison' (ibid.). The argument for simplicity was important and certainly motivated many of the econometricians, eager to find procedures for quantification and estimation, but it was based on an epistemic defeat. Moreover, this acceptance of the analogy with mechanics for the sake of simplicity created a new problem, since we do not have the analogue for its laws in economics:

> But what equations of motion, and what laws of conservation of comparable scope do we have in economics? To ask the question is to answer it. There are none that have the definiteness and universal demonstrability of the corresponding physical laws. Thus our economic laws of change are simply empirical extrapolations of the present situation; they do not enable us to determine with certainty what, for example, the demand and supply situation will be in the next instant of time.
>
> (ibid.)

In spite of the difficulty, which was widely accepted as a major shortcoming hindering the development of the new mathematical economics, Schultz argued that the resemblance would eventually dominate. The proof was the dedication to economics of a number of engineers: he referred to Walras, Pareto, Dupuit and Roos,[26] but the older Irving Fisher and the much younger Tinbergen and Koopmans would also have to be added to the list of trained engineers and physicists who had immigrated to the world of economics.

Schultz, a committed Walrasian, introduced extended quantitative methods for the estimation of demand and supply schedules and therefore offered a first approximation to a workable general equilibrium model. After Schultz's death from a fatal car crash, Chicago University decided to welcome the Cowles Commission and Marschak, Koopmans and the other staff came and joined forces with Yntema and Mosak, despite some resistance from the economics department.

Schultz's contribution towards the reshaping of American economics was matched by that of a mathematician, Harold Hotelling, who had obtained a Ph.D. from Princeton with a thesis on topology. Hotelling was recruited in 1927 to the mathematical department of Stanford and four years later replaced Moore at Columbia, where he supervised Arrow's thesis and pursued the fight against

the resident institutionalists. His cooperation with Schultz produced important developments in the estimation of demand functions and defined the programme for the next years of econometric research in the USA, but after Schultz's death he slowly abandoned economics. As previously stated in an earlier section, Hotelling spent one of his Stanford years at Rothamsted, working with R.A. Fisher, who became a close friend, and followed the developments of Fisher's theory of estimation. Hotelling was consequently one of the mathematical economists best prepared for dealing with probability, although he never applied it extensively to price theory or even to his important contributions to demand analysis.

In other words, the fully fledged incorporation of probability into economic models had to wait for the new developments brought about by Haavelmo and the Cowles Commission.

Wesley Mitchell

Mitchell, the founder of the American NBER, which was for a long time the main centre for research into cycles, strongly opposed neoclassical economics as he developed a specific descriptive statistical method for the inspection of trends and cycles. He did not accept the equilibrium assumption, and was therefore entitled to conclude that:

> Secular trends of time series have been computed mainly by men who were concerned to get rid of them. Just as economic theories have paid slight attention to the 'other things' in their problems which they suppose to 'remain the same', so the economic statisticians have paid slight attention to their trends beyond converting them into horizontal lines. Hence little is yet known about the trends themselves, their characteristics, similarities, and differences. Even their relations to cyclical fluctuations have been little considered.
>
> (Mitchell, 1927: 212–13)

Mitchell emphatically explained why he should ignore the concept of equilibrium:

> Nor can the idea presented in many theories that business cycles represent alternate rupture and restoration of economic equilibrium be included in our working construction. Men who take as their point of departure the theorem that economic forces tend to establish a stable equilibrium may conceive the main problem to be how this fundamental tendency is overcome at times and how it presently reasserts itself. I have not chosen that point of departure. Hence it is no part of my task to determine how the fact of cyclical oscillations in economic activity can be reconciled with the general theory of equilibrium, or how this theory can be reconciled with facts.
>
> (ibid.: 462)

His main arguments against the concept of equilibrium fall under two headings. On the one hand, Mitchell argued that the state of equilibrium could not be observed in economic series and therefore that the assumption about a mechanism of deviations and returns to the path of equilibrium was a mere unsubstantiated, ambiguous and arbitrary intellectual construct:

> To say that business cycles are departures from and returns toward a normal state of trade or a position of equilibrium, or that they are movements resulting from discrepancies between market and natural rates of interest, will not help, because we cannot observe normal states of trade, equilibrium positions, or natural interest rates. Nor, when we start observing, can we tell whether cyclical movements are due to factors originating within the economic system or outside of it.
>
> (Burns and Mitchell, 1946: 5)

The empirical analysis of business cycles in multiple series, a massive effort by Mitchell's NBER, was based on the assumption that not only was a general equilibrium pattern irrelevant, but also that each concrete cycle should be analysed as a historical individuality. The conclusion was severe: neither do normal equilibrium states have any ontological significance, nor does the distinction between endogenous and exogenous factors have any epistemological relevance. As a consequence, Mitchell wholly rejected the general equilibrium theory. On the other hand, Mitchell warned against its foundation, the mechanical analogy responsible for the pervasiveness of the equilibrium account, and suggested an alternative view:

> Doubtless it was a mechanical analogy which gave its vogue to the notion of economic equilibria. Everyone admits that analogies, though often most suggestive in scientific inquiries, are dangerous guides. The usefulness of the analogy in question was greatest and its dangers least when the economists were treating what they called 'static' problems. Such problems can be given a quasi-mechanical character.... But the problems of business cycles are the opposite of 'static'.... Yet there is a different conception of equilibrium which may help us – the equilibrium of a balance sheet, or better, of an income and expenditure statement. Such a statement has nothing to do with mechanical forces, and that is a safeguard against false analogies. It deals with pecuniary quantities, and they are genuine elements in our problem.
>
> (Mitchell, 1927: 186)

This alternative solution suggested by Mitchell was, of course, a very limited one. It simply stated that every economic system can be equilibrated a posteriori in an accounting manner, but that techniques can be adapted to any level of disequilibrium by the defined technicalities of the balance sheet. In that case, the final equilibrium situation is artificial since both columns add to the same value

whatever the reality of the economy may be. This alternative is merely argumentative and did not occupy any relevant place in Mitchell's study of the concrete cycles: the difference between the columns indicates the amount needed to produce equilibrium, and is therefore part of the ongoing action, while, of course, the concept of equilibrium implies a situation in which no action is necessary.

Schumpeter's critique of Mitchell's position concentrated on this second aspect and on his descriptive and allegedly 'non-theoretical' methods, but he also mentioned the first topic (Schumpeter, 1952: 337–8n.). Schumpeter's argument was that since Mitchell accepted that all agents sought pecuniary gains, then they were supposed to be rational and the system they conformed to should also be rational, i.e. equilibrating. The rationality assumption was for Schumpeter the safeguard of the equilibrium approach, confirming their mutual theoretical interdependency: of course, if one falls, the other follows.

In a book organised as a tribute to Mitchell's memory, Schumpeter argued most vividly for a concrete distinction between the two:

> He never would listen to the argument that rational schemata aim at describing the logic of certain forms of behaviour that prevail in every economy geared to the quest of pecuniary gains – a concept he understood so well – and do not at all imply that the subjects of this rationalistic description feel or act rationally themselves. And I shall never forget his speechless surprise when I tried to show him that his great book of 1913, so far as the bare bones of its argument are concerned, was an exercise in the dynamic theory of equilibrium.

More precisely, in a note to the same page, Schumpeter developed the argument: 'For what else are his "recurring readjustments of prices" to which he returned again and again but imperfect movements of the economic system in the direction of the state of equilibrium?' (ibid.: 329, 329n.).

In fact, Mitchell rejected the equilibrium approach because he rejected the mechanical view, because his empirical work neither required nor supported such hypotheses and also because he felt out of his depth when discussing the new methods. In an early letter to Frisch, at the time when the Econometric Society was being formed – a meeting to which Mitchell was invited – he insisted on his own difficulty with mathematics and on his 'strong preference' for realism:

> My position is rather that the statistical methods employed ought always to depend upon the end in view.
>
> [...] I have not the slightest objection to the utmost refinement when the data are of a character which will stand such treatment, and when the refinements lead to an increase of knowledge. That I do not indulge in such practices myself is due in part to my lack of mathematical skill, and in part to my strong preference for being able to give a realistic interpretation of my results.[27]

Another of the reference economists, Keynes, who was also invited to take part in the foundation of the Society, shared with Mitchell his difficulty with the methods whose generalisation was the precise aim of the Society. This testifies of the candour and honesty of both Mitchell and Keynes and of Frisch's manoeuvring to bring into the Society the most prestigious economists, whatever their views on the mechanical metaphor, the methods used or the very purposes of the Society.

Return to continental Europe

Apart from Britain, the rest of Europe was directly threatened in the 1930s by the rise of Nazism. Marschak, Schumpeter, Morgenstern, Lange and so many other economists either chose or were forced to emigrate, just as Einstein and other scientists had done. This provided a magnificent opportunity for the American academic world to invite a number of prestigious scholars and consequently to raise the quality of its research and teaching, whilst simultaneously impoverishing Europe. Yet, in the first years of the decade, some important research centres were still working hard in this area, both in Central Europe and in the USSR, although they were also subjected to drastic limitations.

Joseph Schumpeter

Schumpeter's first book, *Das Wesen und der Hauptinthalt der Theoretischen Nationalokonomik*, was published in 1908 when he was twenty-five years old, and it included a long appreciation of the *Methodenstreit*, the intense debate on method which opposed the Austrian theorists (Menger) to the German historical school (Schmoller), between the 1890s and the 1910s. Despite expressing his concern about the artificial separation between theoretical and historical methods, Schumpeter sided with Menger, under whose influence he had studied at the University of Vienna, just as Moore had done. By that time, he was a supporter of the marginalist school and mainly of the Walrasian approach.[28] The book dealt with general equilibrium and static analysis:

> In the centre of the book stands the problem of equilibrium, the importance of which is only slight from the viewpoint of practical applications of theory, but which is nevertheless fundamental for science. . . . The theory of exchange, price and money, and [. . .] the exact theory of distribution are based on it.
>
> (*DW*, quoted in Allen, 1991, I: 61–2)[29]

This presentation is very curious, since it indicates the limitations of the equilibrium analysis – its near irrelevancy for practical applications – but, in spite of this, it also emphasises its central status in the 'pure' theory, describing the 'changeless order and system in which everything fits together perfectly' (ibid.: 81). This is a paradoxical statement and indeed Schumpeter maintained the same attitude throughout his life.

Both his general equilibrium framework and his doubts about its applicability were present in his discussion with Walras. In 1909, Schumpeter travelled to Switzerland and visited the ageing Walras, who received and praised the book that he considered a fair presentation of his own theories – although until the very end of the interview he thought that it was Schumpeter's father's (Swedberg, 1991: 31, Allen, 1991, I: 84). Schumpeter described this visit only in 1937, and according to him Walras wrongly argued:

> that of course economic life is essentially passive and merely adapts itself to the natural and social influences which are acting on it, so that the theory of a stationary process constitutes really the whole of theoretical economics and that as economic theorists we cannot say much about the factors that account for historical change, but must simply register them.
>
> [. . .] I felt very strongly that this was *wrong* and that there was a source of energy within the economic system which would of itself disrupt any equilibrium that might be attained.
>
> (Schumpeter, 1937: 159–60)

In 1910, Schumpeter published a biographical article on Walras: the general equilibrium theory was praised as being able to 'illuminate' the purely economic relations by 'one single fundamental principle', and the author was presented as 'an enthusiastic admirer of Walras', 'the greatest of all theoreticians', who defined 'the only truly general theory to be formulated in the whole history of economics' (*TGE*: 112, 140, 139, 442fn.). In 1935, Schumpeter again stressed his acceptance of the Walrasian concept of equilibrium (Schumpeter, 1935: 4); this was repeated once more in 1939 (*BC*: 45). In 1942, *CSD* presented the Walrasian system as the foundation of economics. In *HEA*, he still praised the General Equilibrium paradigm as the 'Magna Carta of exact economics' and presented Walras as the 'greatest of all economists' (*HEA*: 968, 827).

Schumpeter acknowledged that the Walrasian system of stationary processes and static analysis was 'wrong' since it was incomplete and unable to deal with change and development. But, even so, he still considered Walras as the main modern economic theorist, since he was the only one to have created a science comparable to the achievements of physics and exact sciences. Indeed, Schumpeter was obsessed with the example of physics:

> And so we have reached a stage, perhaps for the first time, where facts and problems are before all of us in a *clear* and in the *same* light, and where analysis and description can cooperate in something like the spirit of physical science.
>
> (Schumpeter, 1927: 287)

For Schumpeter, physics provided the legitimate model for sciences.

But this did not prevent him from frequently emphasising what he considered to be the misleading nature of the physical metaphor: 'Analogy with the entirely different problems of physics is much more apt to be misleading than helpful' (*BC*: 32). In the same vein, he criticised Mitchell for his alleged overstatement of experimental procedures under the influence of the early physics-based epistemology,[30] and Pareto for his illusions about the application of the methods of physics, and argued about the differences between the field of economics and that of physics: since the former is more complex, there is no possibility of experiments, for it includes interpretative variables and the scientist is under pressure to obtain socially useful results (*TGE*: 149, 189–90).

Schumpeter knew that this paradigm dominated the main works of the neoclassicals, from Walras to Pareto and from Edgeworth to Fisher, and that it defined the contours of the marginalist revolution with the sole and relevant exception of his own teacher, Menger. Indeed, in the Austrian tradition there is widespread criticism of the physical analogy, such as that put forward by Hayek,[31] who denounced 'scientism', seeing it as an 'uncritical copying of the methods of mathematical physics in the equally uncritical belief that these methods are of universal application and the peerless example of all scientific activity to follow' (*HEA*: 17). In the course of his discussion about this point, Schumpeter was ready to accept Hayek's argument against the incorporation of such a 'scientist bias', but argued that this was not a general case in economics and that only words were being transferred from physics.[32] On the other hand, following Schumpeter's argument, there were two essential reasons why physical concepts – 'borrowing words and nothing else' – made their appearance in economics: first, mathematics was developed earlier in physics and the exact sciences[33] and, second, the analogies were supposedly very useful for teaching, since students understand a physical analogy more easily than its economic counterpart. The final argument put forward by Schumpeter was that the presence of common concepts and methods only showed that economists and physicists have the same type of brains and act similarly when faced with theoretical problems and, consequently,

> This does not involve any mechanistic, deterministic or other '-istic' errors, or any neglect of the truth that 'to explain' means something different in the natural and in the social sciences, or finally any denial of the implications of the historical character of our subject matter.
>
> (*HEA*: 18)

The whole passage indicates very clearly that, while accepting the general scientific paradigm of positivism and consequently the role of physics both as the forerunner of scientific rigour and as a model for pedagogic analogies, Schumpeter resisted the idea that economic concepts and methods were derived from physics. He did so because of the Austrian influence still present in his writings and possibly because his own research indicated that the available physical concepts were unable to encapsulate the economic reality of movement and change. But the author also accepted important exceptions, since some concepts and not only mere words ('equilibrium' and 'potential', for instance) were

incorporated under autonomous contents for each science. In any case, his own research concentrated on change and evolution and was therefore hostile to the notion of equilibrium since the crucial characteristic of capitalism is 'industrial mutation – if I may use the biological term – that incessantly revolutionizes the economic structure from within, incessantly destroying the old one, incessantly creating a new one. This process of Creative Destruction is the essential fact about capitalism' (*CSD*: 83). In other words, biological evolution, and not mechanics, could inspire and provide a metaphor for economics.

But the acceptance of the model of biological evolution did not ease Schumpeter's task, since this choice alienated the interpretation of his models in the framework of available representations. He simply could not develop a formal model for his theories. In a letter written in the early 1940s, Schumpeter argued that the organic nature of his thought was responsible for the difficulty of formalisation: 'there is nothing in my structures that has not a living piece of reality behind it. This is not an advantage in every respect. It makes, for instance, my theories so refractory to mathematical formulations' (quoted in Andersen, 1994: 2). Furthermore, his own innocence in mathematical techniques prevented Schumpeter from even trying to model his theory: his own diary proves that he worked almost daily and helplessly with systems of equations, at least after 1934 when preparing *BC*, and afterwards looking for a general equilibrium model accounting for the time path of the variables (Allen, 1991, II, 8, 142, 177, 190, 227). Samuelson, who was Schumpeter's student at Harvard, puts this rather candidly: 'I remember how my old teacher Schumpeter, perhaps Frisch's most fervent admirer, marveled at the miracle that imaginary numbers, $i=\sqrt{-1}$ in e^{it}, could drive "real" alternating current and "actual" business cycles' (Samuelson, 1974: 52n.).

Schumpeter is to be credited with the fact that he argued for the development of mathematical economics and was one of the founders of econometrics, fully aware of the tasks the movement was taking on – and yet he could neither understand modern mathematics nor accept the mathematical representation of his own theories.

Keynes and Schumpeter on mechanics

Schumpeter and Keynes had parallel careers in many senses. First, both discussed the hidden epistemology of the physical metaphor: Keynes rejected any borrowing from the physical methods and thoroughly discussed its statistical and mathematical implications; Schumpeter denied that this metaphor was in fact influential in economics, and stated that no borrowing was taking place. He nevertheless accepted the use of some concepts whose generality he considered not to be questionable, which were indeed concrete expressions of that metaphor, such as 'force' and 'equilibrium'. Consequently, Keynes contested the concepts of equilibrium and optimality, while Schumpeter accepted their relevance, in spite of all the practical difficulties involved. Keynes therefore defined economics as an intrinsically inexact science, while Schumpeter hoped it could attain the status of an exact science.

In fact, both used the notion of organic systems, and both rejected the use of the Darwinian or some other sort of biological metaphor – but the conclusions that they drew were quite opposite ones. Keynes's concept of an organic system was not at all trivial or merely descriptive, since it led to the definition of the nature of the variables, on the basis of a long-standing philosophical reflection. This was not at all the case for Schumpeter, an unrepentant positivist. As a consequence, his concept of innovation had a somewhat ambiguous status as an evolutionary model.

The organic conception was completed by Schumpeter's dynamic notion of evolution in time, while, despite acknowledging the constructive role of time – indeed this was the decisive feature in his dispute with Tinbergen – Keynes did not study evolution and predominantly used a comparative statics approach. On the contrary, Schumpeter was more and more interested in economic, social and institutional history, and considered it the central tool for a new theory.

Moreover, both described the nature of entrepreneurship as a function of the economic system and not of a separate social class: the autonomy of this function explained disequilibrium for Keynes, since the decisions of investment were logically independent from those of saving, just as it did for Schumpeter, given the fact that entrepreneurs decided to innovate and moved the system away from equilibrium. Of course, this was a major departure from the Walrasian theory, which described the action of entrepreneurs as passive, since they were not supposed to take independent decisions (Walras, 1874: 380; or 1883: 207–8; Morishima and Catephores, 1988: 41). In this sense, both theories were essentially non-equilibrium accounts.

Finally, both discussed the system's properties of stability. Keynes noticed that 'a profit seeking organization of production is highly unstable in the sense that a movement from equilibrium tends to aggravate itself' (Keynes, XIII: 394), while Schumpeter argued that 'under the conditions created by capitalist evolution, perfect and universal flexibility of prices might in depression further destabilize the system' (*CSD*: 95). But both conceded that the economic system has strong adaptive forces and creates order, just as it creates mutation.

In fact, both Schumpeter and Keynes were dealing with complexity in economic relations and trying to cope with its impacts on the mode of theorising. What else were Schumpeter's entangled explanations about equilibria and that intrinsic drive to instability and mutation, or what else was Keynes's understanding of the role of small effects producing large effects (Keynes, X: 362), other than their recognition of complexity? They understood the problem, even if in a paradoxical fashion: Keynes defined the transformation in economics as an indeterminate evolution moved by expectations and kaleidoscopic movements, but analysed those features from the viewpoint of a closed universe; Schumpeter, on the other hand, intuited the importance of innovation and therefore of an open universe and rejected such an indeterministic rationality, trying to close his model with regard to general equilibrium. Each of them operating in their own province, Schumpeter and Keynes had no close contact or any sort of conversation, which prevented any theoretical cross-fertilisation.[34]

They could not understand each other and neither of them could be understood by their fellow econometricians – if indeed they could be considered among the econometricians.

From Russia, with mathematics

In the late 1920s, another centre of statistical research was to be found in Moscow. Both Kondratiev and Slutsky worked at the Conjuncture Institute and were to be counted among the first to be invited to the Econometric Society, not only for reasons of geographical representation, but also in recognition of their prestige and capacity. Slutsky delivered two major contributions, one on demand theory (1915), which was later discovered by Friedman, Schultz and Hotelling, the other on spurious results from statistical averaging (1927), which was disseminated by Frisch. Kondratiev's work had been known for some time and converted Frisch, Schumpeter, Tinbergen and a number of other economists. Instead, Kolmogorov's work was apparently unknown to the econometricians.

Nikolai Kondratiev

Kondratiev, an agricultural economist and statistician, was responsible for the development of a new generation of applied statistical work in Moscow, after a very short political career – he was for a short time a minister in the last Kerensky government before the October 1917 revolution. In 1919, he was appointed to a teaching post at the Agricultural Academy of Peter the Great, and in October 1920 he founded the Conjuncture Institute. Kondratiev was also its first director and developed the Institute from just a couple of scientists at its beginning into a large and widely respected centre with fifty-one researchers working in different fields by 1923. Kondratiev's papers, suggesting the existence of a long cycle of about fifty years accounting for major changes in technology and social life, had an immediate international impact and offered new statistical methods and explanations for the business cycle and large economic fluctuations. Some of the most influential economists, statisticians and mathematicians of his time wholeheartedly supported his hypotheses or at least considered them to be meaningful: this was the case with Frisch, Tinbergen, Spiethoff, Kuznets, Mitchell, Schumpeter, Lange, Hansen, and many others.[35]

Like many of his contemporaries, Kondratiev was duly impressed by the rise of physics and the temptation of mechanical analogies for the explanatory models and methods. One of his first papers, on statics and dynamics (1924), explores the possibilities of the mechanical representation in economics. Statics was defined as describing the 'essence' of phenomena and, as a consequence, equilibrium became the central organising concept: 'The concept of equilibrium between the interdependent elements of reality is the most typical' (Kondratiev, 1924: 2). Yet these definitions were paradoxical: the Aristotelian 'essence' was supposed to be captured by statics, but reality is dynamic, since there are changes over time: 'Economic reality is dynamic in its very essence' (ibid.: 7).

As is usually the case with the concept of 'essence', its invocation was contaminated by confusion, which was not solved by Kondratiev's references to certain authors interested in dynamics: the Historical School, Marx, Schumpeter and Cassel.[36]

The 'essence' described by static equilibrium was supposed to be the core of the identity and invariance of phenomena, while dynamics was supposed to describe change and difference, under the concept of 'dynamic equilibrium'. But, according to Kondratiev, change presupposes the ontological identity of the object and that is why dynamics was considered to include statics. In that sense, he argued that dynamic processes comprise two types of movements: (i) irreversible processes, which have a direction, e.g. the growth of population and the volume of production, or the models of enlarged reproduction; and (ii) reversible processes, which may change direction, e.g. interest rates, prices and employment (ibid.: 17, 12). The Long Cycle – which Schumpeter would call the 'Kondratiev cycle' – or the 'curve of the conjuncture' – belongs naturally to the second type, if one disregards certain irreversible processes. As Kondratiev acknowledged, he was using a metaphor drawn from physics, the concept of *substratum*, although he recognised that this did not have a convenient analogue in economics (ibid.: 14–15).

In 1925 and 1926, Kondratiev formulated his theory on the basis of detailed empirical studies and argued that crises are 'organically' part of capitalism, as Marx and Juglar considered (1926a: 111). This was an argument in favour of holism: the organic concept of 'totality' implies that there is something more than the simple sum of the components, there is 'something new' in the whole (1926b: 63). Consequently, all cycles are part of the same economic process, as he stressed in a debate with Pervushin (Barnett, 1996: 1,021).

Furthermore, Kondratiev considered that this organic, holistic and non-atomistic epistemology was the necessary counterpart of the reality of social processes, in which the rationality of 'human intervention' implies the creation of a greater diversity than is to be found in the object of natural sciences (Kondratiev, 1926b: 83). In other words, unlike the neoclassicals, for whom rationality was typically associated with the behavioural pattern of a representative agent, Kondratiev attributed it to the creation of variation, although such variation was considered compatible with equilibrium. Indeed, for Kondratiev, the system always tended towards a moving equilibrium: 'So the long cycles of the conjuncture represent a deviation in the real level of the elements of the capitalist system in relation to this same system's equilibrium . . . a process in which the level of equilibrium itself changes' (ibid.: 159). Impulses were conceived of as disequilibrium processes, caused by 'radical changes in the conditions of production' through infrastructure investment in essential capital goods (ibid.: 158, 160). Kondratiev did not discuss in any detail this equilibrium around which the reversible processes were supposed to be organised. He just implied that equilibrium represented the most probable state of the system, and yet, for Kondratiev, the necessary condition for the adequacy of a model of cycles was the endogenous explanation of variation. In that sense, equilibrium was a cliché for Kondratiev and his allegiance to mechanical models was permanently flawed by his own statistical and historical account.[37]

Kondratiev's work is a powerful survey of the contemporary authors in economics, mathematics, physics and philosophy.[38] Well aware of the philosophical disputes of his time, Kondratiev adopted a cautious stance on recurrence and causality: there is no more than a slight chance of repetition of exactly the same causal environment, so *ceteris paribus* conditions are not met in economic history – each event is unique. But, according to Kondratiev, there is a stable causal structure, which accounts for a certain regularity of phenomena. Of course, this implied that the explanation of the complex whole is the priority for any inquiry conducted into the social sciences:

> We must emphasise in particular that each given whole is not the simple summation of its components and cannot be understood from the peculiarities of these elements as such. Each totality represents something new, something peculiar, which cannot be reduced to the elementary phenomena unless by default.
>
> (Kondratiev, 1926b: 63)

Although the author dismissed the possibility of a precise forecast, since the initial conditions were not known, and the causal structure and its regularity were only approximately understood, induction was presented as the sole method capable of increasing the level of understanding of historical data. 'Historico-comparative' and 'statistical' methods were therefore the two available forms of induction, and both were to be used (ibid.: 74). In this sense, Kondratiev and Schumpeter shared the same strategy: both valued the mathematical formulation of theories, both considered statistics to provide the essential inductive knowledge, both praised the historical explanation of each cycle and consequently both used and distrusted the mechanical representation of equilibrium.

The contemporary impact of Kondratiev's writings

It is frequently ignored that the majority of the economists involved at the core of the project for developing econometrics (Frisch, Tinbergen, Schumpeter) and simultaneously some other distinguished scholars involved in quantitative and historical research (Mitchell, Kuznets) had taken note of Kondratiev's work and either fully endorsed it, as in the case of these econometricians, or referred to it with varying degrees of enthusiasm.

Frisch endorsed Kondratiev's ideas in the spring of 1927, with his *Analysis of Statistical Time Series*. From the first pages, Frisch subscribed to Kondratiev's hypothesis of thirty to fifty years 'long time movements around which the business cycle is fluctuating', forming a 'major cycle' (Frisch, 1927a: 4). The source of the reference was the 1926 German version of the Kondratiev paper, but Frisch had also borrowed a manuscript by Kuznets (the book to be published in 1930), which included not only an account of the Russian debate but also statistical information giving credit to Kondratiev's theory. It is quite obvious that Schumpeter – soon to become a close friend of Frisch and also sharing this idea – developed his approach autonomously from Frisch: when their correspondence began, in August 1927, the Kondratiev hypothesis had already been publicly accepted by Schumpeter, who became the main Western defender of the theory of long cycles, dedicating a large part of his *Business Cycles* (1939) to it.

Tinbergen very soon and also independently defended the same hypothesis for quite similar reasons, since he had read Sam de Wolff's book and surveyed it in 1929, noticing that a parallel line of investigation was being carried out in Russia: 'Research on Long Waves is still in an initial stage, and it is mainly in Moscow that valuable work has been done on this subject' (Tinbergen, 1929: 858). It is likely that Wolff did not enjoy Tinbergen's review, and that the latter did not endorse the essential of Wolff's views. But, like Frisch, Tinbergen maintained some interest on the topic throughout his life, mentioning the long-wave hypothesis in his League of Nations report (Tinbergen 1939, I: 42) and in many other instances.

Wesley Mitchell was more prudent. In his 1927 book, he acknowledged the work by van Gelderen, de Wolff and Kondratiev (Mitchell, 1927: 227f.), and commented on their contributions, although the theme of the book was the business cycles. In a later work, Burns and Mitchell again discussed 'the most celebrated of the long cycle theories', 'the daring hypothesis that long waves in the wholesale prices are an organic part of a long cycle characteristic of capitalism' (Burns and Mitchell, 1946: 431–40).

As far as immediate reactions are concerned, Kuznets was the other important young researcher interested in Kondratiev's works at the time. As he was able to read Russian, Kuznets was the first to study Kondratiev's work in depth and the controversy among the Moscow staff of the Institute (Kuznets, 1930: 259f.), although he did not share Kondratiev or Schumpeter's views on the long-wave hypothesis and developed an alternative account of long-term historical evolution.

During the late 1930s, interest in Kondratiev's work apparently began to wane, and no new contributions were added to the research, with the major exception of Schumpeter's 1939 book. At the same time, other researchers into business cycles, such as Haberler for example, distanced themselves from any claim about Long Waves. In spite of this and basing himself on Spiethoff and Schumpeter, Haberler accepted that each long cycle had a historical physiognomy of its own and that a general theory was admissible, although he doubted if anyone could prove the existence of regular factors generating the fluctuations (Haberler, 1937: 308). Another scholar, Alvin Hansen, discussed Kondratiev's arguments compared to those of Spiethoff, Schumpeter and Mitchell. He found that the regularity of the three Long Waves was comparable to that of the shorter business cycles: 'as high a degree of periodicity has prevailed for these three waves as any which we find for the major business cycles' (Hansen, 1941: 29). It might be added that, later on, Hansen took a much more 'agnostic and even very sceptical position' on the same issue (Hansen, 1951: 56).

It is obvious that by the end of the 1930s and the beginning of the 1940s, Schumpeter had become the main proponent of the thesis, or at least the person most committed to its defence, since both Frisch and Tinbergen were isolated in Europe and surrounded by war, and decided not to devote their professional attention to this issue.[39] This impressive list of scientists, including some of the major figures from several decisive research traditions in the first third of the century – neoclassical economics, econometrics, quantitative economics and heterodox approaches – clearly proves that Kondratiev was not alone in recognising major structural changes and patterns of evolution in economic history.

Eugene Slutsky

Slutsky's life was also adventurous, although not as tragic as Kondratiev's. Because of his role in the student revolt against the Tzar, Slutsky was expelled from Kiev University, where he studied mathematics. He then changed to law, in which he obtained a doctorate, although he never abandoned his main interest, mathematics. Thanks to his persistence in mathematics, Slutsky was given a position as lecturer in Kiev and finally moved to Moscow in 1920. Slutsky taught at the University and survived what was to be the fate of many other scientists – he died from natural causes in 1948.

Attracted to Walrasian economics, his main papers would be enough to guarantee the author's place in the Hall of Fame of economics: on the theory of consumer behaviour (1915) and on the analysis of the statistical effects of the summation of random variables (1927). This last paper was prepared when Slutsky was already a top-ranking researcher at the Conjuncture Institute in Moscow. Kondratiev, the director of the Institute, had personally requested him to join the centre (Barnett, 1998: 120) and he did so in 1926. It is true that by 1926 the harsh internal controversy that divided Russian academics on Kondratiev's interpretation of cycles of several orders was already old news, and Kondratiev himself, although acknowledging Slutsky's comments for a paper he had published, seems to have missed the implication of the spurious cycles his colleague had detected. Anyway, Slutsky's discussion of cycles was certainly influenced by these trends in research, and he explicitly referred to different orders of cycles in the opening pages of his paper: long cycles and shorter business cycles should be considered and explained (Slutsky, [1927] 1937: 107).

The 1927 paper was first published in *Questions of Conjuncture*, the theoretical journal of the Institute and was rapidly circulated in econometric circles. By May or June 1927, Frisch had received the Russian version with a short summary in English and immediately reacted with enthusiasm – he was well placed to interpret the results, since his research was being conducted largely in parallel to that of his colleague. He praised Slutsky's work, since 'anyhow the [English] summary is sufficient to show the extreme importance of your problem' and it presented a 'very fruitful idea the following up of which seems highly promising'.[40]

Consequently, Frisch-the-editor asked a colleague to provide a translation. That colleague turned out to be Schultz, who was fluent in Russian and corrected the translation provided by one of his students, Eugene Prostov. In summer 1931, Frisch insisted that Schultz provide him with the translation; as it had not been received by the following February, he sent a letter arguing that he needed it for the book he was finishing on time series, 'and I want very much to know the whole content of Slutsky's article, not only the English summary I have been able to read'.[41] Schultz had forgotten about it but was able to send a copy of the translation in March 1932, announcing that he would also be sending it to Slutsky, for comments on the translation.[42] Frisch confirmed his intention to publish it in *Econometrica* and reassured Slutsky that the publication 'would not take years'; at worst, the paper would be included in the last issue of 1935.[43] Things turned out differently, since Slutsky took a long time answering and finally sent a bunch of corrections – Schultz wrote that it had cost him more than writing the original paper[44] and, due to a lack of space, the paper was only published in *Econometrica* in 1937.[45]

The paper circulated in one form or another before its publication in English and became a reference work for the econometricians dealing with business cycles. This is how Frisch evaluated the paper: 'As you know, I consider it one of the outstanding contributions in this field which has been made for the last years.' And he immediately argued that there was a link with his own research:

> This I will also express in a forthcoming book which I am writing on the subject of Cycle Creation. I believe I have solved in a fairly complete manner the problem which was still left in suspense after your paper, namely what sort of cycles will be created by an accumulation of an *arbitrarily given* weight system. And, furthermore, I have tried to build a synthesis between this mathematical statistical view-point and the view-point of macro-dynamic economic theory. It appears indeed that what dynamic economic theory gives us *is not* the time shape of standard curves with which the empirically observed time series are to be compared, but it gives us the *weight system* by which to perform the accumulation. The fundamental problem therefore rests on what is the harmonic nature of the time series produced by accumulation according to such a pre-assigned weight system. These are the questions which will be treated in my forthcoming book.
>
> (Frisch to Slutsky, 12 December 1934)

In a contemporary letter to Nelson, who assisted him in the editorial tasks of *Econometrica*, Frisch argued that the paper had prompted him to find a better solution:

> In connection with the publication of this [Slutsky] paper I may publish one by myself. I have recently found a practically complete solution to the problem which Slutsky raised in the paper. The nature of my approach is known by Davis with whom I had a long conversation on the subject when in Colorado. If we are very hard up for space in *Econometrica* during the year to come I shall probably publish my comments on this subject in my book on time series instead of in *Econometrica*.[46]

This comment suggests, and the attentive reader will not have missed this, that Slutsky's paper was important although incomplete, and that a dynamic economic theory was necessary for the explanation of the weight system and, one may infer, for the causal determination of the behaviour of the system. The same point was later made in the footnote that Frisch added to the paper when it was published in 1937:

> [the paper is] a classic in the field of time series analysis. While it does not give a complete theory of the time shape that is to be expected when a given linear operator is applied to a random (non-auto-correlated) series, it has given us a number of penetrating and suggestive ideas on this question.
>
> (footnote by Frisch to Slutsky, 1927: 105)

Indeed, the paper does not at all address the question of the explanation of the cycles. What it does do is present a new hypothesis about how spurious cycles could be generated in an abstract situation. The paper's point of departure was a criticism of Schuster's method of hidden periodicities in a series, since it supposed independence between observations, whereas 'the terms of an empirical series are not independent but correlated and at times correlated very closely' (Slutsky, 1927: 106). This is frequently ignored in the assessment of Slutsky's contribution and yet it is crucial. His 'basic problem' was then to answer the following question: 'is it possible that a definite structure of a connection between random fluctuations could form them into a system of more or less regular waves?' (ibid.) – and the answer was yes.

Slutsky discussed two conjectures: first, the undulatory character of the series is generated by a summation of random variables (ibid.: 114, 117), which is extendable to the moving average process, and second this process accounts for the regularity of the cycles (ibid.: 120). The first point was easily verified from an empirical exercise: lottery numbers were used as the raw data for distinctive series and the series resulting from averaging clearly exhibited a cyclical pattern, although the implications were not addressed, namely on the nature of the economic simile applied to these random drawings. On the contrary, Slutsky's example of a real series exhibiting random perturbations is that of the path of planets, if considered from a very long-term perspective (ibid.: 132). Otherwise, a single economic series is just used as an illustration: 125 data points from the lottery-based series by Slutsky are compared with the compressed data of the English business cycle between 1855 and 1877. But both the arbitrary choice of the convenient set of data and its compression (ibid.: 110) obscured the demonstration. In his letter to Frisch in July 1927, Slutsky drew his attention to the paper published that year by Yule, which considered a damped periodic vibration plus casual disturbances impinging on a mechanism. And that idea was discussed in his own paper (ibid.: 131–2). But, symptomatically, no explanation was given for the mechanism and its properties were not discussed.

A major point of the paper was that the summation of random variables would produce not only cycles, but also regular cycles. The author went so far as to admit that 'if we had a much shorter series [than the experimental one], such as a series offered by the ordinary statistics of economic life with its small number of waves, we should be tempted to consider the sequence as strictly periodic' (ibid.: 120). Yet, in the experimental series, changes of regimes were detected after a certain cycle structure set down. Here is the conclusion by Slutsky:

> The summation of random causes generates a cyclical series which tends to imitate for a number of cycles a harmonic series of a relatively small number of sine waves. After a more or less considerable number of periods every regime becomes disarranged, the transition to another regime occurring sometimes gradually, sometimes more or less abruptly, around certain critical points.
>
> (ibid.: 123)

But again, there is no discussion of the reasons for these surprising changes of regimes, a central preoccupation for all those studying the business cycle. Slutsky's paper provided one of the most impressive contributions to the discussion of stochasticity in economics, along with those of Yule and Hotelling – and his paper certainly had more impact than Yule's work, since it was published in *Econometrica* and closely related to the first discussions on the nature of random variables in economics.

Slutsky presented an original argument on the danger of smoothing the series by the use of moving averages and added a suggestive proof that random variables could generate regular cycles under some transitory regimes. This was highly influential in economics, although the message was rather sceptical. One of the economists influenced by this insight was Holbrook Working, a forerunner of the idea of representing stock prices as a random walk. His 1934 paper closely followed Slutsky's work and provided an interpretation for time series based on the concept of the averaging of random variables. Yet the paper did not seek to deliver a new theory of cycles.

A new tool was born; still, a new theory had to wait for other formulations. The time had come for a thorough discussion of mechanical models in economics. It was time for construction.

Part II

Construction

4 What counts is what can be counted

As soon as they landed on the seashores of economics, the positivist invaders proselytised among the natives: the creed of exhaustive quantification was supposed to define what was to be considered legitimate science, expelling metaphysics and sorcery. This drive towards quantification interpreted the mood of the time, marking the emergence of modern science. It could consequently claim the influence of prestigious ancestors who, like Bacon, ruled that what counts is what can be counted.[1] Burn sophistry, insisted David Hume:

> When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume – of divinity or scholar metaphysics, for instance – let us ask, *Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No*. Commit it then to the flames for it contains nothing but sophistry and illusion.
>
> (Hume, 1748: 165)

Quantification requires measurement and measurement assumes theory, the rule of laws: for such a complete knowledge of all forces acting in Nature, 'nothing will be uncertain, the future and the past will be opened to its regard' (Laplace, 1812: vi–vii). In doubt as to how to define the required legislation, the positivist movement in economics turned to the hard sciences, where success was guaranteed in that matter. Scientific and non-metaphysical economics was supposed to follow the lead of the pure sciences, first astronomy and then physics, in particular thermodynamics, and finally statistical mechanics, providing the mathematical guidance for the redescription of economics in statistical terms.

But victory proved to be a burden. As the victors soon discovered, this was not to be frowned upon: the price was simultaneously the contamination of the high values of positivism by the details of the analogy with the physicist's laboratory, on one hand, and by the fierce resistance movements by older alternative traditions, on the other hand. Both were problematical, the former on ontological and the latter on epistemological grounds.

Indeed, experiments following a rigid protocol in the laboratory create a replica of such a universe that has no intentional action, purposeful organisation,

strategy or choice, i.e. it is a universe lacking the distinguishing elements of humankind and social dynamics. This was addressed by the axiom of ontological equivalence between energy and utility, as established by the neoclassical revolutionaries, providing as a consequence the Hamiltonian framework for computation and defining the economy as the galaxy of utilitarian agents behaving like atoms. Consequently, in the name of rigorous quantification, positivism became enamoured of axiomatics and was therefore entangled with abstract derivations. By the first quarter of the twentieth century, economics claimed to be theoretically pure, although these aesthetic ecstasies implied recognition that it could not produce much in the way of statements on real economies. In any case, the analogy between the methods of pure economics and empirical research in the laboratory was done away with, unless the laboratory was metaphorised as a place of strictly thought out experiments, a claim a physicist would hardly accept.

Yet, this was not the main reason for concern, since the second problem, an epistemological one, was even more difficult to address. Modern economics, throughout the nineteenth century, had been concerned with the production and distribution of goods, considering them as the objects of interpretation of a rather complex capitalistic society. Therefore, quantification could not be isolated from the analysis of these markets and societies – and that was where the classical, institutional and historical traditions built their barracks, proposing theories and interpretations of social structure and change. Equilibrium economics could not accept these insights, since they challenged its canon.

It is beyond the scope of this book to discuss the extraordinary turn of events that imposed the theoretical ascendancy of that specific canon in economics: drawing on the prestige and rigour of physics, it was erected in accordance with a network of analogies that imposed techniques, concepts and constructs capturing the peculiarities of the social sciences that it was transforming. This process, nevertheless, had a dramatic consequence, the alienation of the very rigour required for empirical research as developed in physics. Consequently, when it came to the end of the first quarter of the twentieth century and the model of authoritative science had been established well beyond any reasonable doubt, it was too late: this pyrrhic victory neither provided a convenient canonical replacement for the surviving alternative paradigms nor instituted a developing empirical research programme. The heritage of these efforts was the acceptance of mathematics as the new language to refine and redefine economics.

That was when a number of young social engineers came onto the scene and changed the methods, scope and agenda of economics. Among them was the most surprising methodological positivist, a man eager to capture the methods and brightness of physics, but also keen to submit the results to the most anti-positivist and subjective standards of normative economics that imposed moral choices for the betterment of humankind, Ragnar Frisch.

Lectures on the foundations of econometrics

Frisch presented his own concept of economics and econometrics in two important instalments: the collections of his Yale (1930) and Paris (1933) lectures. They are both representative of a singular effort, that of creating a scientific standard, in parallel to his strenuous efforts to build the foundations of the econometric institutions – the Econometric Society, the Cowles Commission and *Econometrica*.

The first lecture at Yale was delivered on 13 February 1930, under the title 'What is meant by economic theory?' It corresponds to a five-page text, opening with a strong statement of a naturalist and positivist flavour: 'The meaning and place of theory within the whole body of thought of a science is practically the same whether we think of a natural science like astronomy or a social science like economics or any other natural or social science' (Frisch, 1930: 1). For Frisch, this similitude extends from the methods to the scope of scientific research, since we create a 'model world of our own' by 'rational induction', whether it be in geometry or in the description of an economic market (ibid.: 3).

The other lectures are another matter altogether. Frisch is no longer introducing his research and addressing the students in order to arouse their curiosity and motivation; he is outlining a road map for exact science – and the typewritten version of the lectures, 'A Dynamic Approach to Economic Theory', fills 256 pages. Again, we hear the positivist voice: astronomy is the example for other sciences, even if 'economic theory has not as yet reached the stage where its fundamental notions are derived from the technique of observations' (1930: 1). Consequently, 'the true theorist in economics has to become at the same time a statistician' (ibid.: 2).

In this way, Frisch proceeded to criticise the empirical approaches in economics, such as that of Mitchell and the institutionalist school, since they were 'dangerous' as they were not supported by theory and the facts did not speak for themselves (ibid.: 20). The example is their common concern with the interpretation of fluctuations: Mitchell is attacked for his 'naïve' analysis of business cycles and his 'cursory remarks' ignoring the interference phenomena among different orders of cycles, which provide the 'ultimate explanation of the ups and downs of business' (ibid.: 23–4, 53). A combative spirit inspired this criticism: Frisch considered the empirical approach to be reminiscent of the underdevelopment of theory and historical analysis to be a second best to analytical dynamics. As he put it,

> Historical dynamics is an attempt to analyze by dynamic principles those phenomena which have not yet been brought into rigorously formulated theoretical laws [formal models], and which must therefore be treated by a more or less vague or subjectively colored reasoning. From a theoretical point of view it is the analytical dynamics, which is dynamics in the restricted sense.
>
> (ibid.: 59)

Interference phenomena were, for Frisch, at the very core of that analytical dynamics usable for interpreting real events and processes. Anticipating one of his main themes for the decade, he presented a mechanical illustration of this interference, a chain of pendula representing different orders of time:

> The following is a mechanical illustration which represents the notion of time components of different orders. Suppose that we have a big pendulum, very long and with a very heavy mass concentrated at its end. To the lower end of this pendulum we attach a shorter and lighter pendulum. To the lower end of this pendulum we attach a still shorter pendulum with a still smaller mass, and so on. Now suppose that we put the whole system into movement. If the mass of each pendulum is small in comparison to the mass of the next higher pendulum, there will be very little influence from the motion of the lower pendulum on the motion of the higher. Therefore, each pendulum will oscillate approximately as if it were a free pendulum. Now let us focus the attention on the movement of the smallest pendulum at the bottom of the system. And let us trace this distance as a time series. This time series will contain a number of components, first a short component, due to the fluctuation of the smallest pendulum at the bottom of the system, then a component with a longer swing due to the presence of the next higher pendulum, and so on. Finally there will be a component with a very long swing due to the presence of the largest pendulum. In short there will be small waves superimposed on large waves. Graphically the most important difference between the nature of these time components will be that the high components will have a much smaller *curvature* than the low components (except in those particular points where the low components change curvatures). In this example we can attach a very concrete meaning to the notion of normal. The normal of the lowest pendulum is at any moment of time the position of the next higher pendulum, and so on.
>
> (ibid.: 49–50)

The swing of the chain of pendula became a dominant metaphor for the construction of Frisch's models of cycles and, indeed, for most of his work during the 'econometric decade' of his life, the 1930s (see Chapter 6). It designed the counterpart of the imaginary mechanical model for cycles, provided the necessary discrimination between variables, and defined causality as a fully describable and computable process, as the necessary and sufficient condition for the determination of an event in the laboratory. But immediately Frisch departed from positivism, for two main reasons. The first is that he attributed causality strictly to the domain of thought experiments, and this peculiarity is therefore a source of confusion. Causality only relates to the innuendoes that the construction of the model provides about reality and is not considered to be an inner property of reality itself:

> So far I have avoided the word 'cause', and for most purposes it would be perfectly possible to do without it altogether. This would have the advant-

age of avoiding much confusion and superficiality which has been introduced into the discussion of the logic of science by this stochastic term. However, it would probably be impossible to get rid of the language of this notion, so we had better take it up and see what it contains.

> [. . .] we think of a cause as something which exists in the *exterior world*. In my opinion this is fundamentally wrong.
>
> [. . .] As I see it the scientific (as distinguished from the scholastic), problem of causality is essentially a problem regarding our way of thinking, not a problem regarding the nature of the exterior world.
>
> [. . .] If any scientific answer is possible it must read: it [a cause] is such and such a way of thinking.
>
> (ibid.: 12–13)

The price for considering causality to be a property of the model was to define statistical estimation as a subjective approach. Consequently, Frisch provided his economist-made-statistician with a universe of confusing references: empirical 'frequency' should be distinguished from 'belief', and that from 'probability', which was just attributed to an abstract 'model world', a sort of personally endorsed notion of probability (ibid.: 16–17).

The second reason for Frisch's departure from positivism was his profound doubt as to the feasibility of empirical estimation of the structural relations causing the real economic processes. For one thing, equilibrium had been devoid of meaning ever since Frisch conceived it as the resting point of the chain of pendula – in the workings of the model it is the only situation that is so irrelevant that it dispenses with computation. Consequently, Frisch proposed a distinction between the concepts of 'assumption-equilibrium', corresponding to the fulfilment of the conditions of the theoretical system, and 'situation-equilibrium'. Yet 'the equilibrium-situation stands in the same relation to the notion of assumption-equilibrium as a rainstorm stands to meteorology' (ibid.: 72). A rainstorm can be described as it happens, but not exactly predicted in the long term and certainly not understood as the workings of a complete mechanical model. The equilibrium-situation was like a rainstorm: its model or mathematical description was unavailable. Frisch concluded from this deep scepticism that a new mathematics was necessary.

A new mathematics was also required because the available estimation methods were crude and insufficient. In particular, Frisch believed structural relations generally could not be estimated using the usual methods. His example was that of the computation of the area of a rectangle, $A=xy$, which, first, holds good whatever the values of x and y and, second, is a correct formula, not requiring the fulfilment of any other condition for validation; therefore, it is an example of a structural relation. Yet, in the cases of models for which the second condition is not met, we have just a confluent relation; if neither condition applies, we have an artificial relation that cannot be estimated from the empirical series. The regression work, Frisch feared, was full of pitfalls of this sort (ibid.: 80f.). During the 1930s, some of the most difficult debates in which Frisch engaged were related precisely to his suspicions of the wrong method of estimation based on the confusion between structural, confluent and artificial relations – these were the cases of the 'pitfalls debate' with Leontief, to be discussed further on, and the debate about Tinbergen's work on business cycles.

Hurried positivism from Yale to Paris

Frisch's attitude towards science was deeply paradoxical and even surprising. He championed exhaustive quantification and argued for the introduction of mechanical models as the convenient representation of reality. Furthermore, he equated the definition of a mechanical model with understanding. But he was not sure that this provided exact knowledge about reality, and furthermore felt that quantification and modelling were vulnerable to false induction and excessive inference. Consequently, a scientist ought to combine all the available methods – after all, what best describes Frisch's attitude in the 1930s was that he was in a hurry: the creation of the econometric movement responded to this sense of emergency.

This is why, in essence, Frisch departed from positivism and presented in his Yale lectures the strongest argument in favour of normative economics that one could imagine at that time: 'There are five types of mental activities in which the scientific worker has to engage: (1) description, (2) understanding, (3) prediction, (4) human purpose decision, (5) social engineering', since the theory must introduce 'ethical considerations' (ibid.: 3–4; 57). Nothing better defines Frisch – and some of his companions in the early econometric adventure – than to understand his motivations as those of a hurried social engineer.

Three years later, Frisch delivered an eight-lecture course on 'Problems and Methods of Econometrics' at the Institut Poincaré in Paris (March 1933). Departing from the 'philosophical foundations of econometrics' – the axiomatic method – Frisch presented some examples of static and dynamic models and his interpretation of the concept of 'force', and used his main topic of research at that time, economic cycles and perturbations, as a guideline for the analysis of time series. Finally, he presented his views on the meaning of social and mechanical laws and elements for a 'philosophy of chaos' (see Chapter 10).

The Yale themes were again omnipresent. Econometrics was defined as composed of two main parts: axiomatics and empirical research, the first being composed of the logical concepts to establish a quantitative theory of economic relations (Frisch, 1933e: 5). Physics, omnipresent as ever, was presented as a model for both: 'So, the spirit of econometrics has more affinity with the spirit reigning in the physical sciences and engineering than with that we can find in the philosophical sciences and other humanist sciences', since this spirit illustrated a 'science aiming at expressing the functioning of this vast economic mechanism that nowadays connects men' (ibid.: 1, 2).

In his first two lectures, Frisch discussed these definitions at great length. He addressed in particular a problem that had been haunting the analogy with physics since the very beginning of its use: the nature of human agency and the peculiarities of these psychologically and socially motivated individuals, as opposed to atoms. According to the lecturer, econometrics could address these peculiarities, since one can measure actions and psychological motivations by introducing parameters into the equations to take into consideration the possible choices; furthermore, empirical research is possible into motivations, in order to depict the curves of behaviour of human beings using 'logical tools' (Frisch, 1933d: 6).[2]

For Frisch, these logical tools were no different from all other resources of mathematical analysis since they were rigorous only at the level of abstract interpretation. In the same sense, he considered statics and dynamics not to be properties of the movements but modes of analysis, whereas phenomena could be described as stationary or evolving (Frisch, 1933g: 2). Consequently, Frisch concluded that, at this abstract level, mechanics was a good starting point for learning economics, as Divisia proposed, since it provided a toolbox for computation and the statistical treatment of evidence from reality (lecture four).

The last lectures were painstaking presentations of the current state of the art in statistics and econometrics, the definition of functions and analytical procedures. Again, as in his own doctoral dissertation, Frisch was mainly concerned with the inversion problem: how to determine the generating function of an empirical series, accepting errors of observation. This problem was reduced to the determination of the coefficients of a linear approximation by considering the plausible explanatory variables: in that sense, econometrics was just an explained extension of statistical inference from multiple regression.

But Frisch understood the potential inaccuracies of these methods as applied to the economic series. Not only had the confusion between structural and confluent relations pervaded all these approximations (Frisch, 1933h: 22), but also false hypotheses and spurious inference were possible (ibid.: 25). The problem was particularly relevant for the research into oscillations, since different dynamics were at stake: the typical regression could fail considering differences in phase among the distinctive movements or be impossible if linear dependence was created through the coordination of the respective frequencies (ibid.: 34–5).

A new mathematics was therefore needed and this was the theme for lecture seven on the subject of time series. Frisch criticised spectral analysis, argued for local methods for the determination of the structure of the movements, as opposed to the integration of the whole curve, and announced results that were to be published soon – which never appeared. Frisch was apparently very optimistic about his method by then, but his enthusiasm waned as time passed and no publishable results were obtained. This was not the only reason for his disillusion.

Cowles looking beyond mechanics

By 1933, the econometric movement was gaining momentum. Its main building blocks were in place: the Society and the European–US network, the journal, the Cowles Commission. It was able to provide an almanac of new methods, exhibit

a shining fabric of empirical results and consequently challenge some of the established fortresses in economic theory. Frisch preached across the ocean, from Yale and Minnesota to Oslo, London and Paris, spreading the word.

During this first part of the decade, few econometricians had both the standing and the knowledge to establish the canon. Two of those who did, Fisher and Schumpeter, were certainly unable to provide the necessary guidance. One was too outdated and the other too little cultivated in mathematics to have the necessary authoritative influence. Furthermore, the intricacies of the mechanical analogies were not accessible to everyone, since physics dealt with simpler concepts of equilibrium than economics – and some of the founders of econometrics intuited that these concepts simply had little relevance.[3] The responsibility of establishing the canon, fighting the error and clearing the ground for new developments, thus fell upon Frisch and a small number of the younger econometricians, who spared no effort in that sense.

Frisch concentrated his theoretical contributions on the determination of mechanical models for business cycles and dedicated his technical expertise to establishing adequate methods for the estimation of simultaneous equations. In that regard, the researchers were haunted by two problems: identification and estimation – not dealing with the identification problem from the structural to the reduced form of the system was the core of the criticism that Frisch levelled at Tinbergen about his later work on estimating cycle models (see Chapters 6 and 7).

Frisch defined as autonomous relations those exhibiting structural invariance, and as confluent those failing to exhibit that characteristic. His concept of theoretical inquiry was restricted to the definition of the model: the researcher should set the variables and the functional form of their relationship, define a hypothesis about the model's dynamics through time, including the response to shocks, and, finally, perform the statistical test. The model should obey the requirement of stability, and confluence analysis was considered to be necessary in order to avoid pitfalls in the estimation.

Defining the theoretical framework in order to establish an authoritative model was the common ground for all econometric efforts. Indeed, both computation and estimation required a manageable reference model, and the choice turned out to be the technology of the systems of simultaneous linear equations. This was a very comfortable option and in fact the only one possible at that time, since other mathematical strategies were inaccessible. Moreover, there was also a philosophical consolation for this selection, since the solution of a system of equations was simultaneously seen as a sophisticated explanation of the secrets of social organisation and a plausible method to decide on economic policy. For some of those who argued in favour of Keynesian policies and even for more stringent planning, general equilibrium as represented by the solution of a system of simultaneous equations was accepted as a trivially accessible mechanism for computation, and this was certainly the case with Frisch (more will be said about this in Chapter 7), but also with Lange, Tinbergen and Haavelmo, as well as some of the younger generation, such as Arrow. Models of general equilibrium and market socialism with planning were seen by many of these authors as equivalents.

But the very development of econometrics required substantiation of the theory for these statistical procedures: practitioners could not content themselves with laborious, yet approximate and, furthermore, doubtful results obtained from rough estimations made under the system of multiple regression. It was not simply sufficient to present this system of simultaneous equations that would describe how the jointly dependent variables are determined simultaneously; it was necessary to establish conditions for technical feasibility and for proxying reality. Consequently, the Cowles Commission took up the challenge to address the problem of identification – a condition for the use of simultaneous equations – and pursued its programme, under Roos and mainly under Marshack and then Koopmans, with a view to incorporating an explicit probabilistic approach.

The Cowles strategy was stabilised in the 1940s as: (1) the definition of a dynamically stable system of simultaneous equations supposedly describing economic behaviour, with a linear system representing the systematic part and assuming the systematic variables to be observable without error, as well as assuming discrete time changes and well-defined and measurable exogenous variables; (2) the further assumptions that predetermined variables are independent, structural equations are identifiable and disturbances are serially independent and normally distributed, with zero mean and finite and constant variance, with a nonsingular covariance matrix; and (3) the assumption of the existence of a reduced form, suitable for estimation (Christ, 1994: 46f.). The major achievements of the Cowles Commission were in providing the solution to the identification problem and consequently methods for estimating simultaneous equations.

The model of econometric modelling that was widely disseminated in the ensuing decades consisted of a properly specified equation with fixed regressors and a zero-mean, non-auto-correlated and non-correlated with the regressors and homoscedastic error term, allowing for *blue* estimators verifying the conditions of the theory (Darnell and Evans, 1990: 62). Assuming a convenient distribution of the random errors, in order for the parameters to be estimated, the research concentrated on stochastic disturbances to the equations and not on errors in the variables, which was the error of measurement conceivable under a strict mechanical model.

Whereas Frisch stuck to the task of building this mechanical model, the Cowles strategy proceeded to generalise the analogy with the stimulus-response laboratory experiments, which among other advantages provided clear epistemological grounds for defining causality: exogeneity and direction of causality are equivalent in this approach (Darnell and Evans, 1990: 116). In his 1944 manifesto on the probability approach, 'The Probability Approach to Econometrics', Haavelmo restated the hard core of the Cowles programme, characterising the economy as a set of autonomous and simultaneous causal relations with structural features captured by the parameters of these relations and supposing these relations to be essentially stochastic (Marchi and Gilbert, 1989: 5). The

application of the stimuli-response approach, valid under controlled laboratory conditions, nevertheless presented a problem, since the experimental nature of the data was not clearly established in the case of social data.

Faced with the dilemma of precluding the use of stochastic inference given the non-experimental nature of the data, the econometricians preferred to address the problem by supposing it did not exist: economic information was supposed to be generated by stochastic processes such as the drawing of lots from urns. Elegantly and openly expressed by Haavelmo, this hypothesis became the foundation for the probabilistic approach in econometrics. Not surprisingly, Frisch did not follow this strategy all the way through and chose another path: whereas Cowles surpassed mechanics and wandered in the new world of abstract representation, Frisch kept his faith in mechanics as the legitimate mode of thought, although not necessarily as a representation of reality itself.

No school without discipline

Whilst these intricacies were being defined, the econometric movement was establishing its scope and parameters – and, as a newborn school, much effort was dedicated to generating the example, the grades, the rewards and the punishments. In fact, looking back, one cannot help feeling a certain surprise when considering how the conversation among econometricians was constrained by these intense disciplinary efforts.

As the editor of *Econometrica*, and given his role as the most distinguished founder to understand the depths of mathematics, Frisch was very active in the 1930s in establishing patterns of research and rigour among econometricians.[4] This strenuous effort was consequential and the best-known example is the heated discussion that Frisch had with Leontief, who had published a technical paper on the estimation of the elasticities of demand and supply functions. Considering it was impossible to accept the premise of Leontief's method, namely the independence of the schedules of both functions, Frisch reacted with a violent attack, fearing that the example of adhocracy might spread. His paper ends with a vitriolic phrase: 'One cannot help feeling that the prestige of economics as a science must suffer when papers containing such mistakes and oversights as Dr. Leontief's last paper appear in a journal of high international standing' (Frisch, 1933b: 39). For Frisch, the correlation exercise was 'meaningless' and 'superfluous', given the unacceptable assumption of constant elasticity along the curve and over time (ibid.: 9).

Yet, there was another twist to Frisch's argument, since this is one of the first instalments of his mistrust of the estimation procedures, given the 'fictitious determinateness created by random errors' (ibid.: 7). Frisch championed

> a new type of significance analysis, which is not based on mechanical application of standard errors computed to some more or less plausible statistical mathematical formulae, but is based on a thoroughgoing comparative study

> of the various possible types of assumptions regarding the economic-theoretical set up of the consequences which these assumptions entail for the interpretation of the observational data.
>
> (ibid.: 39)

There is no indication of what this new type of significance analysis would be.

Leontief ignored this methodological question and responded in defence of his results, stating that the fulfilment of Frisch's requirements would be a 'veritable miracle' and, consequently, 'Professor Frisch is tilting at windmills' (Leontief, 1934: 357). Furthermore, he added that 'the assumption of independence is really the common foundation of all the statistical attempts at supply and demand analysis' and the 'fundamental postulate' of Marshallian theory (ibid.: 358). Consequently, the 'only danger' would be obtaining spurious correlation between independent shifts in supply and demand or being stopped by a lack of data, for which Leontief gave the example of the analysis of long waves of the Kondratiev type.

In a sense, this was an impossible conversation, since Leontief was pragmatically adapting to the limits of the available methods and Frisch was requiring the invention of identification procedures as the condition for any statement made out of the estimation. As both contenders proved unrepentant, the econometric milieu was agitated as people began to take sides and, as others intervened, it became clear that Frisch was essentially right. A rigorous econometric approach demanded a strategy for the generation of new methods that would not give in to the problems they addressed.

Marschak noted that Leontief tried to eliminate changes in the supply and demand curves as erratic, but this could not be the adequate technique, since the shifts may be correlated and the assumption of constant elasticities over time was therefore vulnerable and baseless (Marschak, 1934: 759, 761–3). In any case, Marschak still considered that Frisch's criticism could be verified if the causes influencing specific shifts were eliminated (ibid.: 763). Although this last response was of course a petition of principle and the problem of identification could not be solved through the alienation of the variables explaining the shifts, the author was trying to perform a difficult task.[5] This episode was one of the first defining moments of the identification problem. The production of alternative techniques proved to be an important and laborious task for the Cowles Commission.[6]

There was still another twist to this debate – but it was not public, since it developed in private correspondence between some of the econometric mentors. Worried about the violence of Frisch's disciplinary attack, Schumpeter intervened as a middleman in order to deliver a personal letter of excuse to Leontief and to appease the conflict. Although he never took sides in the episode of the heated exchange, Schumpeter wrote to Frisch about his unpleasant manner of treatment, which some colleagues were complaining about.[7] Frisch reacted in a very amicable way ('You have proved to be such a friend'),[8] although emphasising his own argument: 'I wish it [the last sentence on Leontief: 'One cannot help

feeling that the prestige of economics as a science must suffer when papers containing such mistakes and oversights as Dr. Leontief's last paper appear in a journal of high international standing'] had not been written', but, if Schumpeter does not understand that Leontief is wrong, 'it would indeed be a hard blow to my belief that there exists in this world at least some possibility to settle scientific questions – at least mathematical ones – in an objective way'.[9] On the same date, Frisch agreed to Schumpeter's demand for an apologetic letter to an anonymous correspondent, in reference to his previous eventual mistreatment of that person. Schumpeter acknowledged the generous answer and sent the letter to the anonymous correspondent.[10] Although it was never mentioned that the letter was addressed to Leontief, it is obvious that both Schumpeter and Frisch knew he was the anonymous and offended correspondent. Theoretical discipline was matched with common-sense discipline.

The epilogue to this discussion and diplomatic activity came much later, in 1970, when Lundberg asked Frisch for advice on the candidates for the next Nobel prizes, and Frisch suggested, in this order, Leontief for his Input–Output contributions, Gunnar Myrdal for his critique of the method of economics and Richard Stone for the creation of national accounting.[11] Although given to creating controversy, Frisch was still able to do justice to his previous adversary, who was nonetheless an impressive contributor to the construction of modern economics.

Looking through to the other side of the mirror

Throughout the 1930s, the econometricians discussed their perplexities and doubts quite openly. At the dawn of the movement, in 1931, Divisia sent Frisch his thoughts on the new mathematics needed to deal with the 'absence of economic equilibrium', as a possible topic for the Lausanne conference. At that time and under the influence of his friend, the mathematician Le Corbeiller, Divisia insisted on the promises of a new branch of mathematics suitable for understanding cycles and the study of oscillations of relaxation,

> an oscillation obtained out of a series of ruptures in unstable equilibria; I feel like saying a couple of words about my present ideas on the possibility of absence of economic equilibrium; do you think this topic deserves a different paper from what could be presented by one of our mathematician friends?.[12]

Le Corbeiller presented his paper, originally prepared for a lecture at the Paris Conservatoire des Arts et Métiers, at the Lausanne conference. It was published later on in *Econometrica*, as was a paper by Hamburger on nonlinear relations. Frisch, who was very enthusiastic about the promises of these contributions, rapidly understood the difficulty of the matter, after perusing Van der Pol's 'Theory of Oscillations' and the material by the 'mathematician friends':

> I think it would be exceedingly interesting if Corbeiller could tell us something more about the oscillations he spoke about at the Lausanne meeting. This time I think he ought to go into the matter with more detail, not being afraid of making the paper a mathematical and technical one. If he could indicate those aspects of the problem that would be of importance so far as the statistical treatment of our economic problems is concerned – so much the better.
>
> (Frisch to Divisia, 11 June 1932)

Unfortunately, what came out of all this effort was next to nothing: Le Corbeiller did not follow the workings of the Society for long and no one was able to follow his mathematics. On the other hand, some thought, albeit for different reasons, that difficult mathematical exercises, language and demonstrations should not be the *motum* of the Society. Amoroso was one of these:

> The mathematic economist must take in the economic field the mathematician's *forma mentis* and systems; but mathematics must appear as little as possible in its exposition. Economics must not be for him a mere pretext for mathematical virtuousness, who are absolutely stranger to the very character of the economic facts [sic].

And Amoroso concluded:

> The application of mathematics to the study of these facts has – in my mind – the only aim of simplifying and clarifying, and therefore, the more simple and general the mathematical means used will be, the better the same aim will be attained. I think that the use and application of particular, difficult and hard known mathematical theories ought to be a priori condemned, convinced as I am that they would lead us to absurd and vain abstractions.[13]

Frisch was not prepared to concede:

> If you allow me to say so I don't think it is possible either to you or to me or anybody else today to predict exactly what kind of mathematics will in the end prove useful for our purpose. In particular I think it would be a very unwise policy for the Econometric Society to condemn a priori certain kinds of mathematics on the mere ground that they are *difficult*. Freedom of thought was always in the planning and organization of the Econometric Society conceived of as its key-note.

Nevertheless, Frisch accepted that: '[It is necessary to avoid] that empty display of mathematical manipulations devoid of economic significance which we have sometimes witnessed in the past'.[14]

But this was not all. The choice was not between a mathematically trained econometric movement as opposed to a narrative theoretical economics as in the past – it was between useful mathematics as opposed to mere exercises of logic. For the reasons presented below, namely his attachment to econometrics as a tool for social engineering, Frisch greatly preferred the difficult combination of highly rigorous computation and models applicable to the determination of economic progress. Consequently, the editor of *Econometrica* did not give up in his search for new approaches in mathematics. There was an essential reason for this quest, and that was precisely the limitation of the mechanical models, which did not allow for the capturing of human agency. In 1934, Frisch asked Amoroso, one of the econometric founders who had voiced his suspicion of mechanics as a model, for a paper on 'a representation of dynamic economics according to a model that allows us to take account of the willpower of men and its influence on economic facts'.[15]

In spite of his own divergence with mechanical representations, Amoroso could not contribute to that model on the 'power of men'. Neither could Divisia, in spite of his attention to alternative modes for the representation of oscillations and structural change. Ever restless, Frisch did not give up: he came back repeatedly to his intuition about the need for other mathematical tools and later he proposed to Pieter de Wolff the co-authorship of a book on nonlinear dynamics, but the war also prevented the realisation of this project (see Chapter 10).

This quest for a new generation of mathematical models was motivated by two very powerful reasons. For one thing, Frisch was suspicious of the stochastic approach (see Chapter 8). As he had devoted a great deal of his time and intellectual resources to computation, by the end of the 1930s, Frisch had become more and more pessimistic and even agnostic about the possibility of structural estimation and the use of probabilities in economics. Consequently, he increasingly favoured greater recourse to interviewing in order to compute the parameters, as well as recourse to simulation in order to provide criteria for choosing between alternatives. Yet, this departed from most of the work being done under the auspices of the Cowles Commission. As demonstrated in the episode of his quarrel with Leontief, Frisch greatly preferred searching for adequate although unknown solutions for difficult problems to having to adapt to proven inadequate methods.

The second reason for this search for a new mathematics and in particular for nonlinear alternatives emerged from Frisch's conception of dynamic movements in economics. In one of his 1933 Poincaré lectures, Frisch assumed the difficulty of modelling historical series and discussed some common simplifications for statistical purposes, such as considering the secular trend to be a constant linear growth. Although he claimed this method was robust in relation to alternative assumptions on growth, the very same lecture presented a counter argument, since it was argued that long-term movements are cyclical but not regular. As an application, Frisch detected the frequencies of cycles of nine, twenty and fifty years, as Kondratiev had done, and even a supra-secular oscillation in a series of English prices from 1780 to 1930 (Frisch, 1933h: 37).

This conclusion echoed his previous research into long-term dynamics. Both in his 1927 paper distributed in the US, and then again in 1932 in a university radio lecture (April and May 1932), Frisch endorsed the conclusions by Kondratiev and presented an explanation for those long cycles, insisting on the political determination of the major structural changes:

> The long wave of about 55 years is a combined biological and economic phenomenon that is closely related to the incidence of wars. It is its relation to the biological matters which explains the great regularity, and the connection to wars that explains the force and typical shape of the movement. . . . Wars are not something arising out of thin air, but follow by the force of necessity during a certain phase of the long wave . . . the last part of the long upswing. During this upswing there accumulates, so to speak, a stock of physical force and wealth. The increased physical force and the increase in economic well-being . . . is released through war. Through the war, the physical force is tapped. Through the war some real productive values are destroyed, but more importantly, people after the war become carried away by a deflation psychosis that stifles economic activity. . . . This lasts for a while until the biological and physical consequences of the expansion are defeated. The length of the downswing . . . is partly determined by the time needed for a new generation to grow up.
>
> (Frisch, 1932b: 112–13)

Frisch did not discuss the hypothesis in detail in his scientific and mathematical texts. Yet, Frisch did try to prove that some of his models of cycles could generate Long Waves for certain ranges of parameters, and considered this to be an indication of the likelihood of the models. Moreover, he insisted again and again on his interpretation of the depression of the thirties and the dangers of war by means of the Long Wave argument. Later on, in the pamphlet including these radio lectures and dedicated to the discussion of the conjuncture, Frisch illustrated his argument with a long series of wheat prices for 1201–1800 from a nineteenth-century book by D'Avenel. Frisch looked at the years 1300–1800 in particular, used a ten years moving average much as Kondratiev did, and detected large persistent movements, which he interpreted as indicating long cycles of prices for the whole history as described by the graph.[16] Since this explanation proved very effective for understanding the great ravages of the thirties, at least as far as Frisch concluded, he maintained it throughout his life.

Andvig, who discussed this point of view in detail, considered it a 'rather strange, almost mystical' explanation based upon 'far-fetched notions'[17] and consequently alien to Kondratiev's methods and hypotheses. But an alternative interpretation is possible, and this book endorses it. In 1932 and 1933, after a decade of high unemployment and hyper-inflation and as Hitler rose to power in Germany, Frisch and many other young scientists felt an urgency to understand the nature of the depression and to propose alternative choices for the economies. The Kondratiev hypothesis on long-term processes of change and, in Frisch's version, the power of war to influence these major movements, fitted in with the apocalypse they were living through. Consequently, these men looked

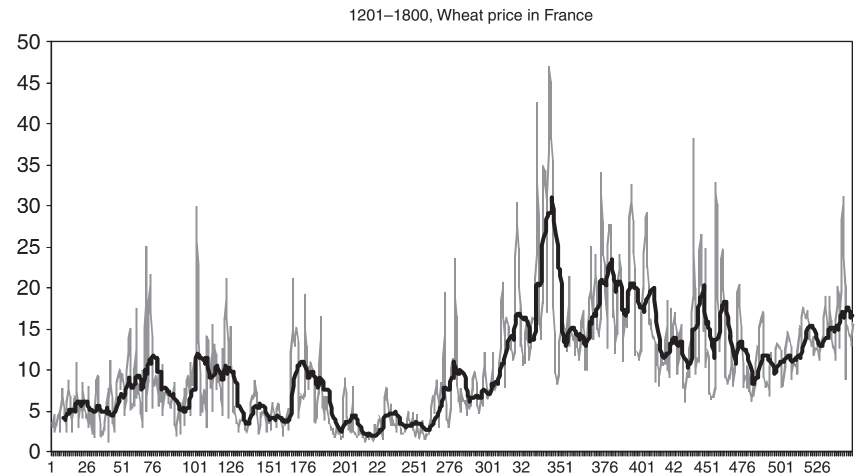

Figure 4.1 D'Avenel series for the price of wheat for six centuries, represented according to Frisch's method.

carefully at these cycles in order to explain the world they were living in: this was the case with Frisch, but also with Tinbergen, Schumpeter and many other econometricians. But, again, a new kind of mathematics was necessary, in order to understand this dynamics of structural change. The precise certainties of mechanics were both too rigid and too monumental for this purpose.

This view of some of the dramatic reasons for economic dynamics was typical of a rather extraordinary scientist. Frisch was an activist methodological positivist, but one of a kind: he argued powerfully in favour of normative economics, although he contradictorily believed science could not establish definitive assertions on reality, and furthermore that causality was a figment of imagination and interpretation. Indeed, it was because he was not an orthodox positivist that he so strongly supported the mechanical approach and the effort to mimic physics, given the urgency of the action science could and ought to deliver in a world moving ever closer to war and general unemployment – it was normative economics that imposed the choice of the methods which positivist economics was preaching. And, when positivism turned strangely into axiomatics, Frisch remained at his post, unrepentantly arguing for economics as a science of social action.

As a methodological reductionist, Frisch was consequently confronted with the reconsideration of the very subject of economics, being forced to weight the alternative: are humans as particles or as intentional and purposeful beings? In other words, the crux of the matter became, for a mathematical economist whose engagement in the mechanical approach was motivated by the production of a guided economy for the well being of his fellows, to accept or to reject their reduction to similes of particles.

5 Particles or humans?

Paradoxes of mechanics

The introduction and development of neoclassical economics has been studied from the point of view of the widespread incorporation of metaphors from physics and the elevation of mathematics as its language. The metaphors drawn from nineteenth-century energetics and based on the First Law of Thermodynamics[1] were decisive for the formal extension of the general equilibrium models and for the acceptance of the heuristic relevance of the maximisation principle, as well as all its paraphernalia of concepts and postulates (Mirowski, 1989a). Yet, the impact of statistical mechanics and the following combination of neoclassical economics and the new generation of econometric models, research and institutions, which are the subject of this book and which provided the decisive step towards the contemporarily dominant form of economic theorising, has not been studied in the same depth.

This chapter provides complementary information and discussion on that period, arguing that the powerful drive towards the incorporation of a new wave of mechanical analogies met with considerable resistance from some of the more important members of the econometric group, and that the implications of these analogies were discussed in lively, if not heated, fashion. As the arguments crossed borders of national belonging, education and professional training, the canon was slowly defined, amidst vigorous debate and intense conflict, on the nature of economics and its identity in the concert of sciences.

The charm of mechanics: passive analogy or active guidance?

The polemics about the relationship between economics and the established model of science, physics, is naturally much older than the foundation of econometrics. Its first instalment was provided by the generation of the neoclassical revolutionaries at the beginning of the last quarter of the nineteenth century: they presented general equilibrium and its computation procedures as being justified by the analogy between the movement of particles in a closed space and the behaviour of economic agents pursuing the maximisation of their utilities. It is known that this contentious programme became an immense success: it provided the epitome for the new concepts and the manual of instructions for their operational use. Furthermore, it responded to the essential requirement of

demonstrating processes and results, creating a glamorous aura for those initiated in the trade.

The early twentieth-century generation of young economists who aimed at the reconfiguration of their *métier* according to the standards of advanced mathematics were educated in obedience to this paradigm. In order to establish an empirical counterpart for the prevailing general theories, they turned again to physics and furnished economics with hypotheses, methods and verification procedures derived from that generous source of inspiration. But, unlike the previous generation, many of these economists, or even physicists turned economists, were sceptical in relation to the immediate translation of mechanical concepts into the domain of their science.

Ragnar Frisch was a major player in this debate. In one of his first papers, ‘A Problem of Pure Economics’, he addressed the limits of the analogy from mechanics into economics:

> There are numerous analogies between rational mechanics and pure economics. Thus the vector *u* [utility] plays a role in pure economics analogous to universal attraction in rational mechanics. But there are also essential differences. Concrete economic phenomena are too complex for it to be possible on the basis of *a priori* considerations to determine precisely the forms of functions given by the components of *u*. There is no universal law of economic attraction as there is a universal law of gravitation.
>
> (Frisch, 1926b: 40)

The lack of the authoritative framework of a generally accepted universal law naturally established a major difference between the two sciences, although many did not intuit the impact of its absence. In fact, it implies that statistical analysis and empirical research in economics do not relate to a clearly defined mathematical description of Nature that is, beyond any reasonable doubt, the analogue for Newton’s laws. Frisch struggled against this difficulty throughout his life, pursuing the definition of adequate mechanical-type laws and the right models, in spite of his own suspicions about their inaccessibility.

At his presentation to the round table on the ‘Present Status and Future Prospect of Quantitative Economics’, at the joint meeting of the American Economic Association and the American Statistical Association held in Washington, in December 1927, Frisch adopted the explanation that economists typically used to exhibit at that time. Economics should mimic physics in order to gain scientific *gravitas*, although this calls for a rather abstract and, if necessary, unpopular procedure, the adoption of axiomatics:

> It is abstract, but neither in the sense of a logic game nor in the sense of metaphysical verbiage, of which we have had some in economics, at times. Axiomatic economics will construct its quantitative notions in the same way as theoretical physics has constructed its quantitative notions.
>
> (Frisch, 1927b: 3)

The author then proceeds to explain that an example of such an axiomatic enrichment of economics was the incorporation of the definition of force, taken from mechanics.

Frisch accepted that a complete analogy was unwelcome:

> I want to make clear that this does not of course involve a complete parallelism in the scientific methods in the two fields. We are here only concerned with a single point where the economist has something to learn from the physicist so far as the logic of the science is concerned.
>
> (ibid.)

As in his 1926 paper, the argument was for learning but not for mimicking mechanics. A couple of years later, in a lecture given in 1928 or 1929, Frisch reinforced the argument for following the teaching of physics, since 'economics as a science is at the same stage as physics was at the beginning of the 17th century' (quoted by Andvig, 1986: 48).[2]

Learning may lead very far indeed. In a 1926 paper strongly influenced by Irving Fisher's 1892 dissertation, and published in Norway under the title of 'Quantitative Formulation of the Laws of Economic Theory', Frisch retains Fisher's redefinition of 'force' as the analogue for marginal utility (Frisch, 1926a). In 1929, he again recapitulates the concept of force (Frisch, 1929: 393) and justifies the 'borrowing from other disciplines', such as mechanics (ibid.: 391). Shortly afterwards, these concepts extracted from mechanics were widely used: in a lecture under the title of 'Monopoly-Polipoly, The Concept of Force in the Economy', the list of mechanical references is exhaustive – 'market forces', 'force of attraction' of the parameters, 'basin of attraction' and 'field of attraction' (Frisch, 1933d: 23, 32–3, 35). Furthermore, this was essential for defining equilibrium: 'In effect, the introduction of the vector has permitted us to pose the problem in terms of *force*, and we have considered an equilibrium determined by that force' (ibid.: 36). In this text, presented while he was preparing the seminal 'Propagation Problems and Impulse Problems', the substance of his investigation is presented as being derived from the concept of friction:

> We shall there again meet the concept of friction, and we will have to discuss this fundamental dynamic problem: what is the source of energy which maintains these oscillations and which keeps economic life in a state of perpetual flux where static equilibria are never established?
>
> (ibid.: 36)

Time and time again, these concepts were called into play by the author. In his 'Prolegomena', written in 1949, Frisch discussed the notions of 'pressure' and 'gravitational field' (Frisch, 1949a: 140). In 1965, he asked for much more:

> The fundamental concepts of product, production, factor, marginal productivity, etc., apply, in their abstract sense, not only to the problems of a concern producing and selling goods, but to practically every conceivable sphere of human activity: to political action, to social reforms, to the speeding up of economic growth in an underdeveloped country, to improvement of breeding stock, to games of chance, etc. *In many cases, it would suffice to change the names of the objects under consideration to return to an analytical formula common to all.*
>
> (Frisch, 1965: vi, my italics)

The implication is impressive: it suffices to rename the concept in order to apply it in every scientific domain. This was not only possible but most welcome, according to Frisch, since there is a deep explanation for this plasticity of concepts and for their general analytical capacity – human nature itself. The intended explanation is psychological or physiological:

> This [the applicability of the concepts in different domains], clearly, is due to the fact that the fundamental concepts of the theory of production which I have just mentioned – product, production factor, marginal productivity, etc – are not derivatives of concrete objects, but rather spring from the peculiar way in which the human brain functions.
>
> (ibid.)

Frisch considered that this functioning of the brain would induce a mode of thinking common to different fields of knowledge, implying results in mechanics akin to those of other sciences:

> In all likelihood this was probably not only due to similarities between the concerns of the sciences in question. More likely, this distinction between statics and dynamics is tied up with something which is characteristic of the very way people think. It is this characteristic quality of human thought that I shall attempt to subject to closer analysis.
>
> (Frisch, 1929: 391)[3]

At one of his Poincaré lectures (March 1933), Frisch once again argued that these notions were imposed by the similar application of intelligence to different fields:

> The notions of statics and dynamics are taken from mechanics. From there they were transmitted to several domains, as that of economics. The reasons for this transmission may be found, I believe, not only in the fact that the objects in different sciences are similar. The reason is, first of all, that the distinction between statics and dynamics is connected to something that is the very characteristic way of thinking of men.
>
> (Frisch, 1933e: 1–2)

Although he never attempted such closer analysis of this essential human trait, Frisch felt at ease in using the mechanical concepts rather freely and intuiting that, given these brain functions, such use could at least provide adequate and forceful pedagogical means for communication, i.e. for research and explanation. Yet he recognised that many central concepts of the social sciences had no analogues in mechanics, and he provided some examples of this: legal institutions, technology, population, specialisation of labour, the whole institutional setting which requires recourse to dynamic analysis and to 'historical dynamics' (Frisch, 1929: 400). There was, for Frisch, one further reason for the defence and support of this metaphorical transfer that was strictly dependent on the scientific function of economics. This had been transparent since his early career and was openly advertised when Frisch delivered the inaugural lecture for his chair, in 1932:

> One of the most important aspects of the development of economics in an experimental direction has been quantification of economic concepts, that is to say the endeavour to make these concepts measurable. It is not necessary to recall what quantitative formulation of concepts and laws has meant to the natural sciences. . . . Quantitative formulation of laws and concepts is very nearly as important in economics. This can be seen most clearly if we consider the final goal of economic theory, which is to clarify the interrelationship between the various factors and to do so in such a way as to secure a basis for evaluating what practical measures are most suitable to promote socio-economic aims.
>
> (Frisch, 1932a: 1–2)

The very aim of economics, evaluating alternative courses of action in order to promote socio-economic objectives, and in that sense transforming itself into an experimental science, implied and required quantification, so that modern economics should follow the example of the natural sciences, which had long been leading the way. Quantification, the fundamental condition for economics, was for Frisch inseparable from the incorporation of metaphors imported from physics, for the simple reason that quantification and experimental methods were far more developed and sophisticated in that domain.

Quantification and experimental methods were not completely satisfactory, however. The very reason for their transfer from mechanics to economics suggested the need for great care, as far as Frisch was concerned: since these concepts and methods were necessary for economic understanding and attenuating the effects of cycles and crises – this was passionately written and discussed at the end of the 1920s and early 1930s, before and after the Great Depression and with the outbreak of a new world war imminent – rigour also required an understanding of the great differences between economics and mechanics. An economic interpretation is always necessary, Frisch emphasised, since mechanical applications may lead to spurious results and may deceive the statistician.

When Fréchet, from the Poincaré Institute in Paris, consulted Frisch on the interpretation of a correlation coefficient between two curves describing cycles, he was met with words of prudence:

> I do not think one can use any mechanical rule of interpreting the magnitude of the correlation coefficient as the one you quote. Whether a correlation coefficient shall be considered as significantly different from zero or not depends essentially upon the whole setting of the problem. Suppose for instance that we have two time series that have the shape of sine curves with the same period but a slight difference in phase. If the observations were absolutely correct, the slightest amount of difference in phase would be indicated by the correlation coefficient being different from zero. There even is a very definite connection between the size of the phase difference and the size of the correlation coefficient.[4]

A mechanical interpretation was still possible for this maladjustment, but no mechanical use of mechanical concepts could be economic in their economic interpretation.[5]

Although Ragnar Frisch's formal dynamics is derived from physics, and he was a staunch defender of the strategy for exhaustive quantification, he felt that this quantitative science was all about complexity: the first editorial of *Econometrica* emphasises that 'Economic life is a complex network of relationships operating in all directions' (Frisch, 1933a). Given no direct and simple cause–effect relations, given too many degrees of freedom, quantification and experimental methods should be submitted to discrimination and reconfiguration. In that sense, in a letter to Mitchell, who was of course guilty of suspecting the non-critical use of mathematical formulations, Frisch complained about simplistic technical derivations: 'I have frequently noticed the deplorable fact that some investigators seem to give up thinking when they get a chance to apply mechanically one mathematical formula'.[6] Yet, he thought the mechanical models were necessary and even indispensable for reasoning: 'We all have our peculiar way of working, and I for one, never understand a complicated economic relationship until I have succeeded in translating it either into a graphical representation or into some mechanical analogy'.[7]

Although reluctant to accept too close an analogy with physics, Frisch always considered that it provided a role model for sciences and in that sense he can be considered a typical member of this generation of young mathematical economists from the first half of the twentieth century. Moreover, he excelled in the transfer of methods and concepts from mechanics to economics. Yet Frisch was aware of the epistemic differences and of the dangers of literal translation, in particular in the crucial instances that defined the whole specificity of the approach of economics to society: for example, he condemned Walras's use of the concept of equilibrium as a 'complete misunderstanding' (Bjerkholt, 1995: xxx), and he praised Wicksell's notion of the normal rate of interest, defined as the instantaneous equilibrium obtained after a modification in the system, since it was different from the 'mechanical notion of a stationary state' (ibid.: xxix).

It was certainly not easy to campaign, on the one hand, for learning and copying from physics and, on the other hand, for the understanding of the peculiarities of economics as opposed to mechanics. This paradoxical confrontation highlighted other differences in the econometric generation, and the difficulties became quite apparent in several editorial episodes in the management of *Econometrica*, the subject of the next section in this chapter. The characters are the editors of the journal, corresponding between the US and across Europe, and the plot is set in the early 1930s.

The Creedy episode

On 20 March 1934, the second year of publication of *Econometrica*, Harold Hotelling suggested that the journal would be the appropriate destination for a paper he had received from one of the members of the Econometric Society, Frederick Creedy, a professor at Lehigh University, Bethlehem, Pennsylvania. Rather unconventionally, Hotelling wrote to the author and simultaneously sent a copy of his letter to Frisch, the editor of *Econometrica*. On 1 April Frisch answered Hotelling, stating that 'I am glad you suggested to him to present this paper to *Econometrica*. I have just received the manuscript and find it highly interesting. It will appear in one of the early issues'.[8] Very shortly afterwards – indeed, four days later, on 5 April and definitely not to be compared with current delays in the same business – Frisch informed Creedy of the acceptance of his paper 'On Equations of Motion of Business Activity', suggesting only minor changes. It is quite obvious that he considered the paper to be in line with his own major preoccupations and his project for the development of econometrics as a body of formal research and modelling according to the standards of physics.

Yet the paper (published in *Econometrica*, 2, 1934: 363–80, and shortly to be discussed below) was not well accepted by other econometricians. On 26 September Tinbergen told Frisch that he did not rate the paper very highly, and that in general he was quite suspicious of mechanical analogies:

> My opinion of Creedy's paper is that I am rather sceptical on its value; so I am in general concerning analogies between physics and economics. I never saw one that did not, more or less, force economic phenomena into a form that is not characteristic to them. I still must see the first important result from these analogies. But I may be wrong; and as there may be suggestions in this treatment, I do not quite make objections to accepting it for *Econometrica*.
>
> (Tinbergen to Frisch, 26 September 1934)

The final phrase was enough for Frisch and, for the time being, he was content simply to register the attitude of his close friend and collaborator: 'I notice that you are somewhat sceptical about Creedy's paper, but that you do not quite make objections to accepting it for *Econometrica*'.[9] Nothing more was written on the subject in that letter.

But this difference of opinion was rapidly challenged again by a second paper submitted by Creedy, which was indeed a second instalment of the same project – to base economics on Newtonian dynamics. Probably due to his previous experience, Frisch was much more prudent in his reaction to the paper, and indicated to the author that a referee [Tinbergen] was 'not vastly enthusiastic'.[10] But the text was not explicitly rejected: Frisch merely limited himself to suggesting that Creedy should find some means of partially financing its publication in *Econometrica*, through a grant from some Canadian university.

At the same time, in view of Tinbergen's previous remarks, Frisch sent the paper to two other influential members of the Econometric Society, Le Corbeiller and Charles Roos, asking for their comments. Le Corbeiller was a French physicist who had participated in the first meeting of the Society, in Lausanne, in 1931, where he presented a paper on relaxation oscillations, published in the first issue of *Econometrica*. Apparently, he later on lost interest in the workings of the Society, but in 1934 he was certainly considered to be one of its authorities in mathematics and in particular in physical analogies. Roos was, of course, one of the founders of the Econometric Society and at that time one of the main driving forces behind its development.

Although the first elements in this correspondence with Le Corbeiller about the paper are not available, there is indirect evidence that Frisch received a letter from his colleague on 10 November praising Creedy's paper. The answer from Frisch to Le Corbeiller, dated 19 November states that:

> I am glad you find Creedy's paper of interest. This is some encouragement to me because from some other important member of our Society [Tinbergen] I have had the reaction that mechanical analogies are not very useful for application to economics. On the other hand, you know that Divisia is a great believer in the usefulness of mechanical analogies. If I remember correctly Divisia even said once that it is more important to teach the young theorists *mechanics* than to teach them pure mathematics.
>
> (Frisch to Le Corbeiller, 19 November 1934)

The reference to Divisia was important in this context, not only because he was closely associated with Le Corbeiller, but also because Divisia, then the vice-president of the Society, was one of the voices arguing for the widespread incorporation of analogies with physics.

But the reaction of the second referee, Roos, was quite the opposite and, since he was much more closely involved in the management of the Society – he was its secretary at that time – and much more concerned with economics proper, his opinion was certainly more influential. Frisch had sent him the paper on 16 December indicating that Tinbergen was not enthusiastic about the publication but that, 'On the other hand, my impression is that Creedy is a man who knows what he is talking about'.[11]

Unlike the previous referees and commentators, Roos took five months to answer. His reply challenged Frisch's opinion:

> I am afraid I share Tinbergen's view on the inadvisability of publishing Creedy's paper in *Econometrica*. Indeed all the paper does is to set up a series of analogies between economic and physical situations. One can do this *ad infinitum* without getting anywhere in particular. In general, I feel strongly that we should not encourage mathematical exercises of this nature.
> (Roos to Frisch, 6 May 1935)

And the letter goes on with further critical remarks, concluding: 'Finally, the paper is decidedly wordy. You might tell Creedy that he should explore the possibility of writing a monograph which would have as its purpose a determination of useful theorems resulting from his analogies' (ibid.).

Certainly surprised by the long period spent waiting for a decision on publication, compared to the rapidity with which the first part of his essay had been accepted, Creedy wrote to Frisch on 24 May 1935, explaining the purpose of his paper: to apply the Principle of Least Action and to discuss the application of Gibbs's statistical mechanics to economics. This was certainly much more ambitious than the first article on Newtonian mechanics, and the author hoped it could be published in *Econometrica*, although he had not found any complementary funding as suggested by Frisch eight months earlier.

At the end of June 1935, Frisch finally answered Creedy quoting an uncited referee [Roos] who rejected the paper and stated that 'Indeed all the paper does is to set up a series of analogies between economic and physical situations. One can do this *ad infinitum* without getting anywhere in particular'.[12] Frisch continued to quote Roos: 'In general, I feel strongly that we should not encourage mathematical exercises of this nature.' Consequently, the paper was rejected, in spite of Le Corbeiller's acceptance and Frisch's initial enthusiasm but paradoxical management of the affair.

Creedy did not give up and presented a paper to the econometric conference in Colorado Springs in 1935. The paper, on the principle of least action, was discussed by Georgescu-Roegen, who asked for statistical confirmation (*Econometrica* (1935), 3(4): 475). At the time, the matter was sufficiently bizarre to be ignored by the econometricians. Yet, the whole exercise was not a superfluous one, since, during this exchange, Frisch also had proof of the fact that Tinbergen was indeed quite suspicious of any effort to develop economics on the basis of simple analogical reasoning. Commenting on another paper, this time proposed by Bolza under the title 'A Generalization of the Conservation of Energy Law', Tinbergen concluded that 'I cannot see it is very useful for economics until better examples, giving really new insight, are given by him.'[13]

Frisch accepted that the divergence was related to the consideration of the role of mechanical analogies for economics: 'For instance, with regard to the application of the mechanical analogies, I think I believe a little more in them than you do. But of course there must not be any "mechanical" application of mechanical analogies.'[14]

The epilogue of this story was also written by Creedy, who proposed a new paper four years later (3 January 1939): 'The Mathematical Theory of Society'.

Having received no answer, he insisted again on 2 May. Frisch rejected the paper on 25 May, offering no explanation for his decision, in sharp contrast to the previous lengthy correspondence that had been maintained between the two parties in relation to the first paper submitted to *Econometrica*.

Newton in the province of economics

Although the paper published by Creedy did not deserve much attention – neither then nor later on – some recent authors (Dimand, 1988: 159; Boianovsky and Tarascio, 1998: 20n.) have noted that it had one original feature: unlike most of the work at that time that was based on maximisation principles, Creedy proposed Newton's laws as the basis for the analogy that economics should incorporate from physics and his objectives were indeed clearly outlined in the paper:

> The present investigation aims at basing the subject of Economic Dynamics on clear mathematical foundations as rigorous as those employed in any other branch of dynamics. It is shown that it may be based on postulates in complete formal analogy to those of ordinary dynamics. Economic Inertia and Economic Resilience (and Storage) are then defined and illustrated by examples. Differential equations involving these are next formulated for simple cases corresponding to the ordinary Dynamics of a Particle and it is shown how they enable us to plot curves of economic behavior as functions of time.
> (Creedy, 1934: 363)

The paper takes the analogy very far: economic 'force' is defined as the rate of acceleration of an economic action, economic 'inertia' is defined by the finiteness of increases in economic variables, money deposits in a bank are equated to the storage of energy in a spring or of electricity in a condenser and, finally, oscillations are defined for the case of radiation as well as for economics (ibid.: 363–4, 372, 380). Furthermore, the analogues for the three Newtonian laws of motion are also defined: the first law is redescribed as the permanence of economic actions unless the circumstances change, the second law is translated into 'the books must balance' and the third law, $F=ma$, force equals mass times acceleration, is translated into 'effective persuasive force = rate of acceleration of economic actions times a constant' (ibid.: 363–4).

Although the author recognised that economics lacked the means for mimicking physics in all its rigour, he argued that there are also some phenomena in dynamics for which we do not have the full knowledge of the relevant equations. In spite of that, dynamics could provide important information on these systems, and the same should be done for economics. For Creedy, this was a supplementary reason for a literal metaphorisation:

> We have no such convenient instrument as the spectroscope (although mechanical harmonic analysers might serve the same purpose) to resolve our periodic phenomena into their component simple harmonic oscillations,

> but our problem is essentially the same. 'Given a jumble of periodic phenomena, to find an interconnected dynamical system which will parallel the observed phenomena without departing at any point from what we can observe in other manners.' This is a statement of the problem which *is applicable without changing a word to either the physical or the economic case*.
>
> (Creedy, 1934: 380; my italics)

Frisch, as we saw, agreed with Creedy as far as the literal translation of concepts from mechanics into economics was concerned, although he could not accept the discipline of analogues for universal laws such as Newton's. This already constituted a major reservation, but other econometricians thought it was still too uncritical in relation to the real differences between the two scientific fields. This sharp difference in appreciation fuelled the next round of debates.

Quarrels about mechanics

Although all – or most – of the econometricians shared an immense curiosity about the mathematical and formal developments of physics, and almost unanimously considered this to be the paradigm for sciences, the discussion that took place over the editorial policy of *Econometrica* suggests that the group was not absolutely homogeneous in relation either to the possible forms of such incorporation or to its uncritical acceptance.

The veteran of the econometric movement, Irving Fisher – who was simultaneously responsible for the introduction of neoclassical economics into the US – was an unrepentant advocate of the heuristic primacy of the mechanical analogy. His Ph.D. dissertation from 1892 presented a much quoted dictionary for the translation of mechanical concepts into economics. This table was accepted and praised by most economists, and above all by those interested in the new revolutionary trends that neoclassical economics was announcing. Forty-five years later on, this table was still considered to be a landmark for a new brand of economics: Harold Davis,[15] in a report written for the Cowles Commission, argued that 'the famous table of mechanical analogies published by Irving Fisher in 1892 furnishes a powerful guide to exploration and generalization in economics' (Davis, 1937: 11).

It is obvious that the econometricians meant to establish much more than merely an analogy between the definitions of the two sciences and the use of available and translatable equations and definitions. Physics and mechanics in particular represented a standard for establishing the legitimacy of the argument, a model of representation and demonstration, an archetype of scientific communication. In fact, this required economics itself to be redefined accordingly, so that the analogy might hold and conservation principles could be applicable: as a consequence, agents were described as atoms, markets as closed fields, as required by conservation of energy, and economic action as maximisation of an objective function under constraints.

There are abundant examples: according to the Joseph Mayer report printed in *Econometrica* on one of these first conferences, the meeting of the Econometric Society held at the Hotel Syracuse in New York, 20–23 June 1932, and organised by Hotelling and others, a paper by Davis, from Indiana University, 'showed how the problem of perturbation in economic series has all the essentials of the problem of explaining the methane spectrum by means of the perturbations of the atoms' (*Econometrica* (1933, 2: 94–104).

The atomic metaphor was quite convenient, on several grounds. First, it allowed for the use of Hamiltonian mathematics and all the methods derived from the dynamics of conservative systems. Second, it fitted in with the postulates of rationality and the over-simplified description of the *homo economicus*. And third and not least, it paved the way for the introduction of probabilistic concepts into economics, as it provided the rationale for the use of the Law of Large Numbers and the Central Limit Theorem. But its shortcomings were equally impressive: it was not easy for many to accept that human choice was equivalent to the trajectories of gas particles, or furthermore that social structure and social behaviour would not be any more complex than the random movements of these particles.

Consequently, the econometricians divided into three groups expressing contradictory trends of opinion. A first group accepted and argued in favour of the analogy all the way through – and these were essentially the economists who accepted and defended the neoclassical postulates. The analogy was simply designed to translate the concepts from physics into economics. A second group did not dispense with the mechanical analogy and thoroughly explored its mathematical implications, although they remained suspicious of the behavioural implications and semantic value of this metaphor. For these economists, the analogy produced the translation of law-like assertions between different fields. And, finally, a third group openly challenged the metaphor, and in general deduced radical implications in relation to the use of mathematics in economics.

Fisher, Schultz, Marschak[16] and so many others belonged to the first group, whereas Frisch was a representative of the second one. Tinbergen and Roos might also be included in the latter, although they argued for a much more restrictive application of mechanical metaphors.[17]

The third group was certainly important at the time of the foundation of the Econometric Society, and several disputes about the role of mathematics highlight the internal differences of opinion on this subject. Later on, however, this group was to become marginalised and increasingly ignored. In these founding years, the sceptical group had two main apostles: Amoroso and Schumpeter. Schumpeter, who happened to chair the assembly that created the Society, in December 1930, had a long argument with Frisch on the role of mechanical illustrations and demonstrations for the explanation of economic oscillations, and remained quite unconvinced of the usefulness of these tools until his death (see Chapter 6).

Schumpeter defended the same argument in his discussions with Roos. Although Roos was particularly critical of mere transcriptions from physics to economics, as he showed in the Creedy episode, he still argued for an intense use of the analogy between both sciences and tried to convince Schumpeter:

> I feel that you draw the line too sharply in your physics-engineering metaphor. There are both theoretical and experimental physicists; the great advances in the science have, for the most part, been made through the fusion of ideas of the two groups. You probably recall that medieval philosophers argued without avail for several centuries over the problem of whether an empty bucket or one filled with soluble material would hold the most water. The problem was, of course, quickly settled when someone suggested an experiment. Now, in economics it is impossible to experiment except by injecting theories into the political lives of nations. If experiments show truths which have been mainly argued about, perhaps econometricians would do well to attempt to analyse the experiments. I do not feel that a growing science, such as econometrics, can secure proper nourishment without tackling important contemporary economic experiments.
>
> (Roos to Schumpeter, 1 May 1935)

Roos continued seeking to identify some of the possible misuses of this experimental science-to-be:

> There is, of course, a grave danger in considering contemporary problems in that clever [way in which] statisticians are often able to document their theories too well. For instance, Keynes in his *Treatise on Money* gives a very convincing argument to the amateur economist. I would not agree, however, that for this reason econometricians should leave the field of contemporary problems to the *charlatans and amateurs* [handwritten note in the margin: 'not meant to apply to Keynes']. I feel rather that each member of the Society should constantly be on guard to point out flaws in logic and methods of analysis to their fellow members. Of course, I do not mean that we should air our quarrels publicly except in the cases in which fellow members have publicly advocated fallacious plans.
>
> (ibid.)

In spite of this argument, Schumpeter was never convinced. Indeed, he resisted it on grounds that most other economists could not understand at the time: he was representing the economies through an incomplete literary model, remaining hostile to the available mathematical encapsulation. An illustration of his search for a better understanding of complex relations, innovation and structural change in economics is a letter that Schumpeter sent to Domar much later. Commenting on Domar's paper on capital expansion, the growth rate and employment, Schumpeter marvelled:

> In particular, your paper is the first symptom I have found in the literature of model building of an awareness of the fact that variation in output *never means* simply variation in the output of a homogenous quantity or else a process that can be dealt with *according to the schema of a kinetic theory of gases*, but also and inevitably means structural change with some of the molecules eating up the others. So far I have in vain looked for a method of expressing this in any exact form. I do not know whether I am making myself quite clear, but I hope, in any case, for further discussion with you.
>
> (Schumpeter to Domar, 21 March 1946; my italics)

In vain, Schumpeter looked for a model to express his thoughts and could not find a mathematical way to do so.

Amoroso was another member of the econometric group[18] who could not share the general enchantment with physics, for he was convinced that the physics that the economists were looking at was already out of fashion. On the other hand, he believed that the crucial problem for economics was to abandon the ill-defined concept of equilibrium. In a letter to Frisch, Amoroso told him:

> We would not be right today to use the ideal of representation of the economic facts according to the – essentially static – model of classical physics. Physics itself abandoned this model and its deterministic conception; what must econometrics do? Its true *raison d'être* as a science is to represent economic dynamics following a model able to represent the element of 'will', which importance in economic facts is capital and cannot be underestimated. This being my point of view, I am led to consider surpassed, and out of place in a journal of a Society created to excite the progress of mathematical economics, papers based upon certain concepts directly related to the theory of economic equilibrium; these concepts, although carefully elaborated, have already given what they had to give.
>
> (Amoroso to Frisch, 21 December 1931)

But this could not be accepted at the time. Equilibrium – whether defined as a process of balance in the social realm or as a device used in close analogy with mechanics – was irreplaceable in econometrics, and that is why even those who remained suspicious of the analogy with physics could not dispense with the mechanical methods. Some of them, as seen in this chapter, were quite outspoken in their mistrust of trivial applications and the increasing returns of the industry of mechanical analogies. Yet it prevailed, under one form or another. Humans were pictured as particles, behaving like particles and organising themselves like particles – the mechanical consensus was imposed upon the dissidents, and the econometric revolution proceeded on its way.

Models of science

The mechanical consensus was nevertheless a feature of a passing science. Since the scientific revolution of the seventeenth century, the archetype of scientific thought was supposed to be defined by its vehicle, the perfectly logical language of mathematics; but since mathematics was mostly developed as a mode of intelligibility of physical phenomena, the fervours of the imagination of the upcoming sciences were mostly captured by mechanical representations. Understanding a phenomenon became equated with modelling it, so that both the perseverance and success of physics as the legitimate model among sciences may be attributed to the mechanical consensus.

Volterra, a major player in international science who was reverently consulted by Pareto on the soundness of his own maximisation philosophy, and himself a member of the Econometric Society, emphatically argued for this paradigm:

> Many illusions have disappeared nowadays regarding the mechanical explanations we may propose about the Universe. But, when one lost the hope to explain all the physical phenomena under laws by analogy to that of universal gravitation or by a single mechanism, a new idea took shape almost compensating for the fall of this edifice of hopes. That is the idea of the mechanical models: perhaps it does not satisfy those looking for new systems of natural philosophy, but it provisionally suffices to all those, more modest, who accept all analogies and especially the mathematical analogies highlighting the natural phenomena. A mechanical model of a phenomenon is indeed a device built under the sole preoccupation that, once put into action, certain parts follow or change according to the same laws of variation as certain elements of the phenomenon. Experience tells us that models have been very useful. They served and always serve to orient our research in the newest and more obscure domains of science where we blindly look for our way.
>
> (Volterra, 1901: 12–13)

Although suspecting Pareto's application of the maximisation principles of the total amount of energy and the behaviour of particles to the description of economic agents, Volterra defended this widespread use of mechanical analogies. Nevertheless, some decades later, at the time this narrative refers to, this consensus was superseded by the emergence of the purely mathematical model replacing the mechanical analogy (Israel, 1996: 9–11). The apex of such a mathematical model came to be axiomatics, and more precisely the extreme form of Bourbakism:[19] mathematics was thought of as a 'reservoir of abstract forms' (Bourbaki, 1948: 46). Econometrics was created as a powerful intellectual movement precisely at that point where mechanical and mathematical models entered into conflict and where axiomatics made its first appearance in economics and restated the general equilibrium approach.

Frisch's involvement in this conflict is paradoxical. More than once, he emphasised his allegiance to the mechanical representation of an idea as the most suitable procedure for understanding it; yet, throughout his life, he looked for mathematical models of those representations, including struggling with the most difficult models. But, as the mathematical model flowed from the

mechanical representation, he could not conceive of a dichotomy between their respective explanatory powers, since one was no more than the expression of the other. Consequently, he did not follow the Bourbakist movement, since he abhorred philosophical statements on very abstract economics and did not extensively use axiomatics, except in the way that he defined the basic assertions of production theory and certainly not in business cycle analysis, which was his main interest at the time when he was engaged in constructing the building blocks of the econometric movement. Frisch wanted answers and not riddles, and consequently demanded that economics should be seen as the powerful instrument for social choices and not a tool for illuminated games of logic.

In spite of this distance created by suspicion and diverging interests, there is in Frisch's epistemological approach a hint of a milder form of pre-Bourbakism, as he conceived of the functioning of the brain itself as imposing a certain conceptual equivalence through different sciences. This was derived from the early influence of Karl Pearson 'The Grammar of Science' and, through Pearson, of Mach and his concept of scientific logic arranging sensory information with no claim on reality and therefore on causality. Frisch's thoughts on the rejection of causality were too peculiar for a statistician and mathematician of the 1930s and 1940s and they are to be attributed to this idealist spell.

The conclusion was not, for Frisch, an abandonment of science but instead an emphasised role for the mechanical concepts, since they were the universal analytical tools and even the epitome for making adequate generalisations applicable both to physics and to economics – the way in which the brain could understand and formulate the specificities of these sciences. At his Nobel speech, Frisch again introduced this thought in reference to some 'ultimate reality' that may elude the efforts of science (Chapter 9).

Yet, in contradiction to Bourbaki, Frisch believed that concepts emerge from the way in which the human brain functions – he frequently repeated this notion, for example in 1929, 1933 and 1965. Bourbaki, however, believed that reality adapts to rigorous concepts: 'some aspects of empirical reality mould themselves to certain of these forms [the mathematical concepts] as by a sort of preadaptation' (Bourbaki, 1948: 47).

Frisch's ease with 'thought experiments' can be explained through his conception. Since his inaugural papers, Frisch had argued for the methodological value of these experiments, emphasising that real observations were unattainable. His 'Quantitative Formulation of the Laws of Theoretical Economics' (1926), a staunch defence of both Fisher and Schumpeter, 'prominent representatives of two different groups working towards "revising the logical foundation of the theory"', and an argument for mimicking physics, stated that statistics consisted in experiments in approximations:

> The theory gets its concepts from the observation technique. . . . For the logical definition it is enough that [the observations] exist as thought experiment. . . . Nevertheless, this form of conceptualization has opened a possibility for realizing the connection between the abstract concepts of

> theoretical economics and economic life as it is reflected in the numerical data of economic statistics. *Although the observations that can corroborate the abstract quantitative definitions are not possible in practice, they are even so the first step towards efficient observations.* They post a target where there used to be none. They show the point that the statistical technique of approximation shall try to hit.
>
> (quoted in Bjerkholt, 1995: xxii, my italics)

According to the paper presented at the joint meeting of the American Economic Association and the American Statistical Association held in Washington, in December 1927, recourse to 'imaginary observations' was justified in the same way that light signals were used in relativity theory. If real observations are expensive or difficult: 'It is sufficient that the observations considered can be carried through in principle' (Frisch, 1927b: 2–3). At his inaugural lecture as full professor, Frisch restated the same idea (Bjerkholt, 1995: xxii). In his Yale lectures, Frisch defines causality as an attribute of the mental processes of interpretation and not of reality itself. The figments of imagination could be useful as indexes of reality which, anyway, was not fully scrutinisable, and the mechanical representation and its mathematical model were consequently needed as indispensable and logically verifiable constructions.

Science is the method: one could not imagine a more absolutely radical reason for the econometric revolution. This reason became the rationale for intense and fighting debates – after these confrontations, nothing would ever be the same in economics.

Part III

Debates

6 Intriguing pendula

Delights and dangers of econometric conversation

The pendulum swings back and forth throughout the history of science and haunts many of its more creative insights. It is said that in 1581, while attending Mass at Pisa Cathedral, Galileo Galilei, a seventeen-year-old student of medicine and son of a mathematician, counted his own heartbeats in order to verify the constancy of the period of a chandelier gently swinging from the roof. Although this is a legend, created by Galileo's biographer Vincenzio Viviani (Newton, 2004: 1), the fact is that Galileo – probably from his experiments in music – understood the property of isochronism, the independence of the cycle in relation to the amplitude for small oscillations, and this was crucial for designing the mechanism of the clock (ibid.: 51).

In any case, in 1632, the older Galileo discussed the properties of the intriguing pendulum in the *Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican*. This provoked major consternation, since the Vatican had already instructed him to abandon his Copernican view since 1616; the fact that Galileo was able to develop his argument, under the pretext of a balanced overview, was only due to the exceptional fact of his being granted permission to do so by the recently elected Pope Urban VIII. In spite of this favour, the book was banned as soon as it was published. The next year, Galileo was brought to Rome and condemned to life imprisonment. The dialogue was closed: the pendulum was not only a fascination throughout Galileo's life, but also his disgrace.

Galileo was not alone in his fascination. Leonardo had already sketched the possible use of the isochronism of the pendulum, the harmonic oscillator, in order to build a clock. Fourteen years after Galileo's death, a clock was indeed designed and different prototypes were built afterwards – the immense success of the pendulum was based on its ability to provide the mechanics needed for the representation of time. Consequently, the pendulum was erected to the symbol for the era of manufacture. In the eighteenth century, the accepted scientific model of the universe was the clock. Kepler emphasised this simile between the clock and the universe:

> I am now much engaged in investigating physical causes; my goal is to show that the celestial machine is not the likeness of a divine being, but is the likeness of a clock (he who believes that the clock is animate ascribes

> the glory of the maker of the thing made). In this machine nearly all the variety of movement flows from one very simple force just as in a clock, all the motions flow from a simple weight.
>
> (Kepler, quoted in Olson, 1971: 60)

But pendula were not as simple as they ought to be. One century after the imagined Mass at Pisa Cathedral, in 1687, Isaac Newton discussed in his *Principia Mathematica* the collision of two pendula as an expression of the relationship between two bodies. Again, half a century later, both Leonhard Euler and Daniel Bernoulli studied the movement of several pendula hanging from each other; in a text written in 1738, Bernoulli included and discussed graphs of the double and the triple pendulum.

This approach to the problem of the three bodies was an important step in the research: Bernoulli identified the natural modes of oscillation and some simple forms of coupling, for instance the long period movements obtained when the pendula were 'beating' in phase and the short period movements obtained when they were out of phase. As the mathematics of the simple pendulum was straightforward and as physics had majestically imposed itself as the queen of sciences, this simple mechanical device was used by other scientists both as an authoritative reference and as a powerful heuristics applied to other natural or social processes. So it was with the economists: this chapter investigates the conditions, the doubts, the difficulties and the conversation leading to the incorporation of the metaphor of the pendulum into economics.

Indeed, it was not difficult to use the sinusoidal curves describing the movement of the simple pendulum in order to interpret business cycles, the first

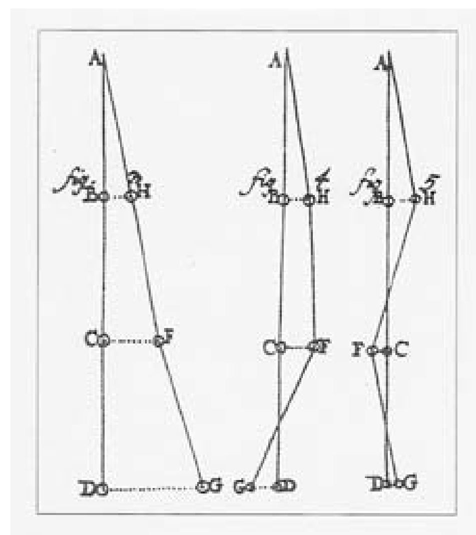

Figure 6.1 The three pendula of Bernoulli.

challenge in the definition of the early econometric programme. In the 1930s, the founders of econometrics concentrated most of their research efforts on this topic: Fisher had already been publishing on cycles for a long time, Frisch worked out his models during the decade and Schumpeter offered his *magnum opus*, *Business Cycles*, in 1939. Tinbergen, Hicks, Kalecki and many others contributed to the research, which became the central theme of many conferences organised by the young Society. This was a quite obvious strategy: for years, the priority of macroeconomic research was accepted to be the taming of the large surges of devastation provoked by cycles and recession, since the dangers they implied were obvious as the world was dragged into war. The choice of the most suitable model for representing innovation, change and equilibrium in economics defined the frontier of economics at that juncture.

The econometricians shared the ambition of providing a correct model for cycles; yet they could not agree on the most adequate model, and quarrelled intensely about its selection. In 1931, Schumpeter and Frisch engaged in an intense correspondence trying to establish the pattern for that necessary and useful model. Their conversation provides a magnificent example of the importance of rhetorics in economics and of the heuristic role of constitutive metaphors in a research programme, but it also highlights the difficulties in defining the most suitable mathematical formalism for dealing with cycles and structural change. Furthermore, this discussion underlines the crucial importance of one metaphor, the pendulum, for the purposes of persuasion and representation, as well as for the creation of new conjectures in economics and in particular for research into long-term evolution and fluctuations.

Although the discussion was inconclusive for both Frisch and Schumpeter, and although their different points of view were specified and debated but not modified, it was an important step for their next contributions (Frisch, 1933a; Schumpeter, 1939 and the posthumous volume of 1954). Frisch, who was an outstanding mathematician, produced a sophisticated model that became a paradigm for the representation of the mechanics of cycles. At the same time, Schumpeter offered a matchless account of the historical processes of revolutionary industrial changes. And, in spite of their close friendship and intense exchanges, neither was able to incorporate the other's intuition into his own model. The conversation ended as it had begun.

Among other metaphors used at the time (the rocking horse, the violin, etc.), the pendulum may be singled out as an exceptionally powerful representation of economic oscillations. This metaphor also indicates a paradox: in spite of its being devised as an argument in favour of equilibrium and the traditional inferences from time series, the pendulum concept allows for a variety of alternatives, some of which imply a regime of simple gravitation towards equilibrium while others imply chaotic attractors – but that was, at the time, not known by those taking part in the conversation. Neither of them was aware of the other possible epilogues, namely of the hidden implications of these verbal accounts or model representations of pendula as the founding metaphor for business cycles.

Consequently, the impressive fact that these models established the canon for decades to come merely prolongs the paradoxical misunderstanding that these authors fathered at the time. In fact, the pendulum model – and its use in the clock – required a finely tuned mechanical instrument and, consequently, a perfectly deterministic structure. But, when it came to the province of economics, the pendulum metamorphosed into a different meaning, since it acquired perturbations changing the course of the harmonic oscillation in order to account for irregularities and, as a consequence, ceased to refer to the pure determinism of a perfect sine curve. In economics, the clock could only account for time if it was wrong. Immersed in these contradictions, the story of pendula in economics is indeed another intonation of the process of transition from the mechanical to the purely mathematical mode of reasoning.

Inner or outer?

As the reader is by now fairly well acquainted with Ragnar Frisch, it is time to introduce his correspondent's points of view. Joseph Schumpeter's main contribution to economics was a defence of the historical approach to cycles as representing the crucial feature of the dynamics of capitalism. Although a staunch supporter of the use of mathematics, as well as a founder and thereafter one of the leading figures and future president of the Econometric Society, Schumpeter distinguished himself as an intensely dedicated researcher in the field of concrete historical processes and not as a mathematical economist. And he was quite successful: Schumpeter eventually became the most frequently quoted economist in the first decades of the century, until the glittering triumph of Keynes's *General Theory*.

Schumpeter's main publications are historical in the sense that they represented applied historical and conceptual work (*Business Cycles*, 1939), but they were also highly controversial in their interpretation of historical and contemporary trends (*Capitalism, Socialism and Democracy*, 1942) and provided a historical account of the science itself (*History of Economic Analysis*, published posthumously in 1954). His single most important contribution, and indeed the major reason for contemporary attention to his work, was his analysis of innovation, creative destruction and disequilibrium processes in modern capitalism.

When Frisch questioned Schumpeter's concept of innovation, the fact is that he could not fully understand or at least represent it in his mechanical world. As a consequence of this misunderstanding, the nature of the evolutionary system of economic fluctuations was poorly discussed from the viewpoint of the requirements for an econometric approach to cycles and structural change – and their conversation turned out to be untranslatable. Nonetheless, it was very useful for both, since it forced each of them to clarify for themselves their own concepts and models, although the correspondents could frequently not understand each other's arguments.

Schumpeter's concept of innovation has been widely known ever since the publication of his first influential book, *The Theory of Economic Development* (1911). Innovation was systematically presented as the encapsulation of a driving force for change that emerged from economic development, a process akin to that of biological mutation, pioneered by entrepreneurs, who were able to incorporate into the economic world new methods of organisation, new products or processes, or to create new markets. This concept of radical change and entrepreneurship was influenced both by the tragic tradition rooted in Nietzsche, so present in the German cultural environment of the early days of the twentieth century, and by the impact of the ideas of John Bates Clark. Yet Schumpeter developed it from the original viewpoint that accounts for his fame.

Schumpeter's lifelong project was to create a general theory superseding, although at the same time including, that of Walras, an economist he admired more than all the others, but whose theory was considered to be wrong if taken in isolation, since it just accounted for static processes. 'I felt very strongly that this [the presentation of economics as the explanation of exclusively static processes] was wrong and that there was a source of energy within the economic system which would of itself disrupt any equilibrium that might be attained', wrote Schumpeter about his meeting with the ageing Walras at his Swiss home in 1909 (Schumpeter, 1937: 160). Therefore, a truly general theory ought to include equilibrium and statics, as well as disequilibrium and dynamics, i.e. economic processes describing the reality of capitalism. This was repeatedly emphasised by Schumpeter in his most important works and represented his crucial contribution to the study of innovation, as he categorically stated in his last major text:

> Social phenomena constitute a unique process in historic time, and incessant and irreversible change is their most obvious characteristic. If by Evolutionism we mean no more than recognition of this fact, then all reasoning about social phenomena must be either evolutionary in itself or else bear upon evolution.
>
> (Schumpeter, 1954: 435)

This evolutionary approach included several important features that are not discussed in this chapter, such as the consideration of distinct modes of change and time dimensions (Kondratiev cycles of infrastructural change and Juglar cycles of industrial change), supposed to determine the dynamics of fluctuations. But the decisive point, the one that distinguished Schumpeter from his colleagues, was the claim that innovation and destructive change are to be seen as central characteristics in the process of self-organisation under capitalism. The evolutionary process incorporated internal change into the structure of the economy. An undated manuscript found at Harvard,[1] 'Statistical Evidence as to the Causes of Business Fluctuations', presents the argument in a nutshell:

> Summing up, it may be stated that statistical evidence suggests and in a sense even proves that business fluctuations are produced:
>
> a By the impact of factors external to the business organization;
> b By an evolutionary process within the business organism which is what is popularly meant by economic progress;
> c By the reactive response of the business organism to both.

This is a reasonably faithful representation of Schumpeter's lifelong adherence to the distinction between external secondary factors in the development process and the internal changes that represented the strength and essence of entrepreneurial capitalism, in the same sense as he emphasised this distinction later on (Schumpeter, 1939: 68).

His close friend Ragnar Frisch shared the same concerns and considered the understanding of business cycles to be the primary task of economists. But he addressed the question from a rather different viewpoint, since he suggested a mathematical approach for the sake of obtaining the level of formal rigour best suited to the normative action that was desired. Their correspondence discussed at great length the possible options for the representation of the economic system and its cycles: while Schumpeter described a very complex causal system, Frisch represented this same system as a rather simple mechanism. As a consequence, there was an obvious contradiction between Schumpeter's approach and the quite different representation of his own theory by Frisch, who proposed the deterministic and passive system and the exogenous but small perturbations as the sole factors responsible for fluctuations and the dynamics of the economic system – the simple pendulum. If this were adequate, we would have exogenous causality determining the movement, plus an endogenous filtering mechanism determining the shape of this movement. This latter mechanism was identified through its mechanical properties, i.e. it required accepting a clear distinction between (exogenous) causality and intelligibility (understanding the mechanism itself).

Later on, Frisch's main contribution to this subject, the one published in 1933 in the volume printed in honour of Cassel, represented a crucial departure for the econometric approach of time series and cycles. Schumpeter referred to this paper repeatedly and always approvingly in his later books,[2] in spite of the obvious differences between his own explanation and this model, which reduced the cyclical mechanism to exogenous impulses impinging on the propagation and the equilibrating system. This followed Wicksell and Akerman's metaphor of the rocking horse, which soon became the first and long-lasting paradigm for the analysis of cycles. Erratic shocks were considered to be the source of strictly exogenous impulses, and therefore the theory could not account for internally generated mechanisms of historical change. The extension of the model, provided by Frisch to account for Schumpeter's objections, was not fully satisfactory for the latter, as we shall see.

This contradiction has remained unnoticed by most of those working on the subject, since the intellectual relationship between Schumpeter and Frisch is little known and since the relevant private letters were never discussed until 1999.[3] Evidence shows that both authors discussed these topics at length, that their concepts did not match and that consequently much misunderstanding remained. Furthermore, Schumpeter never fully accepted the powerful explanation and method his colleague was using and, consequently, could never follow the econometricians in their own particular terrain in the study of cycles: whereas for Schumpeter the cause of fluctuations derived from the very characteristics of capitalism and its inner drive for change and innovation, Frisch's model forced him to attribute change to outside factors. The confrontation could therefore be as radical as the opposition between these points of view, were the contenders able to understand each other fully. But they were not.

Pendula swinging back and forth

Schumpeter and Frisch first met in autumn 1927 and then again the next year at Harvard in February.[4] At that time, Frisch was giving a series of seminars on time series at Yale at the invitation of Irving Fisher, and Schumpeter was also travelling in the United States. From the first moment, they engaged in fruitful discussion, in spite of the differences between them, which were quite striking. Frisch, twelve years younger, was a mathematically inclined economist with left-wing ideas. Schumpeter, instead, was a respected and widely quoted theorist, who had already published a number of influential books. He had occupied the position of Austrian Minister of Finance and later even directed a bank, and was politically very conservative. Yet they became close friends and shared great enthusiasm for a number of projects, such as the creation of the econometric movement, the publication of *Econometrica* and their research into long and short cycles.

They corresponded intensely for many years until Schumpeter's death (1950), and, whenever possible, meetings were arranged. It should be added that by the end of the 1920s and in the early 1930s, Schumpeter and Frisch did not just share a passion for the creation of the Econometric Society: they were simultaneously engaged in time series analysis, although they used different methods and concepts. Frisch had just circulated his paper on time series (1927a) and was preparing what came to be known as 'Propagation Problems and Impulse Problems in Economic Dynamics' (referred to hereafter as PPIP, 1933a), his mature work on cycles, whereas Schumpeter was already engaged in the preparation of his seminal *Business Cycles* (1939). There was an obvious common ground that they were glad to recognise: they both intended to explain how change occurred, accepted the existence of different modes of oscillations – Juglar and Kondratiev cycles – and sought to construct a formal model of the cyclical process within a rigorous analytical framework.

This aim was deeply rooted in previous work by Frisch. In 1931, he suggested that no 'Bacillus Cyclicus' could be found as the basis for a simple explanation:

> A workable explanation of business cycles will never be brought to us, I believe, in the form of the discovery of some 'bacillus cyclicus'. The very nature of the problem is of a different sort. The solution lies, I believe, in a comparison between *magnitudes* of certain economic parameters, such as demand elasticities, supply elasticities, etc. To put it in a nutshell: The 'cause' of the business cycle cannot be pinned down to reside in some particular one of those phenomena which we now know are connected with the cyclical swings.
>
> [. . .] The explanation will be found in the fact that a whole set of these elasticities or other economic parameters *bear a certain numerical relation to each other*, or satisfy a certain numerically specified relation. It is the character of the relationship expressed numerically that is responsible for whether we shall get cycles or not. For certain magnitudes we may get cycles. For others, not.
>
> (Frisch, 1931: 2)

The model should investigate these economic relations between highly aggregate variables, distinguishing between impulse and propagation, the 'kicks that actually prevent the system from adopting a state of rest' (ibid.: 5) – the rocking horse.

This evidence shows that for a long period Frisch developed a mechanical model simulating cycles. Schumpeter feared this deterministic representation and, consequently, their respective points of view were quite different, so that it was not easy to create a common conceptual language that could be used to understand and compare their respective approaches and models. Furthermore, Frisch took the initiative in this argument, since he was better equipped from the point of view of formal and mathematical reasoning. From 1927 until the early 1930s, Frisch worked on the construction of his model, establishing the distinction between the 'impulse' and the 'propagation' problem. This also proves that, by the time of their first discussion, the idea of a mechanical representation of a damping system was already clearly formulated and that its implications were well understood by Frisch, who tried to reduce Schumpeter's theory to his own conceptual model.

The pendulum was already an important reference at that time for the analysis of cycles. Indeed, it had dominated the rhetoric of cycle analysis prior to the use of the rocking-horse analogy: Marx, Fisher, Yule and Hotelling, among others, had used the pendulum metaphor in previous years. Frisch had used it since at least 1927, when he made his first efforts to model cycles as fluctuations submitted to friction, and so did Tinbergen later on in 1935. From then on, all his contributions to the analysis of business cycles and to the discussion of time series analysis centred around this metaphor: when Frisch and Schumpeter engaged in their controversy in 1931, it is certain that Frisch already knew in detail the mechanics and the mathematics of the simple pendulum and had imagined several fruitful ways of extending the analogy.

Frisch's 1927 paper on time series analysis endorsed the 'rather popular' analogy of the movement of the pendulum back and forth as a representation of business cycles, although he presented it as a mere illustration and not as a true representation:

> It is rather popular by way of analogy to speak of the cycle as a pendulum oscillating back and forth. Such an analogy might be good or bad according to the use made of it. Its value can hardly be proved or disproved by any a priori discussion. The ultimate test must be if it works or not when it is applied to actual data. It should be emphasized that the pendulum analogy is here used merely as an illustration for the sake of suggesting some plausible working hypothesis regarding the differential properties of the curves representing the various components in a time series. It is not in itself considered [preliminary version: 'adequate'] as a true representation of the complexity of economic life, which is adequate for all purposes [hand-written added phrase:] and from which all kinds of theoretical explanations might be derived.
>
> (Frisch, 1927a: 9–10)

In the same paper, Frisch presents the solution to the equation of the frictionless pendulum, which is quite trivial, but adds two decisive new ideas. First, if 'small accidental pushes' are added, then 'we shall have a fairly good picture of a kind of cyclical fluctuation where positive and negative deviations alternate much in the same manner as in the cycles revealed by the study of actual statistical data' (ibid.: 11). Second, if the successive components of the series are represented as a chain of pendula, this may illustrate 'the way in which a fluctuation of low order might be said to generate those of higher order or vice versa', considering the 'additive trends of successive orders' (ibid.: 15–16). With the 'small pushes' and the 'chain of pendula', the metaphor was transfigured into a powerful new heuristics: even if it did not ascend to a 'true representation of the complexity of economic life', it was certainly an instrument for modelling, computing and simulation. And that was precisely what Frisch and the young econometricians were looking for.

But Frisch was also looking for a more flexible method of curve fitting in order to interpret breaks in the trend, 'a procedure which would make it possible to trace a given component in its actual historical course'. Such a method was therefore based on local properties, seeking information about oscillation around equilibria, or 'departing for good' from it and furthermore considering the Slutsky effect (ibid.: 74–5, 77–8). Consequently, his early work on business cycles concentrated both on the design of a method better suited for dealing with interference phenomena and on an interpretation of the dynamic properties of historical series.

In 1928, Frisch returned to the topic in a new paper, 'Changing Harmonics', which developed from the time series memorandum and emphasised the metaphor of the pendulum as the representation of the movement of cycles.[5] He studied the mathematical frictionless pendulum over a gravitational field and the general solution provided by mechanics for the case of small oscillations around the equilibrium. Although this was not actually stated, he concentrated exclusively on the specific case of a linear approximation to the nonlinear equation. From this equation, Frisch considered three distinct cases of changing harmonics. The first was that of a non-constant period or amplitude of movements, for

instance due to variations in the length of the pendulum. The second was a very interesting case of coupling between two or more components, through the joint effect of their 'beating', each of them having a constant frequency and obtaining greater amplitude of the combined movement if the frequencies were sufficiently close. Finally, the third case, the only one that the author studied in detail, was that of the change in the initial conditions, or the superimposition of erratic shocks upon the damping system. For his subsequent research, only the last one was considered.

Such a choice was not an innocent one. Although an economic interpretation could be offered for them, the first two cases did not lead to the desired clear distinction between the 'propagation' problem and the 'maintenance' problem, since they implied the predominance of exogenous shifts imposed on the system, or worse a nonlinear process. Frisch was not willing to accept that the irregular features and continued oscillation of the equilibrating system should be explained either by the non-determined system of unknown and unknowable variables or by the rather obscure process of coupling. As an alternative, free oscillations (the mathematical pendulum), with friction and a new source of energy, could account for the desired properties of the model: consequently, the construction of the system of equations followed this option.

But Frisch was also struggling against another difficult challenge: he looked for an alternative to the ordinary least squares method of regression, since it imposed the rigid assumption of constant parameters all along the curve (Frisch, 1928: 220). Assuming additive and well-known components (ibid.: 220, 226), Frisch argued for local methods in order to detect changes in the parameters and used a linear operator to describe the evolution of the process. Again, the chain of pendula was used in order to metaphorise the interference phenomenon of the different cycles and eventually the difficulty of fitting the curve:

> The following example will illustrate the principle. Suppose we have a chain of *n* pendula: To a long pendulum with a great mass is attached a much shorter pendulum with a movement in a field of gravitation whose intensity is slowly changing. The length of the individual pendula may also be slowly changing. The fluctuation of the individual pendula may also be measured from the vertical of the point of suspension. The problem is to determine the individual components, i.e. to determine the fluctuations of each pendulum measured from the vertical through its own point of suspension.
>
> If the interval of observation is long enough to cover a considerable total change in the intensity of the field or in the length of the pendula, no kind of curvefitting with constant period sine functions would be successful. In particular the harmonic components determined by ordinary harmonic analysis will have no real significance. But in the vicinity of a point of time the components y_i will approximately satisfy a relation of the form $\theta h\, y_i = g_i^h\, n_i$ *and this is sufficient to determine approximately the ordinates of the respective components in the point considered.*
>
> (ibid.: 231)

The 1927 manuscript and the 1928 paper formed the basis for Frisch's lectures on time series at Yale. The first lecture is entitled 'A Method of Decomposing and Smoothing Statistical Series' and discussed the 'interference' among different orders of cycles:

> Let me use an analogy. A scientist who was sitting on the seashore patiently watching the shifting aspects of the surface of the water would be thoroughly mistaken if he tried to account for *all* the changes observed by the *same* kind of explanation. He would at least have to admit three different sets of ideas: first, the idea of direct action of the wind on the surface of the water. This would account for the small waves. Next, the idea of propagation of the long swells coming from the ocean. And third, the idea of ebb and flow caused by the attraction of the moon.
>
> The situation in economics is exactly the same. In order to understand the ups and downs of business, we must first of all try to understand the fundamental differences between the various *sorts* of ups and downs.
>
> (Frisch, 1930: 3)

Around the same time, Frisch wrote to Mitchell to discuss this interference process, using the same analogy with tides and waves:

> I have definitely started from the assumption that the ups and downs of business cannot be interpreted as *a* business cycle, but must be looked upon as an interference phenomenon between at least two kinds of waves, namely the Juglar cycle (7–10 years) and the subcycle (3–5 years).
>
> [. . .] I feel rather strongly that if we do not take the possibility of such an economic interference phenomenon into account we will be just as hopelessly on the wrong track in our attempt at explaining the ups and downs of the ocean by the *same* sort of argument, without distinguishing between the wind waves (that have to be explained mainly by the friction between the atmosphere and the water), the long swells (that have to be explained mainly by the inertia properties of the water), and the ebb and flow (that have to be explained by the attraction of the moon).
>
> (Frisch to Mitchell, 24 November 1930)

One week later, Frisch suggested interference could be accounted for by decomposition of the different orders of cycles:

> What I ventured to suggest was that the emphasis in cycle analysis ought to be shifted to a study of how one sort of wave may be superimposed on the longer fluctuations, somewhat in the same manner as seasonal fluctuations are superimposed on the longer fluctuations. It was in this sense that I distinguished between *a* business cycle and the notion of a composite phenomenon made up of several components.[6]

The image of the chain of pendula summarised his strategy for decomposing different cycles.

In his series of lectures at Yale, in 1930, Frisch again insisted on this interpretation and presented the chain of pendula as the 'mechanical illustration' of interference among different cycles:

> The following is a mechanical illustration which represents the notion of time components of different orders. Suppose that we have a big pendulum, very long and with a very heavy mass concentrated at its end. To the lower end of this pendulum we attach a shorter and lighter pendulum. To the lower end of this pendulum we attach a still shorter pendulum with a still smaller mass, and so on. Now suppose that we put the whole system into movement. If the mass of each pendulum is small in comparison to the mass of the next higher pendulum, there will be very little influence from the motion of the lower pendulum on the motion of the higher. Therefore, each pendulum will oscillate approximately as if it were a free pendulum. Now let us focus the attention on the movement of the smallest pendulum at the bottom of the system. And let us trace this distance as a time series. This time series will contain a number of components, first a short component, due to the fluctuation of the smallest pendulum at the bottom of the system, then a component with a longer swing due to the presence of the next higher pendulum, and so on. Finally there will be a component with a very long swing due to the presence of the largest pendulum. In short there will be small waves superimposed on large waves. Graphically the most important difference between the nature of these time components will be that the high components will have a much smaller *curvature* than the low components (except in those particular points where the low components change curvatures). In this example we can attach a very concrete meaning to the notion of normal. The normal of the lowest pendulum is at any moment of time the position of the next higher pendulum, and so on.
>
> (Frisch, 1930: 49–50)

After the 1927 and 1928 papers and the Yale seminars, Frisch had a pretty clear idea of a research project on cycles, and that is what he summarised in a paper prepared shortly afterwards. He rejected the traditional time series analysis, not fit for 'social investigators', namely because they were unable to explain evolution:

> The technique which is now most in vogue does not seem powerful enough to deal with the more complicated situations which arise when the time series studied represents an *interference phenomenon* between several components: short cycles, long cycles, different orders of trends, etc., and when, furthermore, the cyclical or progressive characteristics of these various components are changing.
>
> (Frisch, 1931: 73)

In particular, he rejected all methods that abstracted from change, such as the OLS regression and Fourier analysis: 'I am thinking of a procedure which would make it possible to trace a given component *in its historical course* so that we can compare a given historical swing in the component in question with the next swing of the same component. In many sorts of data, and particularly in economic data, it is quite obvious that the cyclical character of a given component is not constant', giving the example of the changes in the cycles at the beginning of the First World War (ibid.: 75). Using linear operators, Frisch suggested descriptive methods in order to detect the wave lengths of different cycles. Yet, the description of these components was rather imprecise: 'The assumptions by which I give a meaning to the notion of "component" are built on the idea that each component shall represent something oscillating around, or departing for good, from a point of equilibrium' (ibid.: 77) – but, of course, the linear operator could not be successfully applied to a process departing from equilibrium.

Frisch was also aware of the potential Slutsky effect creating spurious cycles (ibid.: 78), and trusted that his method could easily detect this effect and avoid its consequences. Consequently, when it came to his correspondence with Schumpeter in 1931, Frisch had already defined a very clear strategy for the analysis of cycles, had worked through different statistical methods and was confident that his solution would shed new light on the question.

The ensuing discussion highlights some of the reasons for the simultaneous use of both metaphors and the distinction between them, and underlines Schumpeter and Frisch's attempts to reach an agreement that ultimately collapsed, although neither of them explicitly recognised its failure or the great chasm between their conceptions.

Magellan's dreams

The first piece of evidence is the letter Frisch wrote to Schumpeter in May 1931. It indicates that Frisch was already approaching the definition of his analytical solution:

> I think I understand now your point about dynamics. Those things you mention: the more or less unpredictable innovations are those things that in my terminology would form the substance of the *impulse problem*, as distinguished from the *propagation problem*. Some other time I want to write you more fully about this.
>
> (Frisch to Schumpeter, 28 May 1931)

Schumpeter answered on 10 June.[7] From the outset, the letter openly stated his reservations about the pendulum analogy:

> This [the discussion of the nature of statics, 'a problem *à la pendulum*'] would be all, if data did not vary except by influences which we could call influences 'from without' or by 'growth'. But there is an agent, within the economic world (=system of quantities) which alters data and with these the economic process: entrepreneurial activity, which I have elsewhere given the reasons for considering as something sui generis (and the sociology of it).
>
> [. . .] It not only destroys existing equilibrium, but also that circuit-like process of economic life, it makes economic things *change* instead of making them *recur*. And its effects are not recurring – Ford can never be repeated – but 'historic' and definitely located in historical time. They are also irreversible. This distinction acquires importance owing to the importance of the phenomena incident to the mechanism by which 'innovations' come into existence. I do not like the analogy with 'growth', else I could express that distinction by comparing it to the distinction between the circulation of blood in a child and the growth of that child. Biological mutations would be a better analogy.
>
> (Schumpeter to Frisch, 10 June 1931)

And Schumpeter added an illuminating postscript to the same letter:

> On rereading this letter I do not know I have succeeded in clearing things up. But always think of the pendulum which, given mass force and so on, and no resistance of medium, would eventually swing in the same way, perfectly [.], and displaying no relevant historical dates. Now let its mass swell from within or a new force act upon it with a sudden push, shifting and deforming it *for good*, and you have a case of 'Dyn. S.' or 'Evolution'.
>
> (ibid.)

This letter defined the terms of the discussion, as far as Schumpeter was concerned. First, it argued that the relevant movements were the irreversible changes occurring in economies ('Ford can never be repeated'), historical changes and mutation instead of simple and mechanical recurrence. Second, it pointed out the nature of the changes emerging from internal forces (entrepreneurial activity) that determine economic evolution. Third, internally generated change was not a process of simple physical growth, and the analogy with biological mutation was thus more appropriate.

Consequently, Schumpeter added the postscript: if the model was to be represented by the pendulum, then the mechanism should eventually be subject to deformations and would be changed by the impacts of innovations, so that it could 'display relevant historical dates'. In that sense, just two weeks later, on 24 June Schumpeter insisted on his critique of the pendulum analogy:

> I am not *quite* satisfied by your classification of the 'innovations' as part of the impulse problem . . . because this seems to coordinate them with events, which come from outside the economic system such as chance gold-discoveries. The problem with these is simply to discover the reaction of the economic system on them.

[. . .] Now as I look at it, any innovations are something different to impulses in this sense. They come from inside, they [.] economic phenomena sui generis.

[. . .] If you class innovation simply among impulses you . . . miss what seems to me the heart of the matter: you only catch the 'vibrations' [.] to the impact of the 'impulse' and not the phenomena attaching to the impulse itself.

(Schumpeter to Frisch, 24 June 1931)

The critique was very clear: innovations should not be considered as part of the small and random impulses, since this would imply ignoring both their causes and their real qualitative impacts. For Schumpeter, innovations were part of the economic system itself, 'coming from inside', and that was indeed his unique contribution. Otherwise, the 'heart of the matter' would be missed, since the effect of the phenomenon would be studied without any attempt to inquire into the causes of the phenomenon itself – as implied by the mechanics of the pendulum.

The long and detailed reply by Frisch is a magnificent example of an attempt at persuasion, and quite an effective one, as we shall see: it is a rhetorical monument. The letter was dated 5 July and recognised the continuing differences between both authors.[8] Furthermore, it argued that a mechanical analogy was indispensable for developing the argument and defining the problem:

You say that you are not satisfied with my classifications of the innovations as disturbances (part of the impulse problem), and I think I understand now why you are not satisfied, but I believe you will be so when you have read this letter. Before I received your last letter (of June 24) I had started again pondering over your point of view, and I began to see clearer why you would not capitulate entirely to my pendulum.

Let me tell you right away that I am glad you did not smooth out our differences in a more or less formalistic adoption of my pendulum analogy, but took the trouble to attempt to convince me that there is something fundamental which is not represented in the picture of the pendula as I gave it originally. We all have our peculiar way of working, and I for one, never understand a complicated economic relationship until I have succeeded in translating it either into a graphical representation or into some mechanical analogy.

[. . .] I think I am able to do so now. Your San Francisco letter [10 June 1931] must have been working in my subconscious even after I sent you my all too simple answer classifying your innovations under the impulse heading.

(Frisch to Schumpeter, 5 July 1931)

Frisch then proceeded to demonstrate his new mechanical analogy: he considered a pendulum with friction, and water flowing at a constant rate into a

container above the pendulum. A pipe connected that container to the lowest point in the pendulum, with a valve in the left side of the bob. The peculiar feature of this system was that the opening of the valve should vary with the velocity of the device, increasing when moving to the right, decreasing when moving to the left. As a consequence, this was a system that provided a self-maintained oscillation. Finally, Frisch applied this analogy to explain the two different sources of impulses, Schumpeterian innovations and random shocks:

Of course you understand already the whole analogy: The water represents the new ideas, inventions, etc. They are not utilized when they come, but are stored until the next period of prosperity (or even longer, some of the molecules in the container may rest there indefinitely). And when they are finally utilized they form the additional surplus of energy which is necessary to maintain the swings, to prevent them from dying out.

[. . .] This picture may now be completed by taking into account random disturbances of the type which I had originally in mind: Imagine a series of random impulses, working either to the right or to the left and being distributed in time and size according to some sort of chance law.

[. . .] Which one of the two is actually the most important in the sense of representing the largest source of 'energy' for the maintenance of the economic swings I think nobody can say today. This can only be found out by painstaking studies that are *econometric* in the best sense of the word. I should be very much mistaken if such studies would not lead us to new Magellanic[9] Oceans in cycle theory. At any rate I think I see now the two-sidedness of the problem. One side I have seen long ago, and the other I have finally realized through your patient explications.

(ibid.)

Indeed, we have not only the verbal description, but also the graphical representation of this model: in the lectures that he gave at the Institute of Economics in Oslo in 1933–4, which were later compiled under the title of *Makrodynamikk*, Frisch included a drawing representing this forced pendulum, closely following the description included in the correspondence between the two economists (Frisch, 1933c: 8505).

Schumpeter reacted to the letter less than two weeks afterwards. On 17 July 1931, after dealing with the preparations for the Lausanne meeting of the Econometric Society,[10] he insisted on the need to consider irregularities, deformations and shifts in the body of economic relations throughout the cycle:

I want to hurry on to our discussion on 'impulses'. I have been fascinated by your analogy, which I think is much superior to the one I had formed myself: I tried to think of the process I have in mind (and which claim precedence as against irregularities, which are the consequence of influences acting from without the economic sphere, but being part and parcel of that sphere itself and sure to display themselves, even if we abstract from

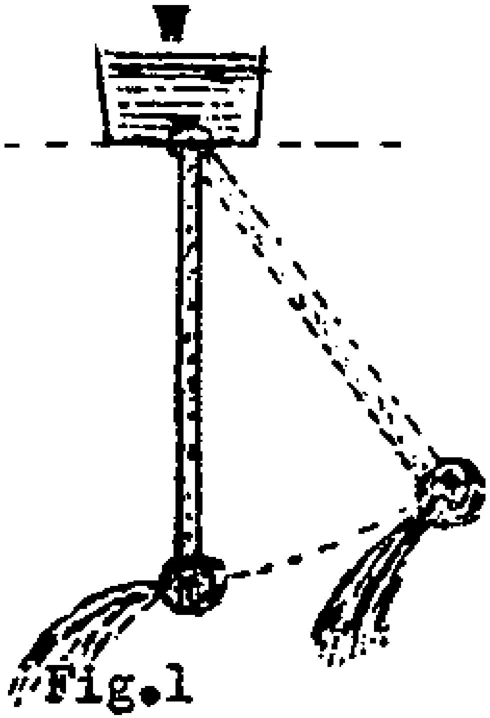

Figure 6.2 Frisch's representation of the Schumpeterian pendulum.

> outside or chance disturbances as we must in a theoretical approach) as of a force acting intermittently on a body (or system), which is partly elastic and partly subject to deformation.
>
> This force pushes the body or system up, deforming it in the process, after which we observe a sagging back of [.] with further deformations, and besides vibrations, elastic reactions, etc. A new system (or form and position of the body) establishes itself, after which the force starts acting again. Now your analogy grips one element of the whole thing so elegantly that it will be possible to proceed with it while very little progress seems (in spite of Volterra *et al.*) to me to be possible with that clumsy and complicated model of mine.
>
> (Schumpeter to Frisch, 17 July 1931)

Again and again, Schumpeter returned to his main objection: in order to represent reality, the system had to allow for change and deformation as a condition of its own movement. Otherwise, the model would be able to grasp just one element of the whole process and nothing else. Therefore, the pendulum metaphor could not be accepted, since it ignored the importance of structural

change and its consequences on the cycle itself: the intertwining of cycle and trend, of fluctuation and evolution:

> They are, however, of less service for another type, which in constructing its models (the word being now taken in a wider sense, of which the model in the sense of a mechanical contrivance is a special case) primarily thinks of the inner life and structure of the economic process. . . . Judging my *Entwicklung* [*TED*] you must not forget that it was problems such as this I was aiming at. In this connection I beg leave to touch upon two things. First, being truly glad to see that my *manière de voir* may possibly, in your hands, [.] of being gripped by the tools of the other type, I am anxious to point out where I am not yet quite satisfied with your brilliant construction. On the other hand, something within me rebels at our pendulum keeping its suspension point. I do think it a great improvement, provided it be feasible, to shift the suspension point in the process. *Il y a plus*: we both surely agree that it would, for many reasons, be highly unsatisfactory to set aside the shifting (I need not explain what economic facts I mean by this) by means of some of the vulgar methods of [.].
>
> Other considerations quite apart, this cannot be done because the shift is *no phenomenon* sui generis, around which the cycle moves, as another phenomenon sui generis, but the *net result* of the cyclical movement, *which is the essential point of evolution*, *de la sort* that our model, to express the theory, would have to be constructed such that the water must arrive *at the same time* [creating] the pendulum movement, [which] displaces the suspension point *and does so only by and through* the pendulum movement . . . and of disturbances.
>
> (ibid.)

Finally, Schumpeter listed some of the inconveniences of Frisch's model:

> I do not quite like the mass of the pendulum remaining constant. I should like our water mechanism to *increase* it in the process. Finally, if the pendulum is to represent not only the social product in the sense of the consumer goods, but the whole of the economic system with all the higher values of goods and commercial activity, an *inner vibratory system* would be extremely useful if it could be [.].
>
> [. . .] However, even so your model would be most useful. And my comments are independent of any unreasonable hopes about what is immediately within our reach. Magalhães' [Magellan's] dreams?
>
> (ibid.)

Dreams indeed, because this requirement of the combined explanation of internally generated changes of regimes, of cycle and trend, pointed towards nonlinear models, and this was still unexplored territory. Consequently, Frisch interpreted Schumpeter's thoughts as mere literary *rococo*, with no analytical correspondence to the formal and rigorous treatment econometrics was already able to deliver by this time. The unreachable and mathematically untreatable Schumpeterian model was consequently completely ignored. Thus, Frisch replied with a polite letter on 24 July, dealing with the preparations for the Lausanne meeting and briefly acknowledging Schumpeter's comments but adding no further argument on the pendulum question. It is quite obvious that he considered the matter to be closed and his pendulum metaphor to be enough, in spite of Schumpeter's remarks and their continuing disagreement about the major issues. Moreover, Frisch was proud of the results of this discussion and, next September, presented the contents of the correspondence to the first econometric conference in Lausanne.

Their differences concerned at least two major points. The first was the formal and analogical representation of those specific disturbances: 'inventions' as Frisch called them or 'innovations' as Schumpeter preferred to call them, since they emerged from inside the system itself.[11] As Frisch's reasoning was dominated by the need for a mechanistic formulation, susceptible of mathematical treatment, he could envisage only two possibilities: either the variable was endogenously determined by the system of equations describing the process and was therefore explained by it, or it was exogenous to that system and therefore served to explain the changes in the process. The representation of two classes of exogenous variables – *à la* Slutsky and *à la* Schumpeter – is a recognition of this difficulty, since there are obvious epistemic differences between them, one representing an aggregation of unknown irrelevant exogenous impacts, and the other a description of crucial changes in the economies. This undefined nature of the random variables – indistinctly dubbed 'shocks', 'perturbations' or 'stimuli', in spite of the difference in meaning involved in each of these denominations – was one of the many consequences, and by no means the least of these, arising from the cursory discussion about the nature of randomness in the early econometric movement. In particular, the discussion between Schumpeter and Frisch on the nature of the variable of innovation in their models is one of the most important instances of the questions asked about randomness, illuminating the pervasive difficulties of simple mathematical formalism applied to economics.

Furthermore, Frisch's model insisted that the variables accounting for innovation do not alter the structure of the process, and merely generate a recursive cycle – a *perpetuum mobile*, albeit an irregular one. Schumpeter's reasoning was dominated by a completely different requirement, a non-mathematical representation of the innovative process, based on a literary approach and influenced by an undefined biological metaphor – mutation – which was designed to explain the driving force behind change in capitalism. Consequently, the mechanical contrivance of the formal model Frisch had in mind had no place in Schumpeter's system. Indeed, one of the relevant contradictions in this controversy is related to the epistemic distinction between the concepts of 'exogenous' and 'endogenous' variables. Frisch created this distinction in reference to the formal models or systems of equations, whereas Schumpeter used the non-equivalent concepts of

'external' and 'internal' forces, defined in relation to the scope of the theory itself and depending on the limits of what could be explained in this way. In short, the difference was that, for Schumpeter, causality was not equivalent to mechanical implication, which was the only form of determination Frisch could accept within the framework of his model.

Indeed, Frisch worked within the confines of formal mathematical models, whereas Schumpeter worked according to narrative and appreciative theories,[12] and consequently their discussion was largely a case of mistaken identities and untranslatable languages. For the former, the heuristic richness of the metaphor was related precisely to its capacity to impose discipline in the mathematical formalism of the cycles. For Schumpeter, this was the tragedy of the model: it could not account for change, the only relevant subject matter. Nevertheless, paradoxically, the rhetoric used by the authors provided the possible space for their actually communicating – indeed, this case highlights the importance of their use of metaphors in order to create a shared conceptual platform and to understand each other's arguments. In spite of their misunderstandings and difficulties, this is a fascinating example of a rich conversation between economists using different analytical tools.

Yet a solution that would satisfy both parties was particularly difficult to achieve, since there was a second decisive difference in their positions: Schumpeter was in fact rather naively searching for a very complex system to represent the process of innovation. If the pendulum were adapted to encompass changes in its suspension point as well – as a result of the cycle itself, it should be noted, so that the trend would indeed be indistinguishable from the oscillations – and if its mass were also to increase or its shape were to be deformed as part of the effects of the 'inner vibratory system', this obviously implied a nonlinear representation. Of course, Schumpeter argued in favour of this solution while remaining unsure of how to proceed, since he did not and could not formally represent that model. It required and still requires an adventurous journey into the unknown, like the one Magellan made into *mare incognita*. And Frisch knew this better than anyone else at that time.

Epilogue to the discussion

As the epistolary discussion went on, Frisch tried to address Schumpeter's reservations by presenting alternative versions of his pendulum model. But the illustration he offered was just a rhetorical approach, since it deeply diverged from his favoured strategy for the mechanical representation of cycles. Indeed, he considered forced oscillations to be an erroneous representation of the cycles.

In a paper prepared as this discussion was going on, Frisch rejected considering forced oscillations in a very outspoken manner:

> Here it cannot, so far as I understand, any longer be a question of a forced oscillation. The bundle of phenomena we call business cycle is, I believe, a complex we have to attack as composed of free oscillations if we as econo-

mists are ever able to understand it. The explanations of the cyclical character of the oscillation must be sought in the inner structure of the system.

(quoted in Andvig, 1986: 137)

The reason for this strategy is that 'in a free oscillation both the periodic length and the general cyclical properties of the oscillation which we want to explain are caused by intrinsic properties of the system' (ibid.: 134) and, consequently:

> To remain in the pendulum illustration, the difference between the two types of problems may be explained in the following way. If the pendulum initially is at rest and I walk up to it and give it a kick, then two things happen: (i) the fact that I kick the pendulum, this is the subject of the impulse problem; that may be explained by my mood at the moment and things like that; (ii) the fact that the pendulum when first moving follows its typical trajectory has nothing to do with my state of mind: it has something to do with the inner structure of the pendulum. And that is the subject of the propagation scheme.
>
> (ibid.: 134–5)

This inner propensity to oscillate is a puzzle. In fact, it was in order to contemplate Schumpeter's resistance against his rocking horse or pendulum metaphor that Frisch considered the forcing term – innovations. This was of course rejected by Schumpeter, who could not conceive of this forcing process as exogenous. Yet, what Frisch considered to be the crucial property of the system was its inner capacity to define the trajectory of the oscillation, through the damping mechanism determining the amplitude and convergence to equilibrium. In other words, Schumpeter valued the inner property of change, and Frisch the inner property of adaptation after change.

Later on, while preparing his PPIP and just three months before finishing the manuscript, Frisch reconsidered this polemic in his Poincaré Lectures and once again emphasised his preference for a model that did not include forced oscillations.[13] By rejecting models based on forced oscillations (e.g. Jevons on sunspots and Moore on the crises influenced by the phases of Venus), he clearly indicated his preference for free oscillations, in which 'the system grows in a sense by itself', proving 'as such a theory can explain as the past determines the future' (1933g: 25). Therefore, closed systems should be used for modelling, in particular 'systems determined by themselves and whose movements are determined by the intrinsic structure of the system', allowing for the study of impulsion as well (1933j: 1). In this sense, Frisch was developing the insights of Wicksell, Akerman, Slutsky and Yule (ibid.: 5).

This lecture is a fascinating epitaph to the previous discussion with Schumpeter, since it proves Frisch clearly understood their divergences. In contrast to his own bias towards free oscillations, Schumpeter opted for another strategy: 'Another current of ideas, in relation to the nature of energies maintaining the economic oscillations is the Schumpeterian idea' of innovations cumulating and

generating the expansion (ibid.: 5, 38). According to Frisch, this could eventually be represented as a van der Pol type of relaxation oscillation (ibid.), which produces a limit cycle.[14]

In the same lecture, Frisch again presented his model for the Schumpeterian pendulum in order to represent innovations as the source of energy, but emphasised that this was not very clear:

> I will give you a mechanical example which may be sufficient to define how Schumpeterian innovations may be conceived of as sources of energy. As far as I am concerned, I found this mechanical illustration very useful. Indeed, it was only after I built this analogy that I could well understand the idea of Schumpeter. *Prolonged conversations with Schumpeter himself could not make this absolutely clear for me.* Naturally, this is a personal remark.
>
> (ibid.: 38, my italics)[15]

The mechanical illustration provided what prolonged conversations could not.

Although the topic was never again discussed properly, Frisch and Schumpeter maintained their respective positions and, writing in reference to the previous exchange, elaborated on them two years later. On 25 October 1933, Frisch wrote to Schumpeter, announcing the conclusion of his paper in honour of Cassel, 'Propagation Problems and Impulse Problems in Dynamic Economics' (PPIP), a paper he had sent to the publisher the previous June. The text again mentioned the two types of impulses, random shocks and Schumpeterian innovations, and added that such a distinction had 'satisfied you to a considerable extent':

> You will probably remember our long correspondence back and forth about the pendulum analogy in business cycles. You will perhaps also remember that I developed a mechanical model, that satisfied you to a considerable extent, expressing that feature of the business cycle which you have particularly insisted upon and which you found was not present in the example with the ordinary pendulum hit by erratic shocks. In a rather big paper to be published in the volume in honour of Cassel I have insisted upon these two ways of looking upon the maintenance problem: on the one hand the idea of erratic shocks (starting with Wicksell, being developed by Slutsky and perhaps having been carried to a sort of relative completion by my theory of linear operators and erratic shocks soon to be published in Econometrica) and on the other hand your idea of the stream of energy coming in through the 'innovations'. I hope you will be satisfied with my mention of your ideas in this field. In the paper in the Cassel volume I was not able to devote more than a brief section to your theory . . . but I hope that I have succeeded in exhibiting *the gist of your view-point as contrasted with the viewpoint of erratic shocks*.
>
> (Frisch to Schumpeter, 25 October 1933)

It is obvious that Frisch minimised or ignored the objections previously raised by Schumpeter in his letter of 17 July 1931. In his 1933 paper, Frisch was trying to clarify and develop his model of cycles, and for this purpose he used both the metaphor of the rocking horse (with Slutskian shocks as the source of energy) and that of the pendulum (with Schumpeterian innovations as responsible for the generation of the movement). The pendulum was evoked in order to describe the second 'source of energy' maintaining the oscillations and acting in a 'more continuous fashion' than the random shocks. Frisch went so far as to mention that 'After long conversations and correspondence with Professor Schumpeter I believe the analogy may be taken as a fair representation of his point of view' (Frisch, 1933a: 203).

At the end of the 1933 paper, Frisch considered some of Schumpeter's points very briefly. Recognising that the analogy provided a picture of an oscillatory system, but 'not of the ['irreversible'] secular or perhaps supersecular tendency of evolutions', Frisch suggested that a simple solution would be to make the suspension point a consequence of the movement itself, so that the trend would be generated by the cycle. Within such a framework, 'there will be an intimate connection between the oscillations and the irreversible evolution' (ibid.: 205). Nevertheless, although insinuating that it would be a simple task, Frisch decided neither to formulate this mathematical model nor to elaborate on it, restricting his own work to the discussion of the simpler case.

Throughout his life, Frisch argued for this general approach to cycles and indeed considered it to be one of his major contributions to economics. In fact, his model established the linear stochastic differential or difference equations as the most suitable representation of the cycles, and buried the contemporary alternative nonlinear models. This was a major part of his writings on cycles and economic evolution, as well as part of his teaching.

Schumpeter took a long time to reply to the October letter, since he was travelling at that time. In December 1933, after a digression on the subject of the baroque and medieval cathedrals of France, he added only a few lines politely alluding to his reservations in relation to the solution suggested by Frisch:

> I am greatly [.] looking forward to both your papers, the one on the erratic shocks (if these are only small, many, independent!) and the other in the Cassel volume . . . from which I hope to derive the usual help in my perplexities.
>
> (Schumpeter to Frisch, 28 December 1933)

Later on, in the preparation and writing of his *Business Cycles*, Schumpeter repeatedly returned to the same perplexity, implicitly indicating a completely alternative solution to the mechanical device of Frisch. The *leitmotiv* was obvious: 'It [innovation] is an internal factor because the turning of the existing factors of production to new users is a purely economic process and, in capitalist society, purely a matter of business behaviour' (Schumpeter, 1939: 86). As a consequence, the innovative process of change and destruction should be

modelled as an internal feature of capitalism, and this would be the proper explanation in economics (ibid.: 7). Furthermore, he argued for a definite rejection of the mechanistic metaphor, since the relevant external events could not be appropriately represented as random shocks on a pendulum: 'But the influence of external factors is never absent. And never are they of such a nature that we could dispose of them according to a scheme of, say, a pendulum continually exposed to numerous small and independent shocks' (ibid.: 12). This is the clearest indication of his rejection of one of the decisive features of this mechanistic metaphor.

But Schumpeter took pains to explain that Frisch's model of impulse and propagation was really quite distinct from the available alternatives, namely from the allegedly *perpetuum mobile* systems, such as the one proposed by Kalecki. He went so far as to attempt to distinguish Frisch's model from those of Wicksell, which had served as the early inspirations for the rocking horse, and that of Slutsky (ibid.: 181fn., 189, 560fn.). The reason for such complacency is difficult to explain, although one may hypothesise that Schumpeter essentially wanted to preserve the feeling of intellectual closeness to Frisch, the only major econometrician to welcome his *Business Cycles*.

Finally, in his *History of Economic Analysis*, Schumpeter suggested a metaphoric shift, insinuating that the crux of the question was the limited value of the available mathematical representations. His distance from the mechanical analogies was expressed in an inspiring manifesto against reductionism, which suggested a new and alternative metaphor, that of the violin being played by a gifted musician:

> It has been said above that macrodynamics helps us to understand mechanisms of propagation. It will perhaps assist the reader if he will look upon the economic system as a sort of resonator, which reacts to the impact of disturbing or 'irritating' events in a manner that is partly determined by its physical structure. Think for instance of a violin which 'reacts' in a determined manner when 'irritated' as the player applies the bow. Understanding the laws of this reaction contributes to a complete 'explanation' of the phenomenon that we call a violin concert. But evidently this contribution, even if reinforced by the contribution of the neurophysiologist, does not explain the whole of it: aesthetic evaluation and the like apart, there is a range of purely scientific ground that acoustics and physiology are constitutionally unable to cover.
>
> (Schumpeter, 1954: 1167–8)

And here Schumpeter introduced a powerful critique of the claim of the unlimited explanatory power of formal models:

> Similarly macrodynamics, while quite essential to an explanation of cyclical phenomena, suffers from definite limitations:[16] its cyclical models are what acoustic models of resonators are for the violin concert. But its votaries will

> not see this. They construct macrodynamic models that are to explain all there is to explain, for economists, in the cyclical phenomena. The very attempt to do so involves several definite errors of fact.[17] And flimsy structures based upon arbitrary assumptions are immediately 'applied' and presented as guides to policy, a practice that of course completes the list of reasons for irritation in the opposite camp. One sometimes has the impression that there are only two groups of economists: those who do not understand a difference equation; and those who understand nothing else. It is therefore a hope, rather than a prognosis to be presently fulfilled, which I am expressing if I venture to say that this entirely unnecessary barrier – but one which is no novelty in our science – to fertilizing interaction will vanish by virtue of the logic of things.
>
> (ibid.)

This was the methodological stance of Schumpeter towards the end of his life: he strongly but nostalgically argued for a *Sozialoekonomie*, combining concrete historical inquiry with theoretical practice, statistical research and inference. Structural change, irreversibility and history, all this should be part of the general vision of economics – precisely the conditions he had tried to impose on the pendulum metaphor earlier.

Now, the reader may accept that this is a convenient although rather dubious epilogue to the story of an intense, fruitful and almost completely ignored discussion on the foundations of the econometric programme for the analysis of cycles. Schumpeter was apparently under the impression that the mathematical capacities of his friend and colleague restricted his thoughts to a narrow domain and prevented any consideration of the decisive qualitative features of innovation under capitalism. In spite of this, he was conditioned by the public claim, made in Frisch's influential 1933 paper, that the pendulum accurately represented his own point of view. He chose not to challenge this claim. Yet he repeatedly stressed that a mechanistic representation could not incorporate change, evolution and irreversibility in economics so well as the aesthetic pleasure of a violin concert – and that the explanation was still somewhere submerged in the immense oceans of Magellan's fantasies or dreams.

As a consequence, this episode highlights the crucial role of metaphors as a way of directing the construction of the argument, its formal representation and the definition of possible alternatives. Although these metaphors were unable to solve the puzzle that Schumpeter and Frisch were discussing, they provided the framework for the dialogue. And they were also invoked by several other economists.

The hidden implications of pendula

Swinging all the way through the history of modern science, pendula became an epitome of oscillations in different natural or social processes. Marshall, for one, used a very peculiar pendulum in order to indicate the complexity of economic processes: in his view, the understanding of purposeful action, particularly if

superimposed on the real-life complexity of natural processes, lay outside the scope of formal reasoning that the common economic models were able to develop. Here is how he presented his argument:

> But in real life such oscillations are seldom as rhythmical as those of a stone hanging freely from a string; the comparison would be more exact if the string were supposed to hang in the troubled waters of a mill-race, whose stream was at one time allowed to flow freely, and at another partially cut off. Nor are these complexities sufficient to illustrate all the disturbances with which the economist and the merchant alike are forced to concern themselves. If the person holding the string swings his hand with movements partly rhythmical and partly arbitrary, the illustration will not outrun the difficulties of some very real and practical problems of value. For indeed the demand and supply schedules do not in practice remain unchanged for a long time together, but are constantly being changed; and every change in them alters the equilibrium amount and the equilibrium price, and thus gives new positions to the centres about which the amount and the price tend to oscillate.
>
> (Marshall, 1890: 288–9)

The outcome of this very complex process of human and natural turbulent flows – sometimes controlled and sometimes free – and the intentional action of the person holding the string are indeterminate: they can either tend towards equilibrium or aggravate disequilibrium. Indeed, Marshall introduced this argument precisely in order to emphasise the difficulties encountered in trying to include the time dimension in economic reasoning: this humanly controlled pendulum depended on will, chance and opportunity.

Akerman, whose dissertation provoked Frisch to suggest the pendulum-like metaphor of the rocking horse, described social evolution as a stream of liquid accelerating over an uneven riverbed. And Schumpeter, as shown above, preferred to consider 'the economic system as a sort of resonator, which reacts to the impact of disturbing or "irritating" events in a manner that is partly determined by its physical structure', and he believed that such a resonator could be 'for instance a violin which "reacts" in a determined manner when "irritated" as the player applies the bow' (Schumpeter, 1954: 1167–8).

Table 6.1 summarised these metaphors that were suggested along the same lines as those described by Marshall.

However, these suggestions were generally ignored: they did not impress the scientists who were engaged in quantitative and statistical analysis, or in theorising the new econometric and probabilistic approach, and preferred a clearly defined framework for the analysis of evolutionary processes. Consequently, the pendulum metaphor was interpreted instead in economics as a purely mechanical representation, as the *leitmotiv* for an irreducibility of real processes. One of the most remarkable triumphs of this dominant version of the intriguing pendulum is how it came to be transformed into the simplistic framework of mechani-

Table 6.1 Pendulum: non-mechanistic versions

Metaphors	*Literary and heuristic treatment of the metaphor*	*Formal treatment of the primary subject*	*Comment*
1 Pendulum driven by purposeful human action	Marshall, 1890	–	Complicated or chaotic movement
2 Stream of fluid flowing in an uneven riverbed	Akerman, 1928	–	Turbulence
3 Violin	Schumpeter, posthumously 1954	–	No formal model

cal modelling, organising the following research into cycles. And here the main feature was Frisch's approach to mechanistic processes.

Table 6.2 presents the most relevant examples of this new generation of metaphors, and emphasises Frisch's role in their elaboration and modelling.

This second line of argument was based on a shift of emphasis, from a narrative approach to complexity towards analytical simplicity: first the intuitive functioning of the pendulum and then the well-researched mechanical properties of

Table 6.2 Mechanistic metaphors in the early analysis of business cycles

Metaphors	*Literary and heuristic treatment of the metaphor*	*Formal treatment of the primary subject*	*Comment*
1 Simple pendulum for the representation of cycles	Fisher on Pareto (1911)	–	Oscillation
2 Simple pendulum with friction, hit by shocks	Yule (1927), Hotelling (1927), Frisch (1933a), Tinbergen (1935)	Frisch, 1933a	Maintained oscillation
3 Rocking horse	Wicksell (1918), Akerman (1928), Frisch (1931, 1933a)	Frisch, 1933a	Maintained oscillation
4 Chain of pendula	Frisch (1927a, 1928)	–	Chaos
5 Double pendulum	Frisch, manuscript notes (1932a)	–	Chaos
6 Forced pendulum	Frisch, interpreting Schumpeter (1931)	–	Chaos
7 Triple pendulum	Frisch (1932–3 and 1950a), interpreting Marshall	Graphical treatment in Frisch, 1950a	Chaos

the simple damping pendulum were invoked as a representation of the movement towards equilibrium. Consequently, the metaphor was developed as a powerful heuristic for the equilibrating mechanism, under the equivalent forms of the simple dissipative pendulum or that of the rocking horse, both subjected to friction, as well as to shocks maintaining the oscillation. The first interpretations in the same sense had occurred very early on: Fisher described Pareto's 1899 model of business cycles as an analogue for the pendulum (Fisher, 1911: 70 fn.), and Pietri-Tonelli used the metaphor of the pendulum for the representation of cycles in 1911 (Pietri-Tonelli, 1911: 220). This is how Yule described his model some years later:

> unfortunately boys get into the room and start pelting the pendulum with peas, sometimes from one side and sometimes from the other. The motion is now affected, not by *superposed fluctuation* but by *time disturbances*, and the effect on the graph will be of an entirely different kind. The graph will remain surprisingly smooth, but amplitude and phase will vary continually.
>
> (Yule, 1927: 268)

The irregularity of the graphs describing real processes was consequently explained by the superimposition of these small shocks. Yet Hotelling understood that this metaphor introduced an element of uncertainty, related to the skill and determination of the boys. Therefore, the implication could be much the same as the one that Marshall had deduced:

> Like a weight suspended from a spring, an index of the business cycle moves up and down, but as when the spring is in the hands of a small boy, one can never be quite sure what is going to happen next.
>
> (Hotelling, 1927: 290)

So, the metaphor was also used to explain uncertainty, the unpredictable variation of events and their effects on the economy. Ragnar Frisch put an end to these divagations and, towards the end of the 1920s and in the early 1930s imposed a new concept of dissipation – describing the process of convergence to a stable equilibrium – in which he defined random shocks as the means to maintain the oscillations. Therefore, Yule's hypothesis became computable and Marshall and Hotelling's uncertainty was suppressed. Along the way, a new and decisive revolution was introduced into economics with the acceptance of the adequacy of the probabilistic approach to time series.

It was Frisch who took the decisive step forward. By the 1930s, he was the driving force behind the formalisation of the metaphor and the establishment of linear differential, difference or mixed systems of equations as the legitimate mode of argument in the analysis of economic fluctuations. Indeed, Frisch is the only name appearing in the third column of Table 6.2, which indicates the formal treatment of the primary subject.[18] The exceptions, such as Akerman's riverbed or Schumpeter's violin, were literary excursions suggesting, as

Marshall did, the inadequacies of the mechanistic metaphor. As they suggested quite another language, these metaphors or critiques were easily disregarded because they were so far removed from the rigour that econometrics was already requiring and beginning to establish. But these alternative metaphors were also ignored because they were at odds with the then prevalent econometric approach. Consequently, the predominance of this simplistic alternative was such that the available nonlinear models were hastily dismissed and only reconsidered some decades later.

Finally, Table 6.2 also includes some of the afterthoughts, such as Frisch's representation of cycles in the distinctive Marshallian time dimensions as a triple pendulum. Although this was not discussed in any great detail, Frisch obviously believed the example to be in line with his previous work on the matter. He was, however, wrong on that score. The hidden implications of these pendula are the topic to which we now turn. But before we do so, it is time to call Slutsky onto the stage: when Frisch prepared his Cassel paper, he was aware of Slutsky's conclusions and sought to provide an alternative.

Enter Doctor Slutsky

When preparing his 1933 paper, Frisch had already looked at the 1927 article sent to him by Slutsky, and then read the 1932 translation by Schultz. Very impressed by this work, he decided to publish the article in *Econometrica* and had even promised to include it in the very first issue.[19]

One year after receiving Slutsky's paper, Frisch addressed the Poincaré Institute in Paris on his theory of oscillations, just as he was finishing PPIP. His fifth conference dealt with 'The creation of cycles by random shocks – A synthesis between the probabilistic point of view and the points of view of dynamic laws'. Frisch pointed out that the 'very common procedure' of taking a moving average of a series could have serious consequences of imposing correlation where none existed, creating as a consequence spurious periodic movements (1933k: 1–2).

> Following from that, and it is a very interesting idea, precisely from the point of view of the interpretation of the energy maintaining the economic oscillations, one can conceive that *nature* by itself proceeds as if it applied a linear operation to the random shocks building the time series we observe.
>
> (ibid.: 7)

Frisch added that the problem was also relevant for physics.[20] This is a very strange statement: although the author does not commit himself to any of the interpretations, the suggestion of nature acting as a moving-averager forbids considering cycles as a mere statistical artefact derived from mathematical procedures. In the same lecture, Frisch did not develop the point any further.[21] Yet, Frisch retained Slutsky's intuition on the role of random shocks and suggested they should be considered in order to explain the changes in amplitude of the cycles, as well as their non-convergence to equilibrium.

The importance of Slutsky's paper lies in its innovative statistical analysis, from which the author suggested an alternative approach to the study of the origin of fluctuations. According to Slutsky, two possible origins could motivate cyclical behaviour: the existence of deterministic and structurally created cycles, or the averaging of random shocks through time. The second cause was the object of his study, as it was the object of Frisch's, but he addressed the issue in a totally different way. In fact, his argument was not absolutely original: two years before the publication of this article, Irving Fisher, arguing that there was no cycle but only a 'dance of the dollar' above and below the trend, had concluded that the oscillations were nothing more than 'Monte Carlo Casino's cycles' (1925: 191–2). But this was just an intuition, and not an articulated argument. Fisher, who was also the dean of the young community of the founders of econometrics, proclaimed it in order to devalue the research being conducted into cycles. No later than four years later, Fisher's theoretical argument and personal fortune suffered a devastating blow with the Wall Street collapse. Instead, Slutsky was at the same time taking it seriously, and provided some mathematical foundations for the casino argument.

Slutsky considered pure random shocks and even suggested 'giv[ing] up the hypothesis of the superposition of regular waves complicated only by purely random components' (Slutsky, 1937: 107) – clearly distancing himself from the strategy Frisch would follow later on. In his paper, the sole process of random disturbances created the irregular fluctuations: using several series from the Russian lottery and other sources, Slutsky was able to prove that the summation of random processes could create cyclical patterns with approximate regularity (ibid.: 105–8). He then suggested that moving average methods could as a consequence show cycles where none originally existed.

Slutsky's scheme may be interpreted in several distinct ways. In the original sense, it is a model generating artificial cycles, which exist only in statistical representation through the imposition of linear filtering (moving averages of other) procedures. Consequently, it is a vigorous alert against spurious statistical results, and it is inscribed in the tradition of George Yule's work.

Furthermore, Slutsky also showed that, for some cycles, the summation of these random causes could imitate the harmonic series of a small number of sinusoidal curves, but that, after a limited period, a new and radically different regime was established (ibid.: 123). These very sharp changes do not correspond, of course, to the phenomena observed for most historical periods. Otherwise, if the economy is considered to be like a dampening and stabilising system, as in the Frischian model, then the random shocks can be conceived of as being averaged and dissipated by the very functioning of the economy, and the cycle is supposed to happen in real terms. This was not necessarily Slutsky's contention, but it was certainly what Frisch made of it, and it is still the interpretation followed by more recent models, such as Lucas's: the linear filtering of random shocks (to the money supply) creates autocorrelated fluctuations, and therefore involves all other variables in a cyclical process.

The argument could therefore be accommodated to the idea of a dampening internal mechanism leading to a normal state of equilibrium, upon which some external shocks impinged, creating oscillations. Frisch consequently argued that the economy could be modelled as a mechanism (markets) with a stable rest state and a tendency towards equilibration (market-clearing processes). If the economy was conceived of as gravitating around equilibrium and cycles were viewed as the outcome of shocks moving the economy away from the centre of gravitation, then the combination of some degree of realism and a general equilibrium approach was still possible. Moreover, as Frisch openly proclaimed, 'if fully worked out, I believe that this idea will give an interesting synthesis between the stochastic point of view and the point of view of rigidly determined dynamical laws' (Frisch, 1933a: 197–8). Thus, the theory of cycles became a central point for the inclusion of neoclassical economics in the time series domain, following the probabilistic quantum revolution and the parallel econometric revolution.[22]

Frisch considered the intrinsic structure of the market economy to have a dampening tendency determining the length of the cycles; but the causes of the oscillation, nevertheless, were the external shocks, the 'source of energy in maintaining oscillations . . . a stream of erratic shocks that constantly upsets the continuous evolution, and by so doing introduces into the system the energy necessary to maintain the swings' (ibid.). The problem was then to explain how those shocks were accumulated and transformed by the weight system provided by the internal mechanism (ibid.: 202–3). The analogy illuminating this process was the movement of a rocking horse: the deterministic part of the economy is represented by the dampening propagating mechanism (the wooden horse), while irregular cycles are created by the impulse system of stochastic and external shocks (the force applied to the horse) (ibid.: 198).

The mechanical analogy inspired the Frischian synthesis of determinism and randomness: external causal forces create the impulses, while the propagation system is the mechanism that accounts for the stabilising properties and the convergence towards equilibrium. The requirements of orthodox epistemology were met: causality was clearly defined and attributed and, although the primary cause was considered unknowable since it was exogenous, stability and equilibrium were guaranteed by a controllable specification of the model, which was the domain of a practicable econometric study. The inquiry was therefore restricted to defining the hypotheses about the behaviour of the shocks, estimating the deterministic part of the model, generating a series from it, and comparing such a series with reality in order to confirm the theory.

The modern theories of cycles are the legacy of the implicit polemics between Frisch and Slutsky. For Frisch, the rocking horse was useful for understanding both the stationary processes with the underlying oscillatory but dampening mechanism and energising shocks. His opposition to Schumpeter's radical view of endogenous technological shocks was directed at establishing an operational linear model of the oscillatory system. For a time, this heuristics proved useful to economists. The Klein–Goldberger model, translated by Adelman and Adelman to the Frischian framework, was the reference for a decade or so: the

shocks were supposed to maintain oscillations of the dampened solution of the system of simultaneous equations (Adelman and Adelman, 1959). In this context, the dampening structure is the deterministic system, but the size and distribution of the impulses determine the form of the oscillations. Consequently, this stream of shocks cannot be represented as a 'residual'. However, the model was criticised mainly for its lack of realism: the emergent symmetry of upswings and downswings contradicts reality, and this was taken as an argument for nonlinear models (Blatt, 1980; Louçã, 1997).

Alternatively, Slutsky generated irregular cycles as a result of random shocks combining over time. 'Is it possible that a definite structure of a connection between random fluctuations could form them into a system of more or less regular waves?' he asked – and answered affirmatively (Slutsky, 1937: 106). Series of white noise may be transformed into sine waves. This was substantially different from Frisch, and not only because Slutsky did not engage himself in any hypothesis about the economic nature of the autoregressive process responsible for the transformation of disorder into order. In fact, there is a more substantial difference: the impulse-propagation model is based on an endogenous correlation-inducing system, whereas the Slutsky cycle-propagation model depends on exogenously fixed correlations.

The rocking horse that does not rock

The paper for the Cassel *Festschrift*, on 'Propagation Problems and Impulse Problems' (PPIP), consolidated Frisch's assessment of economic oscillations.[23] A preliminary version was presented at the Econometric Conference in Leiden (1933), and greatly impressed the audience, being debated by Machlup, Koopmans, Kalecki, Divisia and Schultz. Frisch was convinced that this was a ground-breaking contribution.[24]

After the introduction, sections two and three present an economic theory for the rocking horse – a three-dimensional deterministic system representing the accumulation of capital, money and the structure of lags in the production of capital equipment[25] – and simulated its cycles under defined parameters. The rocking horse was kept in movement given the erratic shocks, 'a source of energy in maintaining the oscillations' (Frisch, 1933a: 197).

This rocking horse metaphor was originally suggested in a footnote by Wicksell, and then referred to by Akerman in his doctoral thesis. Both references would probably have been condemned to obscurity if Frisch had not considered Wicksell to be a great economist and if he had not been a member of the jury examining Akerman's thesis in 1928: he quickly incorporated the metaphor into his own research and developed a seminal model of cycles inspired by this insight.[26] Curiously enough, in spite of its relevance for the dissemination of the piece for the Cassel *Festschrift*, this metaphor of the rocking horse did not play any role in Frisch's correspondence with Schumpeter; indeed, it was expressed on paper only some time later.

Strictly speaking, there was no substantial analytical difference between the dissipative pendulum and the rocking horse, since both were conceived of as mechanisms filtering and damping free oscillations, although the analogy of the horse suggested a somewhat more interventionist impulse system. Yet Frisch's intellectual strategy was precisely based on the antinomy between the role of Slutsky's moving average of random shocks, which generated change, and the stabilising properties of the body of the system, which reduced such impacts to the precise form of the cycle. As a consequence, the movement of a damping propagation mechanism was represented by the wooden horse, which was supposed to be under the impact of frequent kicks making it rock.

In fact, Frisch used several different vivid metaphors in order to indicate to Schumpeter the nature of the mathematical argument, which was otherwise rather obscure for his colleague. When Frisch corresponded with Schumpeter, it was obvious for both that this rocking horse, moved by the unexplainable kicks, could not represent major systemic changes, most particularly the bursts of innovation Schumpeter had in mind. Instead, Frisch used a peculiar version of the previously accepted metaphor, which was to dominate his construction of the argument – that of a pendulum hit by exogenous shocks. This metaphor became a powerful heuristic device that contributed to the orientation of future research: it was in fact more suitable, since the rocking horse suggested the dominance of a damping mechanism, while the pendulum suggested instead the greater influence of 'innovations'.

In any case, the model is a masterpiece of ingenuity, but it could not be solved analytically. Consequently, Frisch and his assistants went through a painful exercise of simulation, generating cycles from the model (Figure 6.3).

The fact that, under different parametric specifications, the model could indeed produce oscillations was seen by Frisch as a confirmation of its accuracy. Furthermore, he looked in the space of parameters for those achieving a good match with the patterns of short Kitchin, Juglar and Kondratiev cycles.[27] Simulation was being reinvented in cycle theory.

This model has been analysed and criticised on three main grounds. First, it chose to ignore the nonlinearity in the monetary equation, in order to 'sanctify' the modelling methodology based on linear systems (Velupillai, 1992: 58). Velupillai emphasises that Frisch's innovation was not the economic explanation, actually derived from Aftalion, but the 'theoretical technology' based on a linear system with random shocks, free oscillation and dynamics studied in terms of the intrinsic oscillatory properties of the propagation mechanism. The economic problem was consequently reinterpreted as an energy problem: without the impulses, the system would tend to rest in equilibrium (ibid.: 59, 61). Otherwise, if nonlinearity was considered, the system could be conceived of as producing endogenous oscillations (ibid.: 65).[28] There were obvious alternatives to this linear specification: (i) relaxation oscillations and (ii) a forced oscillator. Both were accessible and indeed Frisch explored both, although inconclusively.

The cycle can be generated as relaxation oscillations, in the van der Pol-Liénard tradition (Corbeiller, 1933: 330; Velupillai, 1992: 68). This general

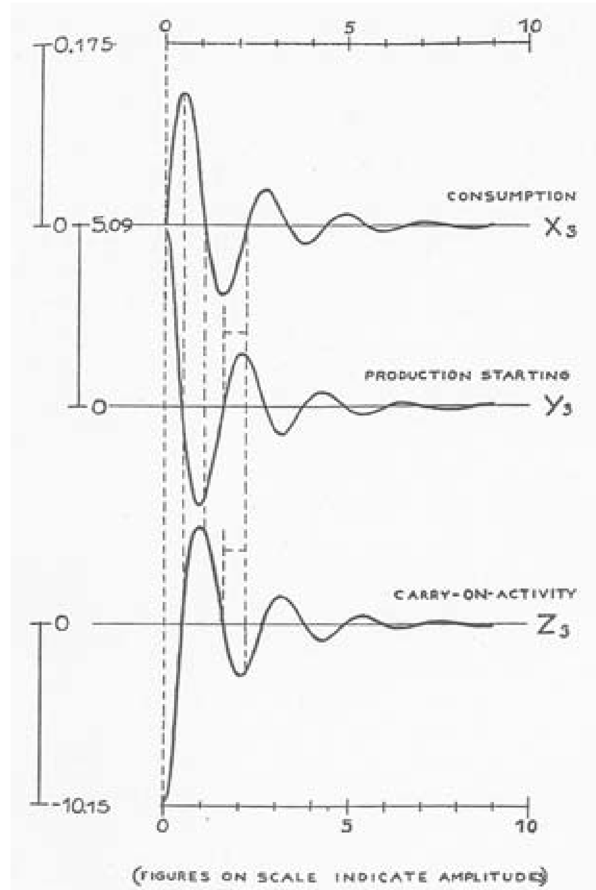

Figure 6.3 Frisch's cycles obtained from the model of PPIP.

approach was available since the econometric group was aware of the work carried out by Le Corbeiller, who was present at the first meeting of the Econometric Society in Lausanne and published his paper in the first issue of *Econometrica*. Although Frisch was greatly interested in it, he apparently never took the time to cooperate with Le Corbeiller in order to work out this alternative. Yet, the authors corresponded for a time and Frisch made careful inquiries in order to understand these points of view. When PPIP was being finished, Frisch wrote to Le Corbeiller:

> It was indeed very kind of you to state with all the details the various cases of oscillatory systems. These indications will no doubt be very helpful to me. The main idea of my proof of the effect of the moving average is the following. I first prove that if a moving average (with any set of weights,

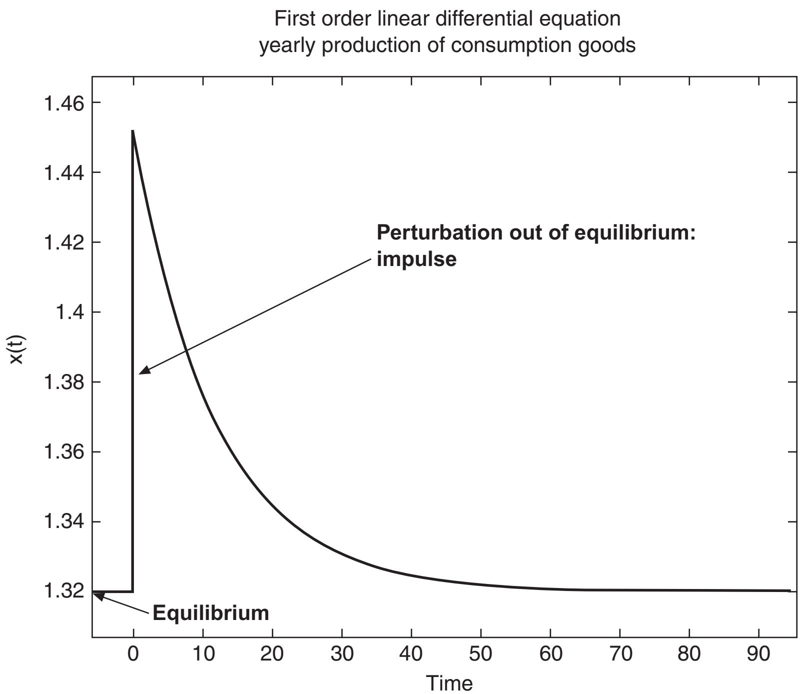

Figure 6.4 Zambelli's representation of the process obtained from PPIP.

symmetric or not) is applied to a time variable that is auto non-correlated, then we obtain a time curve whose spectrum is proportional to the standard deviations of the random variable. By the operation factor I mean the function of the frequency by which the amplitude of a sign [sine?] function is multiplied when the operation in question is applied to it. It is indeed easy to show that if a linear operation is applied to a sign curve two factors are produced: firstly the amplitude is multiplied by a factor (the operation factor) and secondly the face [phase?] is changed. In case the operation is symmetric the change in face is 0. This being so, it is easy to construct the operation curve, for instance, for an unweighted moving average, and thus verify the existence of a fundamental period of about 0.7 of the length of the moving average. Of course the construction of the spectrum as here envisaged is only a preliminary tool in the analysis of the cycles created. We must further discuss such things as the mean amplitude of the cycle created, the fact that this amplitude is itself changing, that it contains a cyclic movement and that this cyclic movement in the amplitude has again a characteristic amplitude and period. These things may also be brought out by a further study of the spectrum and also by a more direct approach, but it would be too long to go into details here. All these

things will be discussed in a paper which I am writing up for the October Econometrica.'

(Frisch to Le Corbeiller, 13 May 1933, my italics)

The paper for *Econometrica* turned out to be PPIP, although these points are not developed in the final version. Neither were Le Corbeiller's indications on oscillatory systems ever used by Frisch. In the same sense, he had corresponded with Hamburger a couple of years before:

> In my own attack to the economic cycle problem I have also been very definitely under the impression that the conception of rigorous harmonic components must be given up [as in the Changing Harmonics paper]. You will see that my approach to the change in the components is purely empirical. *In this respect your approach, built on Dr. Van der Pol's idea, is more powerful, it seems to me, since it introduces some rationality into the explanation of the change*. I want very much to take this idea up through a closer study and try to combine it with my own idea of a 'moving contact approximation'. It seems to me that such a combination ought to furnish a very powerful method.
>
> (Frisch to Hamburger, 6 May 1930, my italics)

There is no indication that Frisch ever 'took the idea up'; in fact, he became very disappointed with Hamburger's approach later on – and this spelled the end of the nonlinearities:

> With regard to Hamburger – I don't think that he will be able to give us anything particularly interesting. You remember of course that he wrote a paper some time ago on van der Pol's Theory of Oscillations. That paper in itself seemed promising but nothing more seems to have come from him so, on the whole, I am a little bit disappointed with him.
>
> (Frisch to Divisia, 11 June 1932)

The rocking-horse model of the propagation cum impulse system obtains a mixed second order differential and first order difference equation, a rather complex system that Frisch could only simulate. The result was an impressive demonstration but an unattainable solution: the model required a rather implausible set of parametric values, otherwise the horse would not rock. Zambelli, the author of the second critique, argued that the propagation mechanism is not 'intrinsically cyclical' and the convergence to equilibrium, after a shock, proceeds in a non-cyclical manner (Zambelli, 1992: 52). Given the too quick dampening, 'the main conclusion is that PPIP is not a model of the cycle or, to use the Wicksell–Frisch's metaphor, it is a wooden horse that wouldn't rock' (ibid.: 27; also Zambelli, forthcoming).

The third critique came from Thalberg. He reconsidered the PPIP model with additive random shocks, normally distributed and serially correlated or uncorrelated, with zero expectation and finite variance: the conclusion was that the shocks maintain the cycle with a high degree of damping, but the cycle itself is very irregular and unpredictable (Thalberg, 1992: 108). When other repercussions are considered in a reformulated model, e.g. the Keynesian effect of investment on consumption, instability grows (ibid.: 110). Even under linear specifications, the conclusions are obviously dependent on the specific modelling strategy and on the values chosen for the parameters, and therefore may lead to rather different implications. Thalberg also concluded that Zambelli's objection could eventually be superseded by the addition of random shocks, since disturbances can maintain the fluctuations even under a strong degree of damping, but that their amplitude depends on the variance of the shocks (Thalberg, 1992: 108).[29]

These points were not considered in the early construction of the model. Yet, after the rocking horse, Frisch introduced the pendulum metaphor into PPIP in order to accommodate the Schumpeterian argument: besides the erratic shocks, another source of energy is 'operating in a more continuous fashion', this being the case with Schumpeter's innovations (ibid.: 203). That this argument was not sufficient to satisfy Schumpeter has already been demonstrated in the previous details of their 1931 correspondence. Schumpeter's idealised pendulum was far too complicated to be represented by a simple mechanical device, as Frisch intended. As Frisch put it in 'The Nature of Time Series', a handwritten note he prepared while researching for PPIP, it was as if Schumpeter was conceiving of 'a little devil sitting on the pendulum and changing its length' – a very Schumpeterian little devil, who was so abstruse that he was silently expelled from the model. Yet, this was not all. Even without changing the structure of the pendulum, the little devil could simply act by imposing a forcing term: the mechanical illustration Frisch offered to Schumpeter was indeed a treacherous solution. Indeed, the equation of the forced pendulum is easily obtained from that of the damped pendulum. It was the simplest way to model the Schumpeterian innovations following Frisch's interpretation, although this was not consistent with the concept of free oscillation under exogenous shocks, which was necessarily lost. It was consequently a pity that Frisch did not compute the equations, merely pointing to the forced pendulum as an illustration of his argument: as a result, he missed the implications of his own model. Indeed, this model of the forced pendulum is not trivial and constitutes a mathematical conundrum.

The simple and the forced pendulum

Since Frisch understood the need to explain the new and extraordinary source of energy represented by innovations, the Schumpeterian pendulum was invoked. But the debate with Schumpeter introduced further entropy into this formulation, since the latter could not accept the idea of a purely exogenous source of energy accounting for innovation and the dynamics of capitalism. As previously mentioned, Frisch tried to convince his colleague of the accuracy of his mechanical metaphor, but then stopped insisting, persuaded as he was that Schumpeter's

ideas were satisfactorily represented by his model and that nothing more could be done to accommodate the latter's lasting reservations.

The equation of the pendulum can be derived from Newton's Second Law or from the First Law of Thermodynamics. Adding the damping factor one obtains:

$$\ddot{\theta}+\beta\,\dot{\theta}+\alpha\sin\theta=0 \tag{1}$$

As the treatment of the solution requires the use of a Jacobian elliptic function, Frisch chose to circumvent this by opting for a linear approximation to the damping pendulum, ignoring all but the first term of the expansion of $sin\theta$. This alternative is, of course, only valid for small oscillations, and Frisch used the following form:

$$\ddot{\theta}+2\beta\,\dot{\theta}+(\alpha^2+\beta^2)\,\theta=0 \tag{2}$$

θ being the angular deviation from the vertical. The general solution for this case is

$$\theta(t)=H\,e^{-\beta t}\sin(\phi+\alpha t) \tag{3}$$

where β is the parameter for friction, α is the frequency, ϕ the phase, and H the amplitude. One naturally obtains complex conjugate roots and therefore an oscillatory regime in the damping system. According to Frisch, the solution to the determinate dynamic system should be interpreted as the weighting system for the accumulation of erratic shocks.

Frisch did not understand that if he added a forcing term to this equation of the simple pendulum he would open the Pandora's Box of unknown mathematical diversions. The Schumpeterian pendulum requires more than a trivial extension of the model of the simple damped pendulum. The following equation represents the external parametric forcing in the nonlinear framework:

$$\ddot{\theta}+\beta\,\dot{\theta}+\sin\theta=\rho\cos\varpi_D t \tag{4}$$

where ρ is the intensity of the driving frequency and ω_D is the angular forcing frequency. As before, this is a dissipative system, but it now has three dimensions, allowing for periodic oscillations and limit cycles, as well as for chaos. For some values, if the driving frequency exceeds the natural frequency, the pendulum locks onto the driving frequency and periodic motion is obtained; but if the driving frequency is slightly inferior to the natural one, then resonance may lead to chaos. In that case, the largest Lyapunov exponent is positive, indicating the presence of chaos, and the sum of the exponents is negative and approximately equivalent to $-\beta=\sum\lambda_i$, indicating dissipation (Baker and Gollub, 1996: 122; Moon, 1987: 157). Kapitaniak, following the Melnikov method, established the necessary conditions for the chaoticity of this system (Kapitaniak, 1991: 123f.). Figure 6.5 shows the behaviour of the solutions of the system for a range of parametric values that pass through critical points:

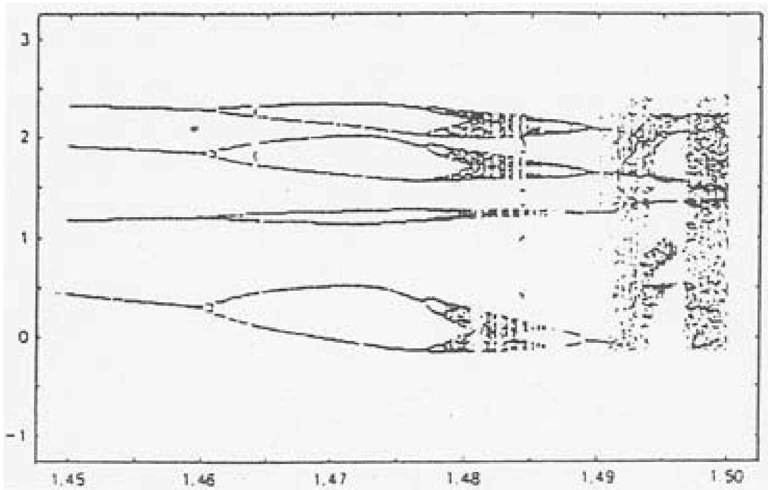

Figure 6.5 Bifurcation map for the double pendulum.

For a range of values of a given parameter, this bifurcation map highlights the effect of the addition of a number of new stationary solutions, since the Jacobian of the function representing the flow acquires eigenvalues with zero real parts at a stationary point. The loss of stability may indicate a route to chaos, as is proved by the study of the behaviour of the latent roots of the Jacobian of the system as the parameter varies (Gandolfo, 1997: 479). Applying the Kaplan-Yorke conjecture, for the values of the parameters considered for this simulation, the Lyapunov dimension is large and positive (Baker and Gollub, 1996; Gandolfo, 1997). As Figure 6.5 shows, we have a period-doubling scenario, the Feigenbaum route to chaos (Eckman, 1981: 650). Under such conditions, the conclusion by Frisch becomes suspect:

> One could even imagine that the movement [after the forcing] would be more than maintained, i.e. that the oscillations would become wilder and wilder, until the instrument breaks down. In order to avoid such a catastrophe one may of course, if necessary, add a dampening mechanism which would tend to stabilize the movement so that the amplitude did not go beyond a certain limit.
>
> (Frisch, 1933a: 204)

The requirement of dampening is no longer sufficient to ascertain the stability of the model, given the problem of coupling between the two frequencies – natural and forcing – which can be aggravated by the disturbances. Moreover, and crucially, nonlinearity may imply sudden changes to the regime of oscillation and the presence of a chaotic attractor. In fact, in the framework of nonlinear

differential systems of equations with three or more dimensions, the traditional concept of equilibrium is lost (Granger and Terasvirta, 1993: 14): catastrophe and chaotic outcomes emerge from the model and we obtain a second explanation for endogenously driven erraticity (Goodwin, 1991: 425). Frisch ignored this since he did not look at the formal representation of the forced pendulum.

Furthermore, the treatment of the nonlinear specification was far removed from the knowledge available in the early 1930s, and he did not have at that time the analytical tools – the iterative simulation by computer – needed to investigate the trajectories of this nonlinear process. So he contented himself with a literary reference to the 'Schumpeterian pendulum', believing that the same general properties would be respected. Yet, they were not: from the simple damping pendulum to the forced one there is a dramatic change, which is the intrusion of chaos.

A room full of rocking horses or pendula

Frisch espoused the point of view of linearity and simplicity and the consequences were clearly assumed: 'The concrete interpretation of the shock e_k does not interest us for the moment' (Frisch, 1933a: 200–1) – and this was the case for the whole paper. Furthermore, the author considered the deterministic oscillation and the perturbations to be completely independent contributions to the composite movement, and the shocks to be independent of each other, so that the final computed deviation would be simply the summation of all the small deviations to the normal trajectory caused by each shock. This additive property was even represented by a number of isolated pendula equivalent to the number of shocks, and this implied the definitive exclusion of one important form of changing harmonics that he had previously considered although not discussed: the coupling effect. The final result was the claim that unexplained independent shocks accounted for the irregularity of the fluctuation: the history of the dynamic process depended on the unproved properties of these external sources of energy.

Consequently, both statistical correlation (of the shocks) and mechanical coupling (the possible resonance of the repeated disturbances with the natural frequency, affecting the amplitude of the movement) were completely disregarded. Indeed, this elimination of correlation and coupling sheds some light on the reason for the cursory treatment of the random shocks, which will be discussed in more detail in Chapter 8. Frisch investigated but could not reach any conclusion on the effects of mechanical coupling.

While preparing the final proofs of PPIP, Frisch engaged in a correspondence with Alfred Cowles, then in charge of the laboratory of the Cowles Commission in Colorado Springs.[30] One of the experiments developed at that laboratory concerned the measurement of the effect of a series of erratic shocks with damped oscillations, which was mentioned to Frisch in a letter on 6 September 1933;[31] the latter asked for clarifications two weeks later. On 9 October Cowles suggested that the matter was equivalent to the task of computing 'a composite of the deviations from equilibrium of a room full of rocking chairs, which are being set in motion at different intervals of time by blows of different intensities'. On 18 October, he insisted that this 'really almost represents the case you had in mind when referring to a pendulum subjected to a stream of erratic shocks. Possibly the idea of a roomful of rocking chairs (or pendulums [sic]) presents a useful concept of what is more likely to be the situation in a complex modern economic system.' Just one week later, on 25 October, Frisch wrote back in order to check his previous results:

> Is it correct to say that the ordinate of the curve at the point of time *t* is the sum of a great number of damped sine curves, each of these being started at some time in the past with an initial ordinate equal to zero and an initial *velocity* equal to some accidentally determined quantity, the point of time where these curves were thus started being also distributed accidentally?

Finally, on 1 November, Frisch acknowledged that this metaphor of the room full of rocking chairs (horses) wonderfully accounted for his model and actually for the concrete mode of computation of the effect of the disturbances, promising to refer to this conclusion and to acknowledge Cowles's work in his forthcoming paper.[32] Cowles concurred: 'The ordinate of the curve at the point of time *t* would be the sum of a greater number of damped sine curves started at erratic intervals with erratically varying velocities'.[33]

Yet Cowles abandoned the measurement project, since it was very difficult to obtain accurate values. Instead, a galvanometer was used at the laboratory: it was adjusted to one cycle with a damping effect, and operated by means of a switch connected to a rheostat in order to represent the variable intensity of the shocks, following a suggestion by Davis.[34] Apparently, Frisch ignored this development, since he was quite happy with the previous result, which he considered to be a sufficient confirmation of his conjectures, although not achieved by empirical means.

The room full of rocking chairs, or of pendula, strongly suggests the relevance of the coupling effect that Frisch had been able to discern in his previous work on the general conditions for changing harmonics. Indeed, in mechanics, the best known phenomenon of coupling was that of two pendula, and the same results applied to the complex setting of the room full of pendula. In both cases, there was apparently no possible way of escaping the problem of resonance: the investigation of the frequency of the disturbances and the dynamic mode locking of the oscillations was the major challenge. And that was why the assumptions regarding the nature of the random shocks were so decisive: transformed into a black box, the insignificant random shocks should necessarily be considered as meaningless in order to perform their important and meaningful theoretical function, to explain the maintenance of the movement. Within such a framework, no query was relevant regarding their nature: they were by definition unquestionable, and that is why they were considered to be explanatory.

Double and triple pendulum

Further work undertaken by Frisch at that time provides further outstanding evidence of his perplexed concern with these strange pendula. One such example is the double pendulum hanging from a spring, represented by Frisch in his notebooks, on 24 August 1932, as 'a gravitational theory of economic phenomena' (Figure 6.6).

If the effect of the spring is ignored and no damping is considered for the sake of simplicity, the system of equations representing the double pendulum – which Frisch could not compute – is as follows:

$$-l_2 m_1(g \sin \theta_2 + l_1 \sin \theta_1 - \theta_2\ \dot{\theta}^2{}_1 + l_1 \cos \theta_1 + l_2\ddot{\theta}_2 - \theta_2\ \ddot{\theta}_1) = 0 \tag{5}$$

$$-l_1[(gm_1 \sin \theta_1 + gm_2 \sin \theta_1 + m_2 l_2 \sin \theta_1 - \theta_2\ \dot{\theta}^2{}_2 + l_1 m_1 \theta_1 + m_2 l_1 \ddot{\theta}{}_1 + l_2 m_2 \cos \theta_1 - \theta_2\ \ddot{\theta}_2)] = 0 \tag{6}$$

The assumptions are massless rods with different lengths (l_i) and masses for each bob (m_i). Under such circumstances, Figure 6.7 represents the plotting over time of both angular deviations from the vertical, indicating that the second pendulum initially transmits energy to the first and then gets energy from it, from an initial condition of a small deviation of the second pendulum (3º) from the vertical.

This simple representation requires the system to be conservative, and in that case each initial condition generates a single orbit (Moon, 1987: 19). It is of course a Hamiltonian system, which preserves the total sum of energy, and for which the attractor is the basin of attraction itself. At low amplitudes, the three-dimensional trajectory lies on a torus, and the KAM theory applies: the system is well behaved for a large range of initial conditions, and for instance it generates

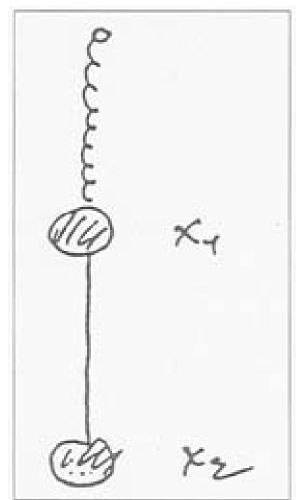

Figure 6.6 Frisch's representation of a 'gravitational theory'.

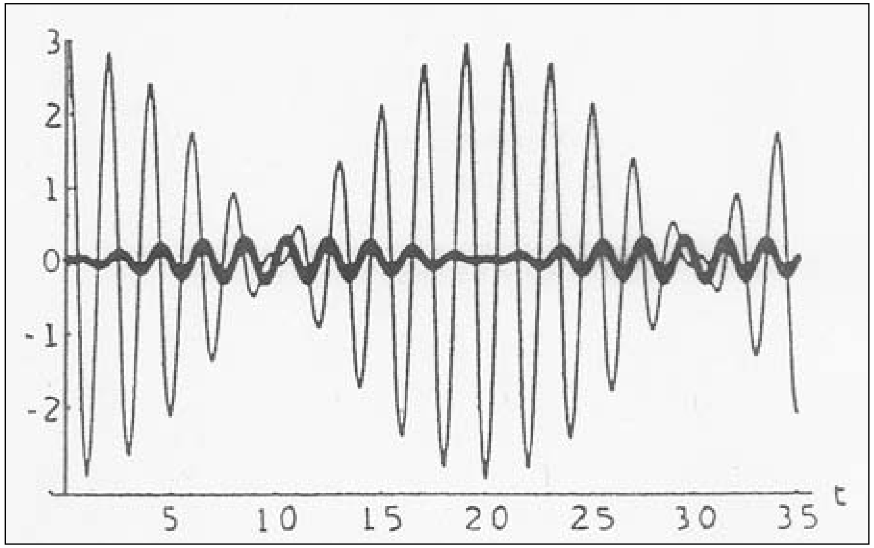

Figure 6.7 Trajectory of the double pendula over time.

periodic motion until almost 63.3º of deviation. But then a transition to chaos occurs near 80º, as exhibited by the phase portrait.

Otherwise, if we consider a dissipative version of the double pendulum, a meaningful concept of chaos requires providing the means for sustaining the movement. Under such a framework, the previous conclusions on the forced pendulum may be generalised to the new case.

At roughly the same time, Frisch also represented in his notebooks a similar ‘interaction between the components’ as a triple pendulum. But neither did he provide any mathematical treatment of this case (just as he did not do so for the double pendulum) nor did he present any simulation of its movement. Yet he returned to this problem some years later, which proves that this was not a minor issue for him. In 1950, at the insistence of Chamberlin, Frisch published an interpretation of Marshall’s theory of value in the *Quarterly Journal of Economics*, based on the 1933–8 lectures he had given on the subject – just after formulating his first hunch on the double and triple pendulum and the conclusion of PPIP.

The paper provides another mechanical analogy based on the distinction between short-term temporary equilibrium, normal equilibrium over short periods and normal equilibrium over long periods. On the assumption that different economic factors determine the price formation for each time dimension, a mechanical illustration was provided in order to interpret the process of value: three pendula hanging from each other, each pendulum being studied as a separate component of the final movement. The assumptions were rather stringent: the larger pendulum did not move in the relevant period for the smaller one, and

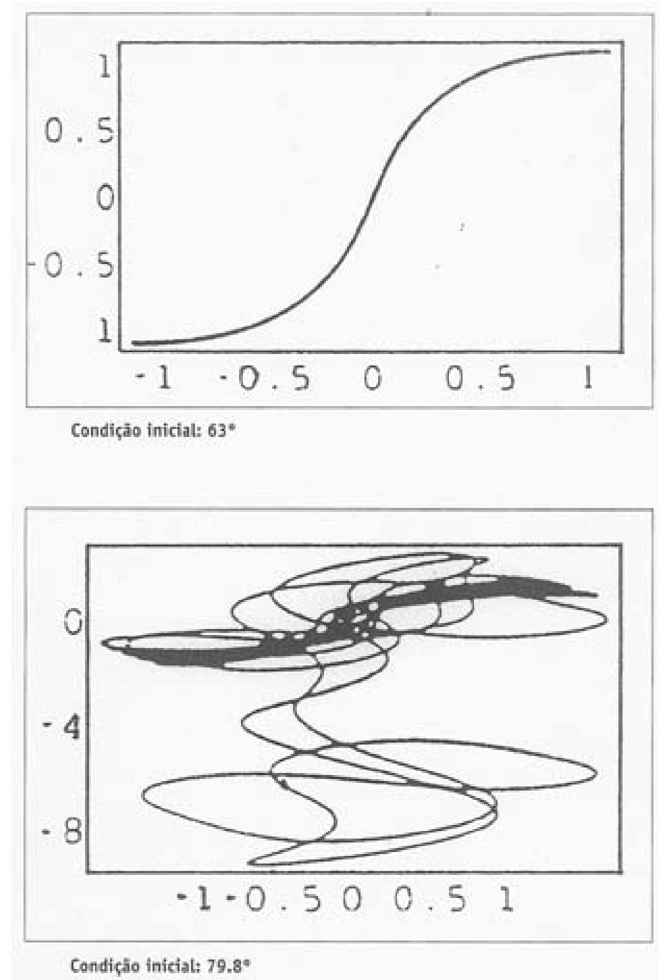

Figure 6.8 Periodic motion and chaos generated by the double pendula.

the latter did not exert any influence at all on the movement of the former (Frisch, 1950a: 496). The linear superimposition and strict independence of the three movements was assumed: ‘When each pendulum is studied in this way, the composite movement can be built from the separate movements’ (ibid.: 497).

According to Frisch, this implied renouncing ‘truly dynamic analysis’ (ibid.: 497–8), and accepting the *ceteris paribus* rule: ‘The motion of each pendulum illustrates the price component *which would be the result* if a certain set of conditions remained constant long enough for the realization of the effects pertaining to these conditions’ (ibid.), just as Marshall had considered (Marshall, 1890: 304).

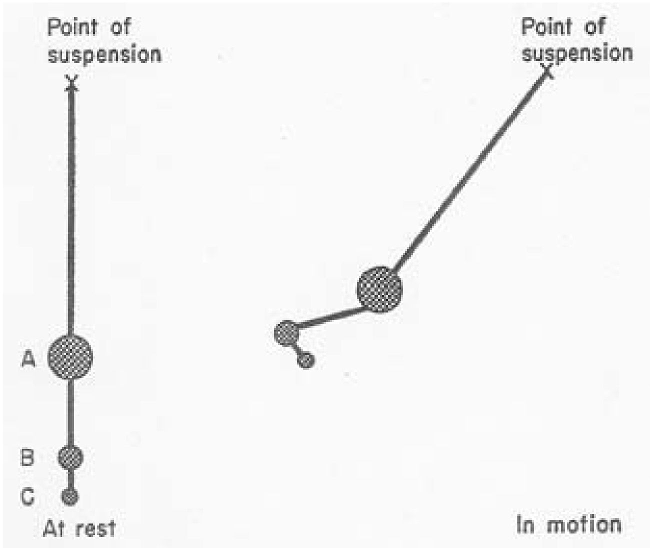

Figure 6.9 Frisch's representation of Marshall's triple pendula.

The system of equations of the triple pendulum is as follows:

$$\ddot{\theta}_1 l_1 \cos(\theta_3-\theta_1)+\ddot{\theta}_2 l_3 \cos(\theta_2-\theta_3)+\ddot{\theta}_3=-g \sin \theta_3-\dot{\theta}_1^{\,2} \sin(\theta_3-\theta_1) l_1+ \dot{\theta}_2^{\,2} l_2 \sin(\theta_2-\theta_3) \tag{7}$$

$$\ddot{\theta}_1(m_2 l_1+m_3 l_1 l_2) \cos(\theta_1-\theta_2)+\ddot{\theta}_2(m_2+m_3 l_2^2)+\ddot{\theta}_3 m_3 l_2 \cos(\theta_2-\theta_3)= -g(m_2+m_3 l_2) \sin \theta_2+\dot{\theta}_1^{\,2}(m_2+m_3 l_1 l_2) \sin(\theta_1-\theta_2)-\dot{\theta}_3^{\,2} m_3 l_2 \sin(\theta_2-\theta_3) \tag{8}$$

$$\ddot{\theta}_1(m_1+l_1^2 m_2+m_3 l_1^2)+\ddot{\theta}_2(m_2 l_1+m_3 l_1 l_2) \cos(\theta_1-\theta_2)+m_3 l_1 \ddot{\theta}_3 \cos(\theta_3-\theta_1)= -g(m_1+m_2 l_1+m_3 l_1) \sin \theta_1-\dot{\theta}_2^{\,2}(m_2 l_1+m_3 l_1 l_2) \sin(\theta_1-\theta_2)-l_1 m_3 \dot{\theta}_3^{\,2} \sin(\theta_1-\theta_3) \tag{9}$$

Since Frisch limited himself to the graphical representation of the model, no further conclusions were to be drawn from it. Yet he felt that under different assumptions his conclusion would not hold: in particular, if dependence between the pendula movements was hypothesised, the linear composition of the movement and the previous results would not hold. Indeed, according to Frisch, the simplistic approximation required a significant difference between the lengths and weights of the three pendula, in order to minimise and even ignore their interaction. But, if instead the pendula were supposed to be quite comparable or if the movement of one impacted on that of the others, a new analysis and theory would be required:

> In this case the system must be seen as a whole and we must study specifically for instance how one pendulum, when swinging, acts as a moving force on the others. Translated into economic language this means that we have to deal *with a truly dynamic analysis of evolution, a theory of progress*.
>
> (Frisch, 1950a: 497, my italics)

Of course, this is the strange feature that we encountered earlier: it is the process of coupling, of changing harmonics! This 'truly dynamic analysis of evolution, a theory of progress' is therefore the only one that is suitable for studying the global behaviour of the system whenever we discard the radical simplifying assumptions, as previously presented. It is also the only one that is suitable, even if we keep these assumptions of large differences among the component pendula but introduce the interaction between them, since the same strange patterns emerge.

The simplistic case of the linear superimposition is just a figment of graphical imagination. Indeed, if 'one pendulum, when swinging, acts as a moving force on the others', we obtain complex resonance between the pendula, which is a generalisation of the process of coupling. That was the intuition of Henri Poincaré, and what he discussed under the heading of the three-body problem. Yet nothing indicates that Frisch or other economic mathematicians knew of this contribution.[35] In this case, as in those of the forced and the double pendulum, a chaotic regime may emerge, with a positive Lyapunov exponent indicating sensitivity to initial conditions (Moon, 1987: 93; Tsonis, 1992: 138). It is a possible outcome – and actually a necessary one – for certain ranges of parameters and initial conditions.

Tinbergen to the rescue of the pendulum

While Frisch was preparing his PPIP, Tinbergen was working on the statistical estimation of business cycles. He was the man best prepared to deliver a survey on business cycles for *Econometrica*, and he was asked by Frisch to provide one. And so he did: the survey was published in 1935 and examined the major problems of econometric analysis.

Tinbergen discussed the models of mixed difference-differential equations by Kalecki and Frisch, both presented at the 1933 Leiden conference (Tinbergen, 1935: 268f.). He fully endorsed the distinction between 'mechanism' and 'exterior influences' (or 'impulses') and tried to formulate the concept of impulses so that they could be represented in the model; in that sense, he argued that 'frequently, the impulses present themselves as given initial conditions of the variables – comparable with the shock generating a movement of a pendulum – or as given changes of the data entering the equations' (ibid.: 241–2).

The survey is otherwise very illustrative of the difficulties that the econometricians were facing and proves that they were fully aware of the problem. In particular, Tinbergen made two criticisms of the model that Frisch had built. First, its theoretical foundations were thin:

Its [PPIP's] important feature is that it makes a distinction between the mechanism and the external shocks operating on that mechanism and shows, by a very interesting example, what shapes the cycles appear when such shocks are assumed to occur. For that reason, the special mechanism assumed by Frisch may have been intended to have merely an illustrative character. Its economic foundation is not clear in every point.

(ibid.: 271)

To prove the point, Tinbergen indicated the lack of statistical evidence for the function of the replacement of capital goods and for the notion of *encaisse désirée*. A second critical point was also indicated: the nonlinearities are pervasive and should not be ignored. Quoting Volterra, Tinbergen, concluded that high degree equations and nonlinearities could be pervasive:

First, it may be stated that higher degree equations are very likely to occur in economics. The simple relation between price, quantity sold, and amount paid, is a quadratic relation. As the number of variables included increases, there are many other reasons for obtaining higher degree equations.

(ibid.: 295–6)

In spite of these criticisms, Tinbergen sided with Frisch on the modelling strategy for business cycles. He even indirectly addressed the discussion between Frisch and Schumpeter, as it could be deduced from PPIP itself, trying to define the mathematical conditions for the different alternatives to the Schumpeterian pendulum:

In this respect, they [business cycles policies] belong in the same class as the problems depending on the influence of other ('natural') perturbances of cyclical movements, such as exceptional crop figures. Some of these problems are analogous to 'shock problems' in pendulum physics; another part may be compared to the problem of changing the length of a pendulum. A third category consists in a shifting of the turning point of a pendulum. In mathematical terms, this is about equivalent to the distinction between three sorts of variation problems, (a) variation of the initial condition, (b) variation of the coefficients of one or more of the equations, and (c) variation or introduction of an additive term in one or more of the equations of the system.

(ibid.: 303)

This was not enough to satisfy Schumpeter, who emphasised over and over again that he could not conceive of innovations as shocks in the sense established by probability theory:

To begin with allow me to repeat that on principle I admit an indefinite number of fluctuations in the material which are due to a great variety of

causes and of very different nature and which all interfere with each other in the most complicated ways. What is called my theory of fluctuations is really simple to the point of triteness: for it merely recognizes the action on the economic system of a very great number of factors external to it *which are neither small or independent in the probability sense, and the presence of a process of change internal to the system* which also produces fluctuation of a great variety of periods and amplitudes.

(Schumpeter to Mitchell, 6 May 1937)[36]

Swinging back and forth, the interpretation of business cycles was kept alive by argument and discussion.

The 'little devil' making history

The remarkable impact on economics brought by developments in mechanics, the science of movement, has been studied thoroughly as far as the marginalist revolution is concerned (Mirowski, 1988, 1989a), as it was discussed as a transition to the mathematical paradigm (Israel, 1996; Israel and Ingrao, 1985). In the same sense, here is another instance of this type of metaphorical incorporation: the argument of this chapter has been that these successive versions of the metaphor of the pendulum provided a bridge between the traditional approaches to cyclical movements in economics, and between these and the modes of formal analysis available in physics, which were widely seen as the hallmark of scientificity.

In particular, the metaphor was used to build the econometric revolution and to apply the new mechanistic insights to discussion of the major puzzle in economics: the fluctuations of the state of affairs. Prior to this episode (or in the tradition of previous theoretical trajectories), economics treated the role of agents and the outcome of their social interaction from two competing viewpoints. First, the Walrasian general equilibrium established a clear deduction of macroeconomic behaviour from microeconomic foundations: the rational choices of agents, maximising their utility (or, in a later interpretation, establishing a maximising strategy), would necessarily lead to aggregate states satisfying a criterion of market clearing (or the Nash equilibrium). In this way, simple one-dimensional attractors were obtained. It is well known that, for a long time, this approach was predominantly narrative, and that it was not formally developed until the seminal contributions of Arrow and Debreu (and Nash). The second approach, on the other hand, suggested that economies could be conceived of as trajectories of dynamic systems, to be described by differential or difference equations representing changes over time in the states of the model. In this case, one- or two-dimensional attractors were typically obtained in the available models.

But these approaches were unable to provide explanations for changes in the system over time, as far as the first one was concerned, or for the process of aggregation in the behaviour of agents, in the case of the second one. And they were both dramatically unable to explain the emergence of new patterns of behaviour throughout history, or evolution, as Schumpeter rightly remarked.

The virtue of Frisch's programme – the adoption of the pendulum as the natural explanation of cycles in economics – was that it provided a partial answer, both conceptual and technical, to some of these difficulties. It established a clear demarcation between the domain of the explicable, the mechanism and the domain of the inexplicable impulses impinging on the system. It attributed to the first domain the property of stability, and therefore considered that the trajectory of the endogenous variables represented the path towards equilibrium. It assumed reversible time, since all events were reduced to irrelevant random shocks upon a repetitive mechanism. It imposed a definite epistemic distinction between the explanatory endogenous variables and the causal exogenous variables. Based on this distinction, it provided the means for formal treatment of the statistical series: the double decomposition between growth and cycle, on the one hand, and between propagation and impulses in cycles, on the other hand.

Yet, this programme could not satisfactorily address structural change either in the statistical series or in models of social interactions. Consequently, Schumpeter challenged Frisch's ability to represent his concept of innovation and the process of creative destruction. As a response, Frisch defined a parametrically forced damped pendulum in order to give a precise content to the shocks, represented as Schumpeterian innovations, which led to a crucial deviation in relation to the original properties of the model. Later on, as seen in this chapter, he toyed with the idea of the double and the triple pendulum, in order to provide a rather simplistic framework for describing some distinctive natural frequencies as modes of temporal oscillation.

Appendix

Distinguished heirs: chaos, Lucas and real business cycles

The rocking horse and the pendulum metaphors generated a distinguished line of research and, for a very long time, provided a canon for the analysis of economic oscillations. In these concluding remarks, two of the modern consequences are brought to the attention of the reader. First, the following section recapitulates the hidden implication of nonlinear and chaotic oscillations in the intriguing pendulum. Second, the contemporary discussion taking place on the Frischian heritage is briefly presented.

Chaos: does it matter?

On the basis of the Schumpeterian pendulum, Cars Hommes suggested that our current chaotic models of cycles are inherited from Frisch: 'The nonlinear pendulum described by Frisch presumably can exhibit complicated erratic dynamics. Therefore, one may view the recent contributions on "chaos in economics" within

the same line of thought already suggested by Frisch' (Hommes, 1991: 276). This is highly questionable: the distinctive appeal of these models is their endogenous generation of very complex patterns, impossible to find in the linear framework, and consequently quite far removed from the traditional explanation by Frisch. For the shocks cum propagation approach, instability is necessarily exogenous to the system, whereas in the nonlinear framework one discovers a possible endogenous source of permanent instability. Indeed, more than one researcher has blamed the success of PPIP for delaying the consideration of alternative nonlinear models.[37] Indeed, the chaotic implication of the Schumpeterian pendulum was totally ignored by all the participants in the discussion at that time.

In fact, it is precisely this challenging reunification of the contexts of explanation and causation that has recently encouraged a number of scholars to investigate alternative models and to depart from the Frischian framework.[38] Endogenous explanations have tended to supersede the previously accepted explanations of exogenous changes within an endogenously driven, stable equilibrium system. Consequently, Hommes's argument is largely *ad hoc*. Indeed, chaos is persistent instability, which is the exact opposite of what the simple versions of the pendulum were striving to demonstrate.

For the general case, this genetic difference is recognised at first glance by Benhabib: 'at first blush . . . cyclical and chaotic dynamics do not sit well with the idea of strict economic equilibrium' (Benhabib, 1992: 3). But he also argues that both the traditional and the complexity approach are compatible and complementary:

> It is more helpful to consider endogenously oscillatory dynamics as complementary to the role of stochastic elements in accounting for economic fluctuations. After all, it does not really make a big difference if endogenous mechanisms by themselves generate regular or irregular persistent oscillations or whether they give rise to damped oscillations that are sustained by stochastic shocks.
>
> (ibid.)

Of course, if the option depends on the intellectual strategy used for designing the model and not on any meaningful feature of reality, the dichotomy is relative to the space of the representation, as argued by Sims:

> Whether fluctuations are endogenously or exogenously generated, stochastic or deterministic, is a property of a model, not of the real world. Only if there were a true model, in much more precise correspondence with the real world than are macroeconomic models, might be a useful shorthand to speak of the actual business cycle as being 'stochastic' or 'deterministic'.
>
> (Sims, 1994: 1886)

Does it really not matter? At night all cats are grey. I myself presume that it does matter. If economic models are designed to provide insights to be explored for the analysis of real-life processes, then the nonlinear framework is obviously more suitable than the linear model for investigating interactions among agents, since it suggests the emergence of new properties. Complex dynamic systems may be more suited for the analysis of social change – and evidence shows that Frisch, Tinbergen, Divisia and other econometricians understood this, but inevitably used linear models since they were more tractable.

In spite of the intrinsic simplicity imposed by the deterministic character of the equations generating the orbits, the recourse to chaotic models requires major paradigmatic changes in economics, particularly in five domains. First, unlike traditional dynamic models, intertemporal arbitrage leads to non-equilibrium in this case, and, furthermore, even agents with rational expectations cannot avoid sensitive dependence on initial conditions (Gandolfo, 1997: 530). Second, it is recognised that inhabitants of the fat tails of the distribution typically drive the processes of change, i.e. that catastrophes may very well occur (Arthur *et al.*, 1997: 5f.). Third, the rich process of social interaction that is modelled requires a new vision of the very evolution of the formation of expectations: there is an ecological dynamics of the population of interpretative devices available to the agents, which are part of their nonlinear adaptive networks or complex adaptive systems. Fourth, new econometric models and nonlinear inferential techniques are required in view of the drastic reduction in the confidence interval of the forecasts: 'Instead, what is needed are new classes of combinatorial mathematics and populations-level stochastic processes, in conjunction with computer modelling' (ibid.: 4). Fifth, statistical inference itself is subject to severe restrictions, given the irreversible nature of the processes under scrutiny, or the changes occurring in real history. This was the motivation for Joan Robinson's powerful arguments against the pendulum metaphor a quarter of century ago:

> Once we admit that an economy exists in time, that history goes one way from the irreversible past into the unknown future, the conception of equilibrium based on the mechanical analogy of a pendulum swinging to and fro in space becomes untenable. The whole of traditional economics needs to be thought afresh.
>
> (Robinson, 1973a: 5)

Besides challenging the concept of equilibrium, the implications of the chaotic nature of the models implicit in Frisch's literary excursions away from the simple damped pendulum are outstanding. First, in the cases of both the forced pendulum and the double and triple pendulum, the system is not necessarily moved by random shocks and endogenously determines its trajectories; furthermore, it is not necessarily driven back to an asymptotically stable equilibrium. Depending on the assumptions about the parameters, it can move further away from equilibrium and generate new patterns of organisation that can only be understood within the framework of the model's complex resonance. Second, free oscillation provided a useful and self-evident distinction

between endogenous and exogenous variables, the former being responsible for the understanding of the intrinsic oscillatory properties and the latter for the maintenance of the movement. By way of contrast, forced oscillation in the chaotic regime blurs this distinction: it is not only random shocks that may eventually be considered, but also a second type of shock is introduced, as in the case of the 'Schumpeterian pendulum', characterised by a certain structure, its specific frequency and resonance with the natural frequency. In this case, nonlinearity, representing the mode of interaction between the variables, is itself responsible for moving it into unpredictable trajectories. The metaphor of the pendulum, imposed by a strategy for the reduction of economics to simplicity, could paradoxically favour the task of thinking economics afresh, as requested by Robinson.

As previously noted, Frisch did not attempt to deal with these complex cases: nonlinearity was still a long way ahead and the problem had just been recognised. It was indeed Schumpeter who acknowledged the intrinsic limitations of the mechanical analogy, more so than Frisch, but no further implications were discussed regarding the nature of the most suitable theory. Once again, Frisch stopped at the edge of chaos.

The paradox is that he was in good company – including some of those whose lack of formalism he tried so hard to address and supersede. Marshall, for one, clearly understood the limits of the analogy with the free oscillations of the simple pendulum. He therefore argued that in realistic descriptions one should take into account the will of the person hanging the pendulum as well as the turbulent flows of the environment: rhythmic as well as arbitrary movements would thus obtain (Marshall, 1890: 288–9). And since this was imposed by the very nature of the flow of time, dynamics was considered to be the proper method for conducting the investigation into evolution: that was the Mecca for economics. It has not been sufficiently emphasised that this required a new type of non-deterministic dynamics, but Schumpeter intuitively understood the difficulty. That is why he resisted Frisch's intense efforts to incorporate his theory of innovations into the pendulum metaphor. In his view, a dynamic study of innovations should concern the inner vibratory system, the deformations, and the long-term trends – the emergence of new properties of self-organisation, in modern parlance.

In other words, for both Marshall and Schumpeter, realism required dynamic nonlinear models, somewhere between the stone hanging over a turbulent river and the rocking-horse metaphors. There was a trade-off between the richness of this insight into the organic structure of real economies and its computation requiring simplicity; and, since both were desired, static approximations and dynamic narrative descriptions coexisted for a time. But true complex dynamic models, in the sense of the Marshallian version of the pendulum metaphor, required a different kind of intellectual framework, one that favoured historical inquiry into irreversibility and change, as well as the study of local attractors. It also implied moving away from the general conclusions about the global properties of equilibrium. Heterogeneity instead of homogeneity, and strategies instead of universal patterns, needed to be considered, so that they could generate a new heuristic programme replacing, once and for all, the self-satisfying assumption of perfect rationality.

Frisch, on the other hand, tried to construct a new theory based on well-researched mechanical metaphors and their rigorous mathematical representation. But he quickly reached the limits of the metaphor: the forced, the double and the triple pendulum, used to represent Schumpeter and Marshall's theories, led to a rather difficult dynamic nonlinearity. He was therefore pushed back to the beginning, to an appreciative narrative of the complexity of economic cycles. Victorious in his drive for the mathematisation of the discipline of economics, Frisch found himself confronted with the intrinsic limits of his endeavour: the immense and still unexplored Magellanic oceans of economic theory.

Frisch, Slutsky and Lucas

Three successive models of business cycles and their respective foundational metaphors were briefly considered until now. The first is the image of the lake, suggested by Walras to the young Schumpeter. It provided a framework, based upon the distinction between structure and behaviour, or mechanics and environmental influences, and established the operational definition of endogeneity and exogeneity, attributing intelligibility to one and causality to the other. Cycles were consequently described as independent modes of oscillation created by factors alien to the natural equilibration of the system.

Nevertheless, the exact nature of this equilibration process could not be captured by the analogy of the lake. The lake is a given, and the winds producing waves and changes are mostly outside the scope of human action – bucolic contemplation is not the function of economics. Action is required to tame the business cycle, and that was the creed of the next generation of metaphors and metaphorists.

As understanding was supposed, in the positivist universe, to equal the design of a mechanical representation of the phenomenon, Ragnar Frisch suggested a successful alternative: the rocking horse – the most impressive of the mechanical metaphors redefining the field of macroeconomics. It provided the epitome, the model and the procedures for the explanation of cycles. As a consequence, this organising metaphor of the rocking horse, with its exogenous impulses and the inner propagation system that is supposed to represent the real economy, has been the cornerstone of the analytical models of cycles from the early 1930s to the present day. This specification was furthermore essential for the viability of the econometric revolution and the successful incorporation of the concept of randomness into mainstream economics: as in fluid dynamics, equilibrium had been conceived in the first generation of models as a state of rest, while this second generation added energy under the form of random and independent movements of particles. This approach was quite different from Walras's, and yet equilibrium was maintained as the centre of gravitation of the theory itself.

Eugene Slutsky provided the third alternative approach. Slutsky worked in

isolation in Russia and thus his 1927 paper remained largely unknown for a decade, although not by Frisch, who recognised the importance of the paper and made it internationally available. Finally published in *Econometrica* in 1937, the paper suggested that the cycles were a consequence of a sequence of disturbances, the cumulative effect of random elements: quite simply, the exogenous shocks created the cycle just as their correlation created order. Consequently, the stabilising structure of the apparatus was no longer required.

Indeed, there is an opposition between Frisch and Slutsky: for Frisch, it is the structure of propagation that moulds the oscillations; for Slutsky, there is no defined endogenous structure and it is the summation of purely random shocks that constitutes the origin of cycles, generating autocorrelation by statistical averaging. What is the economic explanation for this process is a topic Slutsky does not care to discuss.

For Frisch, it is the inner structure of capitalism that generates oscillations, stimulated only by exogenous shocks, whereas for Slutsky fluctuations emerge from the autocorrelation process imposed on random perturbations. This radical difference is highlighted by several episodes in Frisch's career: his 'Circulation Planning' paper arguing that free markets would lead the economy to collapse (Frisch, 1934a), and his ferocious debate with Tinbergen and Koopmans in 1935 about the self-destructive tendency of capitalism (see Chapter 10).

There are consequently two opposite approaches: an impulse-propagation model, developed by Frisch, and a shock-correlation model, which is followed by Slutsky. For the first one, the oscillatory structure is endogenous, requiring only an addition of energy; for the second, there is no structure, and the creation of autocorrelation, or cycles, derives from a process that is exogenous to the economy. The nature of the economic explanation for both models is naturally contradictory.

Indeed, Frisch's theory provided a synthesis between determinism (the propagation system) and the stochastic view (the impulse system) and defined a clear causal relationship. Of course, this is the pure mechanical conception: the system defines equilibrium as the articulation of all its internal movements and mutual influences and, unless there is an externally motivated change, the result obtained is permanent. The solution of the equations is the formal representation of the equilibrating mechanics. This is also a possible synthesis between positivist determination and the assumption of stability: since equilibrium rules the system, disturbances can only be imposed from outside, as Hayek (1933: 22–3) had stated by then. The consequence of this triumph of mechanics, distinguishing propagation from impulse, was to insulate the economics of growth from the analysis of cycles. This constituted a new departure for mainstream economics, and Samuelson (1947: 284) correctly interpreted this paradigm shift as comparable to the transition from classical to quantum mechanics.

As the lake was obviously inadequate for the representation of economic oscillations, it was ignored by theoreticians. Consequently, the history of the last seventy years of business cycle research can be written as the successive invocations of this dialogue and confrontation between Frisch and Slutsky. Is equilibrium best represented either by the properties of a dampening and equilibrating mechanism moved by shocks or by the process of averaging through time the results drawn from one urn? And which are the economic analogues for such processes? Necessity plus chance, or order out of disorder?

As this debate was being pursued, these metaphors evolved into some *fin-de-siècle* derivatives. This was when a new personage, Robert Lucas, came onto the scene. The extraordinary importance of the contribution made by Robert Lucas lies in his ability to rescue this debate, to select and combine some of its elements, to adapt the reasoning to the neoclassical framework and to present a new generation of models that constituted a breakthrough for mainstream economics. In the wake of the new classical upsurge, based upon rational expectations, Lucas attacked the problem of business cycles as well. Using Slutsky's approach, he extended a stable neoclassical growth model to include stochastic changes in the money supply, which have an impact on the economy through a distributed lag system determined by correlated expectations. The oscillations were consequently created by the impulses, although the impulses were not very clearly defined.

This was revolutionary, for several reasons. First, it announced the end of the divorce between the analyses of trend and cycle. The reconciliation was most welcome and it seemed possible, since the first building block of the new classical model was the growth model itself. Second, and most importantly, it established a new concept of equilibrium, which was badly needed. Following Arrow and Debreu, Lucas and Sargent extended the fixed-point approach from static analysis to dynamics: market clearing and utility maximisation came to define equilibrium.

Real business cycles (RBC) constituted an extreme radicalisation of this programme (Hoover, 1995). Yet, although it constituted an open invitation for reasoning and new inquiries, it failed on two essential counts: the new interlinking of cycles and growth, and the new concept of equilibrium. As cycles were modelled as fluctuations of the natural rate of output, instead of deviations of output from a deterministic trend, the RBC programme cried out for the integration of cycles and growth theories. The conceptualisation of cycles and growth was discussed in the previous pages: not only for technical reasons was the notion of stochastic trend virtually abandoned for practical estimation exercises and replaced by smooth deterministic trends. Growth was rapidly expelled from cycle analysis. But it was the second failure, that of the new approach to equilibrium, that was decisive for the evolution of RBC modelling.

Since Smith and Juglar, macroeconomics has had to explain how serial correlation is imposed on macro variables, inducing co-movement between series. In this sense, the co-movement that is to be explained is of a very peculiar kind: it is a recurrent and specific causal process, showing why recessions follow prosperity and not just any statistical parallel between series. Of course, for the radical followers of Lucas this is an empty question, rejected by the very definition of equilibrium: since economies correspond at any moment to a Pareto optimum, the slump just exists because individuals choose to be in a slump, given their information about productivity. The Great Depression between 1929 and 1933 was thus caused by voluntary and informed unemployment given an allegedly large technological decline, but it was still a constrained Pareto optimum.

Yet one must notice a subtle change in the philosophy of equilibrium, based on the nature of the agents' procedures. Neoclassical economics describes the individual agent as a rational seeker of maximal utility, subject to small errors; the aggregation of these individuals determines the emergence of markets subsuming all social relations under perfect exchange. In such a context, errors represent the path towards frictionless economic relations of perfect trade.

Consequently, the notion of this stable mechanism and that of the natural errors of agents presuppose an adaptive structure dissipating these errors. Cycles, or at least persistent and strong fluctuations as observed, are not the expected forms of adaptation through the dissipation of errors, and the RBC notion of 'forced' equilibrium does not correspond to that criterion. Furthermore, if the economies are modelled as 'floating Walrasian equilibrium, buffeted by productivity shocks' (Summers, 1986), there is not much to be said about each economic conjuncture. The problem is that Robinson Crusoe may well be considered the *prima donna* of the typical representative agent model, but the encapsulation of social relations may require other types of agents and, crucially, the consideration of their institutions.

The debate is of course an old one: Keynes pointed to the fallacy of composition implicit in any aggregation of the sort, and Alan Kirman (1992) argued that the excess demand function of a single individual may not resemble the function of the whole economy: the 'rigorous' microfoundations of equilibrium may be empty. Solow's scepticism can be read as a response to these failures: 'Any interesting and useful solution to that riddle will almost certainly involve an equilibrium concept broader, or at least different from, price-mediated market-clearing' (Solow, 1986: S34).

The RBC programme was also a radical response to the Lucas critique: if fiscal or monetary policy implies a change of parameters and therefore proves the lack of any autonomy of relations, econometric estimation is suspect and a new mode of reasoning is necessary. Consequently, RBC modellers abandoned it and instead used calibration of models, as ideal types of societies, assuming that theoretically interpreted variables allow for counterfactual experiments, and that comparison between models and real series is enough by way of proof. But simulation merely states the consequent: if theory A predicts B and if B is verified, then it is impossible to state that A is the legitimate explanation for B, since competitive theories are not excluded. Although Lucas (1987: 43–5) recognised that the construction of a series designed to mimic actual series does not provide an independent test of the model, and others discussed and criticised the method (Gregory and Smith, 1991; Watson, 1993; Canova, 1994, 1995; Hartley *et al.*, 1997), the truth is that this has been industrially developed as an alternative to econometric testing. The results are far from convincing: in fact, after twenty years of proselytising, New Classical economists now seem more inclined towards endogenous growth than towards RBC.

Lucas, as well as Sargent, refers to Frisch and uses the argument of the authority of the rocking horse, but one may argue that they are closer to Slutsky and not Frisch in their difference of opinions. Lucas provided a reconsideration of the Slutsky argument, both through the valuation of simulation instead of estimation and essentially by suggesting an economic processor of information designed to create cycles out of the shocks. His heirs and critics would soon be faced with the same difficulty of interpreting Frisch and Slutsky.

In a 1990 paper, Kydland and Prescott examine four ancestors of their programme: Mitchell, Frisch, Slutsky and Lucas. Mitchell is criticised as deterministic, and, in particular, the authors challenge his commitment to explaining the succession of phases of the cycle, prosperity leading to crisis and depression, and from there to recovery. Departing from Koopmans' critique of Burns and Mitchell (1946), Kydland and Prescott endorse his main point: a theory is needed before variables are selected. But they disagree with regard to a second one, the presumption that the relevant time series are generated by some probability model and the need to use structural systems of equations – ignoring facts or imposing such a straitjacket was a 'grave disservice' (Kydland and Prescott, 1990: 4). In fact, Kydland and Prescott's view of the adequacy of the model is based on a comparison of simulation with 'facts' and does not favour general procedures of estimation using probabilistic models.

The authors are kinder to the second ancestor: Frisch's model of the rocking horse and pendulum, the system of differential or difference equations with random shocks, gained considerable attention. 'But no one built on that work', since the neoclassical growth model was not available, and the Arrow–Debreu approach was not yet developed; consequently, there was no capacity to develop dynamic general equilibrium schemes. Yet, there is something more, and the argument is particularly important for our earlier discussion in this chapter: 'In contrast with modern business cycle theory, he [Frisch] emphasized damped oscillatory behavior' (ibid.: 4), defining equilibrium as a system of rest. Moreover, in Frisch's model there is neither individual maximisation nor a representative agent, nor necessarily equilibrium.[39]

By contrast, Slutsky proposed 'an entirely different way of generating cycles' as the sum of random causes (ibid.). Kydland and Prescott emphasise: 'Business Cycles are, in the language of Slutsky, the "sum of random causes" ' (1999), and this approach influenced RBC (Prescott, 1986b). Plosser concurs: if Slutsky is followed, the concept of fluctuations should be used instead of that of cycles, since there are no periodic cycles, as we experience an 'accumulation of random events or a stochastic process' instead (Plosser, 1989: 52).

Pace Frisch, the stochastic process is enough to create recurrent cycles, and *pace* Mitchell, depression does not necessarily generate expansion: Slutsky's reflection on cycles is obviously the most interesting for the RBC modellers. And then came Lucas. Based on Slutsky's work, his own work relaunched the interest in business cycle analysis. Kydland and Prescott confront Lucas with Mitchell: 'In contrast with Mitchell's view of business cycles, Lucas does not think in terms of sequences of cycles as inevitable waves in economic activity,

nor does he see a need to distinguish among different phases of the cycle' (Kydland and Prescott, 1990: 4). The authors, of course, favour Lucas's treatment of cycles as deviations of aggregate real output from trend, i.e. from the theoretical path of the neoclassical growth model: the object of the inquiry should be the statistical properties of these co-movements and not the history of the succession of prosperity and recession. Of course, if cycles are by definition Pareto-optimal responses to technological changes, there is no ontological distinction between a depression and an expansion.

The debt is mitigated by the fact that Lucas did not provide all the necessary operational definitions:

> Because economic activity in industrial market economies is characterized by sustained growth, Lucas defines business cycles as deviations of real GNP from trend rather than from some constant or average value. But Lucas does not define trend, so his definition of business cycle deviation is incomplete. What guides our, and we think his, concept of trend is steady state growth theory. With this theory there is exogenous labor-augmenting technological change that occurs at a constant rate.
>
> (Kydland and Prescott, 1990: 53)

Plosser added a fifth character to this play: Hicks. The vindication is overemphasised, as if RBC were no less than the continuation of Hicks's programme:

> The underpinnings of our understanding of economic fluctuations are likely to be found somewhere other than a suitably modified version of the Keynesian model. Indeed, there is a growing body of research in macroeconomics that begins with the idea that in order to understand business cycles, it is important and necessary to understand the characteristics of a perfectly working dynamic economic system. Hicks makes this point quite clearly, arguing that the 'idealized state of dynamic equilibrium . . . gives us a way of assessing the extent or degree of disequilibrium'.
>
> (Plosser, 1989: 71)

In that sense, 'RBC models take the first necessary steps in evaluating and understanding Hicks's "idealized state of dynamic equilibrium" ' (ibid.). It is well known that Hicks's contribution to economics left us with a difficult heritage and that so many were tempted to interpret it in contradictory ways. But the very phrase quoted by Plosser should restrain his enthusiasm, since the 'idealized state of equilibrium' was used by Hicks to understand disequilibrium – and that is a crime of *lèse majesté* in the RBC world.

The eclecticism of these references is an obvious, but nonetheless surprising, feature of the drive to establish the legitimacy of RBC models. Indeed, at least in one respect, there are no forerunners for the equilibrium business cycles (EBC) and RBC programmes: the representation of business cycles as a succession of equilibria is unprecedented. It certainly cannot be traced back to Frisch, or

Slutsky or Hicks. On the contrary: the use of the authority of past masters is paradoxical enough. Although he was closer to Slutsky, as previously stated, Lucas claims allegiance to Frisch when he needs to demonstrate that limited monetary shocks and errors of misperception are able to produce large and persistent effects: a propagation system would amplify the errors – but the rocking horse is a device designed to dampen the shocks, not to amplify them. Faced with the same problem, Prescott calls upon Slutsky, exemplifying that small shocks may produce large changes. But he immediately deviates from Slutsky: 'More specifically, we follow Lucas, in defining the business cycle phenomena as the recurrent fluctuations of output about trend and the co-movements among other aggregate time series. Fluctuations are by definition deviations from some slowly varying path [trend]' (1986b: 21). *Les jeux sont faits.*

The evolution, puzzles and queries of the current main contenders in business cycle theory, different brands of New Classical economics (defending exogenous causation spreading through misperceptions or the impact of supply shocks), the New Keynesian movement (considering endogenous factors for underemployment and recession) and endogenous models of cycles and growth, all refer to the founding contributions of Frisch, Slutsky and Tinbergen. The cycle of the debate on cycles is coming back to its point of departure.

7 Challenging Keynes

The econometric movement builds its trenches

During the 1930s, the years of high theory, Cambridge was the hottest place for theoretical production in economics, becoming the crossroads and meeting point for economic theorists and econometricians. This chapter investigates how different groups and generations reacted to the great challenge of their lives, the social collapse provoked by the recession and the inevitable advance towards world war. There were sound reasons for a convergence of efforts and attitudes between the Keynesians and the econometricians, since many in both groups felt the same sense of social emergency and shared the same vision of economics. Yet it was harsh confrontation rather than fruitful dialogue that emerged from their conversations.

This chapter deals first with the discussions between the members of the Keynesian group themselves and with the 'reconcilers' about the interpretation of the *General Theory*, as well as the effects of the transformation of economics during the 1930s as a result of these discussions. Second, it highlights the contribution of the first generation of econometricians, who argued for a new view of economics as an exact science based on mechanical models and mathematically defined theories, while supporting direct control rather than the indirect steering devices suggested by Keynes.

It also investigates a part of that same history, in particular the triangle formed by Keynes, the reconcilers and the econometricians, focusing on the attitude of the econometricians and, in particular, of Ragnar Frisch and his closest associates in the 1930s, who were involved in the foundation of the Econometric Society and in the mathematical reconstruction of economics.

It has been thoroughly argued that one of the reasons for the widespread and initially unopposed acceptance of the early 'synthetic' interpretations of the 'General Theory' was the strategic choice of the 'reconcilers', who wished for a theoretical truce with neoclassical economics in order to concentrate on the urgent policies needed to combat the Great Depression. Keynes did not campaign against such choices, although he resisted them in some private letters and argued for a radical interpretation of his message, at least in the paper that he wrote for the *Quarterly Journal of Economics* in 1937.

One of the bitterest disputes between Keynes and the econometricians is well known and comprehensively researched: the review he made in 1939 of Tinbergen's work for the League of Nations and the subsequent debate. By that time, Keynes was generally hostile to the recourse to mathematical formalism and made no secret of this. As a consequence, he hastily dismissed Tinbergen's research and other works, even when they were intended to give his theories the authoritative empirical content that would allow for their imposition as policy guidelines. This chapter emphasises the importance of the action that Tinbergen's fellow econometricians took at the time in relation to this controversy, and provides evidence of their concerted attempt to counteract Keynes's criticism.

Yet, in spite of the fact that they rapidly rallied around Tinbergen at the gates of the threatened citadel, a debate had begun among the econometricians about the applicability of the new methods. In this particular regard, the contradictions and the evolution of the ideas held about these issues by the econometricians themselves are frequently misread or wholly ignored. Documentary evidence proves that there was a pluralistic and lively technical and epistemological discussion, in which Frisch, Tinbergen, Lange, Marschak, Divisia and others intervened in order to use Keynes's theories, or to address the same problems, aiming at improved social policies.

The introduction of this type of mathematical model into the framework of Keynesian macro-policies was discussed at two major events: the Oxford meeting of the Econometric Society, at which the first version of the IS-LM model (the curves of equilibrium in the product and monetary markets, as suggested by Hicks in order to represent Keynes's approach) was proposed, and the Cambridge meeting dedicated to the discussion of Tinbergen's work on business cycles. Our story goes from Oxford to Cambridge and from 1936 to 1938–9: the next section presents a very brief outline of the argument, centred on the equilibrium reinterpretation of the *General Theory* (Oxford, 1936); the section after that discusses the emergence of econometrics and the main consequences of the Keynes–Tinbergen debate (Cambridge, 1938); and finally some general conclusions are presented.

Oxford, 1936

The Keynesian heterodoxy had been maturing for some time and its developments were publicly discussed and closely followed by a large number of scholars. In the crucial years from 1930 (*Treatise on Money*, hereafter *TM*) to 1936 (*The General Theory of Employment*, *GT*; other books and papers are referred to by the Roman numeral of the volume of Keynes's works in Moggridge, 1971–89), this movement generated a broad consensus in the profession,[1] as Young (1987), Carabelli (1988), O'Donnell (1989), Moggridge (1992), Skidelsky (1992) and others indicated. Keynes was considered to be the most important and influential economist in the early 1930s, and Keynesianism became the accepted theoretical framework for the analysis of unemployment and the distribution of income.

But not all the features of his new vision were so easily accepted. This was the case, for example, with the option for a causal, sequential, deductive and

predominantly literary mode of analysis, which, at least for Keynes, represented the conclusion of his own intellectual trajectory beginning with his early research on the logic of probability, namely the preparation of the 1921 *Treatise on Probability* (*TP*) and his controversial dispute with Karl Pearson. The econometric programme challenged this view of sequential causality, favouring instead simultaneous determination and a simplistic framework, as encapsulated in the systems of equations approach. An important contribution in the sense of the same criticism was made by the internal neutralisation of the implicit and explicit philosophical implications of Keynes's work and its reduction to elementary mechanical models.

Yet, there is much evidence to suggest that, in the early 1930s, the submission of economic arguments to mathematical formulations was still seen as a difficult, hazardous and potentially unwise move.[2] One may interpret this situation as resulting from the underdevelopment of mathematical economics, and such was certainly the case. But the point is that it also corresponded, at least for some of the economists, to a radical hostility towards reducing the scope of the subject matter of economics to the constraints of available techniques. For some of these economists, such a reduction implied the acceptance of a rather poor set of assumptions, far removed from the questions that the theory was meant to address. Furthermore, the statistical treatment of economic material was still based on rather unclear hypotheses and restrictions imposed on data. Keynes repeatedly expressed this idea, particularly in his private correspondence with Frisch:

> Mathematical economics is such risky stuff as compared with non-mathematical economics, because one is deprived of one's intuition on the one hand, yet there are all kinds of unexpressed unavowed assumptions on the other. Thus I never put much trust in it unless it falls in with my own intuitions; and I am therefore grateful for an author who makes it easier for me to apply this check without too much hard work.
>
> (Keynes to Frisch, 24 February 1932)

It is quite obvious that Keynes's scepticism about the mathematical development of economic theories increased with his experience of policy making and was based on his intuition of the organic unity and complexity of society, as well as his awareness of the dangers of the fallacies of composition. The point is also that his scepticism was widely shared in the profession, for the most disparate reasons, and was certainly accepted by some members of the econometric association (the previously cited examples of Mitchell and Snyder, or even that of Amoroso, to mention yet another founder member of the Econometric Society). But it was not shared by all: Frisch, Tinbergen and the younger generation, the forerunners of econometrics, were deeply dedicated to the mathematisation of the discipline and were the driving forces behind the new organisation.

The subsequent outcome of these crucial Keynesian debates – the choice of the best policy for solving the unemployment problem and, implicitly, the deter-

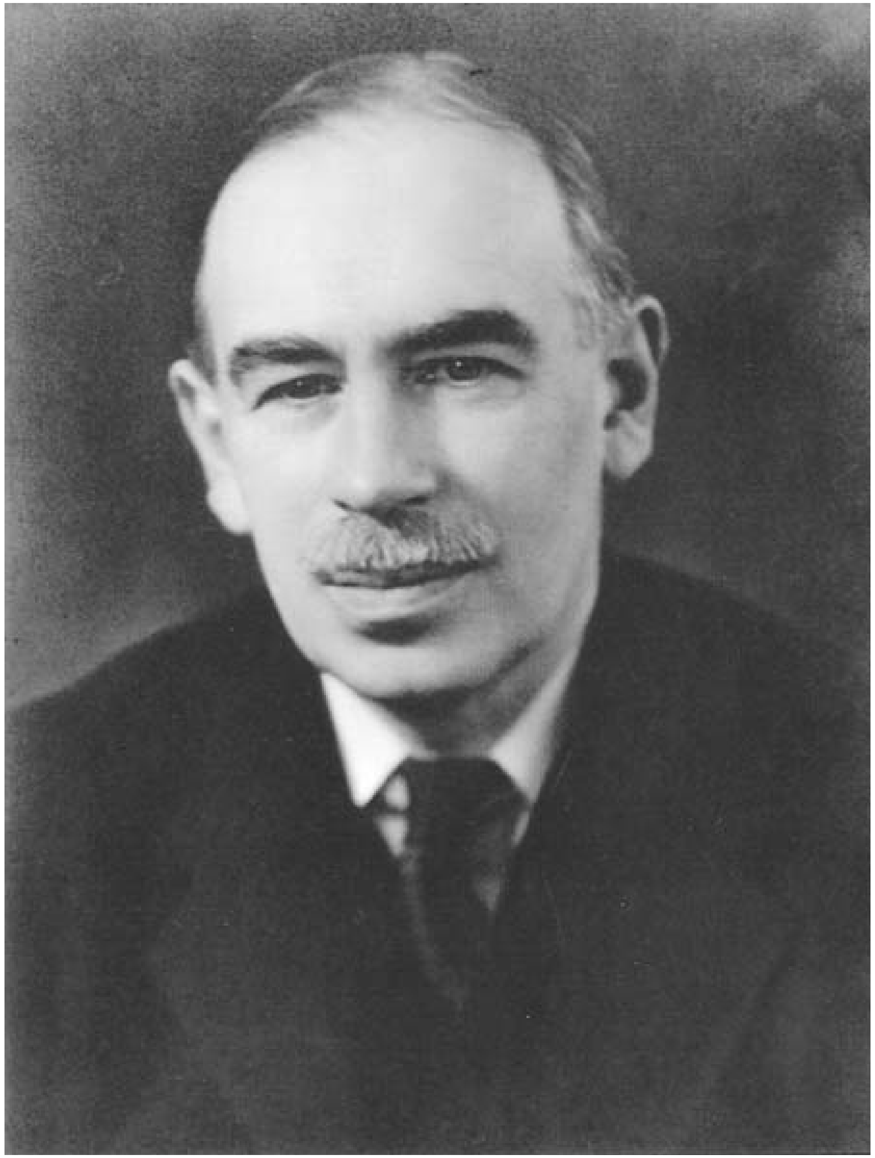

Figure 7.1 John Maynard Keynes, taken in 1936 (source: Dr Milo Keynes).

mination of the subject and purpose of economics, decided the fate of the science over the next few decades. In fact, the acceptance of the urgent need for providing suitable economic advice made it easier to impose toy-macro-models, either in the simple IS-LM approach or in the more elaborate but still low-dimension system of linear equations, such as Tinbergen used. Although this

strategy met with opposition both inside and outside the Econometric Society, this reconceptualisation of economics eventually dominated, and in turn influenced, the subsequent changes in the agenda of economics.

Divergences in normative economics: Keynes and Frisch

Despite the vagueness surrounding the epistemological discussions taking place in the 1930s, it is fair to state that there was no consensus on the need to imitate physics and the natural sciences as the privileged tool for the development of economics. The two opposite camps consisted of very disparate groups. There was, for example, the one including Keynes, Hayek and Mitchell, who fought against the adoption of a general authoritative metaphor for economics taken from physics (and in particular energetics), and another loose group comprising those who were willing to acquire the concepts and to mimic the rigours of physics as the means for delivering exactness and certainty. Indeed, the central role of the analogy with physics – and of the ambition to reproduce the methods of the natural or 'exact' sciences – was acknowledged by the econometricians, just as half a century before it had been acknowledged by the original neoclassicals, and it was seen as part of their specific contribution to the progress of the discipline,[3] as opposed to the traditional modes of economic theorising.

But, unlike the previous generation of theoretical general equilibrium economists, the early econometricians presented their case as both an argument in favour of and the means to be used for economic control: some of these influential young economists were indeed convinced activists. In considering the social problems of their time and consequently the tasks of economics, a vast majority of the protagonists of this story accepted the centrality of the problem of unemployment and even of most Keynesian remedies. This was certainly the case with Frisch in the early 1930s. As early as 1932, in his inaugural lecture as a professor at Oslo University, he explained that:

> quantitative formulation of laws and concepts is very nearly as important in economics [as in natural sciences]. This can be seen most clearly if we consider the final goal of economic theory, which is to clarify the inter-relationship between the various factors, and to do so in such a way as to secure a basis for evaluating what practical measures are most suitable to promote socio-economic aims.
>
> (Frisch, 1932a)

In other words, exactness and mathematical rigour in close imitation of the standards of the natural sciences were necessary in order to provide better policies and sounder economics.

For these authors, the emphasis on the mathematisation of economics flowed directly from this programme for social reform: human beings were called upon to intervene and rule their own affairs, since it was supposed that a pure form of liberal market organisation would imply social disaster. In the draft for a speech prepared in the autumn of 1931, Frisch wrote: 'The depression is a sum of unhappiness and misery, and that is why something has to be done in order to stop this crazy and undignified dance that is the business cycle in a modern capitalist society' (quoted by Andvig 1992: 299). The point, therefore, was that, in order to avoid the 'undignified dance', the modern market should be regulated.

For Frisch, the crisis was a consequence of the inequality and skewness of distribution, both among branches of industry and among social classes.[4] The solution, therefore, was a managed change in social organisation combined with expansive monetary and fiscal policies. As a consequence, Keynes's work was attentively discussed in Frisch's circles: from its publication onwards, and for the next decade, Keynes's *GT* was taught as the basic course of macroeconomics in Norway (Bjerve, 1995: 20), and Frisch had previously used the *TM* for his lectures. But Frisch advocated other forms of economic action, rather than those suggested by Keynes, as a consequence of his understanding of the urgent need for effective action against poverty and unemployment and his awareness of the limitations of the existing policy alternatives. Consequently, he looked elsewhere and developed a new policy proposal in his long 1934 paper in *Econometrica*, emphasising again and again the 'monstrosity' of the situation:

> The most striking paradox of great depressions, and particularly of the present one, is the fact that poverty is imposed on us in the midst of a world of plenty. Many kinds of goods are actually present in large quantities, and other kinds could without any difficulty be brought forth in abundance, if only the available enormous productive power was let loose. Yet, in spite of this technical and physical abundance, most of us are forced to cut down consumption.
>
> [. . .] Of course this implies the conclusion that the cause of great depressions, such as the one we are actually in, is in some way or another connected with the present form of organization of industry and trade.
>
> (Frisch, 1934a: 259)

This paper, 'Circulation Planning', went further than any of his previous contributions arguing for a voluntary scheme of direct and moneyless exchange/barter trade among the various economic agents, under some 'organizer's' supervision. In the following years, Frisch maintained the same analysis of the Great Depression and even extended it to become the rationale for the social and economic engineering he was arguing for. That is why he suspected the indirect steering mechanisms of a Keynesian nature and consequently favoured direct monitoring of the economies.

Furthermore, Frisch's interest in Keynesianism waned during the 1930s. Although he was at first very eager to know Keynes's new work, and of course interested in involving the Keynesian group in the Society, when time came for the publication of the *magnum opus*, the *General Theory*, it did not impress Frisch, since he feared a step backwards. Frisch always considered Keynes's previous work to be far superior to the *GT*, which he thought was not a truly

original book,[5] and furthermore he sincerely thought Keynes had not come up to expectations, given the equilibrium condition adopted in the *GT*. In his tribute to Wicksell, Frisch wrote that when they met in Cambridge, Keynes told him that he had finally decided to equilibrate S = I in his model and he felt deeply disappointed: 'I vividly remember the deception I felt one evening when Keynes told me that he had finally decided to make actual investment by definition equal to actual saving. I am sure this was a step backwards in the *GT* as compared with his *TM*' (Frisch, 1952a: 669).[6] This was of course in line with his own preference for the Stockholm school, although Frisch was forced to deal with equilibrium in his own research into mechanical representations of the cycling properties of the economies. More will be said about this later on.

For Frisch, and obviously at least for some of his colleagues who were involved in the econometric programme, the questions of unemployment and income distribution were decisive: very soon, the Second World War would be seen as the confirmation of their darkest fears. Most of them operated within the framework of these discussions. But, paradoxically, the fact that they desperately wanted to avoid these economic horrors and to prevent the 'monstrosity' caused by 'poverty amidst a world of plenty', a new 'disaster for millions' of human beings, contributed to the downgrading of the Keynesian agenda for economics. Indeed, it facilitated the imposition of equilibrium economics, linked as it was to the only available tools for quantification and estimation. And indeed quantification was required by their approach to economic problems. The inductive statistical treatment of economic data, through estimation of systems of simple linear equations, all this paraphernalia was becoming readily available and the early econometricians were eager to use it.

Based on their experience and theoretical foundations – both the evidence of the crisis of the 1930s and Wicksell's influence, as far as Frisch was concerned – these men knew that disequilibrium was the crucial enigma for real life economics. But their desire to avoid it facilitated recourse to easily computable models and to modes of theorising dominated by the mathematical expertise of the period, which assumed mechanical models from which equilibrium could be derived. In other words, their project was finally transformed by one of the available answers – somehow, the answer changed the nature of their own question. In this sense, both the Keynesians and the reformist inspiration of the early econometricians were defeated, Keynes by the reconcilers and Frisch and Tinbergen by the ensuing evolution of econometrics.

Oxford and the 'frightful tendency to compromise'

As expected, the publication of the *GT* had a profound impact on the profession. It was an impressive achievement: a synthesis of a broad experience in economic observation, explanation and policy making; a recapitulation of some of the most advanced and fruitful conjectures of the time; and an authoritative voice for the economic activism most economists were keen to engage in. But its flaws, its unexplained innovations and changes in relation to the *TM* and its style undermined its influence: a considerable confusion between the dynamic properties of the model – implying disequilibrium – and the comparative static framework in which it was described allowed for many different and contradictory interpretations. Even worse, some of them were not clearly frowned upon by Keynes himself, such as the influential IS-LM equilibrating mechanism: based on this, the 'Keynesian-classical synthesis' reintroduced equilibrium in a matter of years. As authoritative scholars have already investigated this story, this section is limited to indicating some of the evidence on the econometricians' reaction to it.

The first version of what came to be known as the IS-LM scheme was presented by Hicks to the sixth European meeting of the Econometric Society at Oxford, beginning on 26 September 1936, just a few months after the publication of the *General Theory*, together with other papers on the topic by Harrod and Meade. This was a very important meeting, where the most distinguished econometricians presented their research: the sixty-four participants included Frisch, Marschak, Neyman, Haavelmo, the Geneva people (Mendershausen, Staehle, Tinbergen) and many other Europeans. Frisch had a paper on 'Macrodynamic Systems Leading to Permanent Unemployment', on the role of profit in the business cycle, written 'in a quite non-Keynesian vein' (Bjerkholt, 1995: 20). Haavelmo presented his first paper to an Econometric Society meeting, while Jerzy Neyman presented the Neyman–Pearson theory as a 'Survey on Recent Work on Correlation and Covariation', arguing that economics was at the same state as astronomy after Copernicus but before Newton, and defending stochastic calculus as the necessary tool for the standardisation of econometrics.[7]

Acquainted with Meade and Harrod's papers to be presented to the Econometric meeting – and possibly also with Champernowne's (Darity and Young, 1995: 7) – Hicks suggested a formal and geometric representation which established the success of the paper.[8] It was a clear and useful tool; it could be easily adapted to several pedagogical and practical purposes; nevertheless, it was at odds with Keynes's original formulation. In spite of this, the IS-LM and the simultaneous equation interpretation were to become the dominant features in the general interpretation of Keynesianism. The general explanation for such an evolution, as provided by distinct scholars, is that the main followers and disciples of Keynes wanted this to be so, and that those who reacted against this – Joan Robinson, Kahn and Shackle – were very few and very late, since they did not adopt such a position either at the Oxford meeting (in which they did not participate) or immediately afterwards, as they later regretted. The early Keynesians saw the *GT* as a 'machine for policy, and interpreted it primarily as providing a rationale for public spending' (Skidelsky, 1992: 538). In that sense:

> Hicks, Harrod, Meade and Hansen in America, the leading constructors of 'IS-LM' Keynesianism, had a clear motive: to reconcile Keynesians and non-Keynesians, so that the ground for policy could be quickly cleared. These early theoretical models incorporated features which were not at all evident in the magnum opus, but which conformed more closely to orthodox theory. The constructors of these models also thought they were improving the original building.
>
> (ibid.)

This explanation is unreservedly accepted here. But one must add one further point, which has to do with the attractive feature of formalisation, explaining both the rapid spread of these versions and Keynes's lack of concern. And this is emphasised by Hicks's own observation, when he later became disappointed with the scheme, that the diagram was only designed for 'expository purposes'. He then added a crucial point, namely that 'I am sure that if I had not done it, and done it in that way, someone else would have done it very soon after' (Hicks, 1979: 73n.).

A powerful movement towards the construction of formal models was already building up, in spite of Keynes's mistrust. In fact, the core ideas of the *GT* on methodology, uncertainty and evolution challenged those of the newly formed group of econometricians, since they were incompatible with the formal and simpler framework they clearly preferred. But, since many of them shared the overall vision of the *GT*, they intended to prove that it could be framed as an exact model, surpassing Keynes's hesitations in relation to the mathematical formulation of economic theories. Indeed, they thought that this was the only way to move forward, with or without Keynes.

Not all econometricians shared the idea that formulating formal equilibrium models was the only legitimate way for developing macro-theories. But they were strongly attached to the idea that their mathematical treatment was the only adequate means for any scientific explanation, and this was equated with the formulation of mechanical models, in particular for business-cycle analysis, whilst the artificial selection process favoured the offspring of a specific brand of these models, the equilibrium systems.

In the case of Frisch, disequilibrium was part of his dramatic vision of world events and dangers and that was why he argued for direct planning. But his work was dedicated to the formulation of exact and determinate models, whose equilibrium conditions were so decisive for computation. In short, the attention to real world disequilibrium justified the use of thought experiments with equilibrium models.

In that sense, it is possible that other econometricians were ready to propose a model version of the *GT*, as revealed by the preparations for the Econometrics meeting. While organising the meeting in his by then hometown of Oxford, Marschak wrote an illuminating letter to Frisch:[9]

> Incidentally, I had a few days ago a somewhat similar idea – that it would be a good thing to ask one of Keynes's adherents to explain to us in a clear (i.e., mathematical) way the substance of his new book [this sentence was underlined by Frisch, who wrote in the margin: 'excellent!'] which now creates a sensation among English economists.

> I hope that it would be possible to get reporters for at least the following subjects: 1) the main ideas of *Keynes's new book*; I shall ask Kahn, or Meade, or, if you prefer to have Keynes himself, I should suggest that you should write him; 2) on elasticities of substitution . . . R. Allen or Hicks; 3) on imperfect competition, M. Allen or Joan Robinson, or Hicks; 4) definition of income, savings, etc., Lindhal; 5) international relations, by Ohlin, or Harrod, or Lerner.
>
> [. . .] On pp. 297–298 of his new book Keynes makes some nasty and unfounded remarks against mathematical economics. Owing to his enormous influence, that makes our task even more urgent.
>
> (Marschak to Frisch, 8 February 1936)

Note the purpose of Marschak and Frisch: to deliver a 'clear', i.e. mathematical, framework for Keynes's theory.[10] In this sense, the papers on Keynes were presented by Hicks and Harrod, who were not initially supposed to do so, and also by Meade, and were immediately published in the following issues of *Econometrica* (Harrod's in January, Meade's in February and Hicks's in April 1937). They were warmly welcomed, in particular Hicks's: 'I am very glad to have this for *Econometrica*. I think it is an exceedingly valuable paper.'[11] From the available evidence, the discussion at the meeting itself was very intense and highly rated by the participants: the result was seen by some as part of a collective effort to reshape the economic theory of the time. And this explains the priority given to the publication of the papers in *Econometrica*, then edited by Frisch. In contrast, Slutsky's now famous paper took some two years to be published after the translation was ready and the author had added his final corrections.

Frisch embarked on a campaign of letter-writing to try to convince Hicks to include

> an elaborate footnote to be included at the beginning of the paper, explaining what happened in the intensive discussion in Oxford. In particular Lindhal's name should be mentioned. Also perhaps Kalecki and all the English who took an active part. I really think it would be fair to mention these circumstances. It would also be interesting from the new [view?] point of the Econometric Society.[12]

This correspondence also makes it obvious that the final form of Hicks's paper had been subject to major revision, as he stated in the concluding letter on this suggestion:

> With regard to the footnote, I will make some remarks in the proof about a useful discussion at Oxford; but the [problem?] is I can't go very far, because when I came to work it out the things that came out in the discussion didn't lead anywhere, and the version of my paper which was based on those points had to be scrapped. The present version, when it differs from that which I read, has been much more influenced by later discussions at Cambridge than by what happened at Oxford.
>
> (Hicks to Frisch, 1 February 1937)

If this is correct, then one may conclude that the final form reflected much more the opinion of Keynes's inner circle than the outcome of the discussion at the Econometric Society meeting itself. This is quite plausible, since the driving force behind this new approach was a part of the Keynesian group itself. After the Oxford meeting, Cambridge was to assume the predominant role, but the result was the much feared accommodation of Keynes's views.[13]

Cambridge's influence was of course that of Harrod (Skidelsky, 1992: 611), the other main character in this part of the story, both through his paper and through his influence among Cambridge economists. Harrod's own paper was acknowledged by Keynes in a letter dated 30 August 1936 as 'instructive' and 'illuminating' (Keynes, XIV: 84), and the author, just as Hicks had done, interpreted these words as a 'blessing' (Harrod, 1951: 453n.). The episode came about as a consequence of serious efforts made by Harrod to influence the formation of the new theory. Although Kahn and Robinson's cooperation with Keynes was the mainstay in the preparation of the *GT*,[14] Harrod took pains to try to influence the development of the new book through a 'heavy bombardment. . . . These comments were composed with fervour . . . but also with a persistent and implacable zeal to convert him on certain points' (ibid.: 452). In other words, 'My main endeavour was to mitigate his attack on the "classical school". . . . It seemed to me that this was pushing his criticism too far, would make too much dust and would give rise to irrelevant controversies' (ibid.: 453).

Harrod's efforts resulted in a paper which consisted in a reformulation of the Keynesian argument in the general equilibrium framework, claiming that it implied just a 'shift of emphasis' in relation to the traditional theory (Harrod, 1937: 85). Keynes noticed this and, in the same letter to Harrod (30 August 1936), protested against the crucial mistake of ignoring his major contribution: 'You don't mention effective demand. . . . To me the most extraordinary thing, regarded historically, is the complete disappearance of the theory of demand and supply for output as a whole, i.e. the theory of employment, *after* it had been the most discussed thing in economics' (Keynes, XIV: 84). Without effective demand and the employment question, Keynes's general theory became meaningless: the reconciliation implied its misrepresentation and the revenge of the classicals.[15]

Equilibrium was thus being re-established as the disciplinary paradigm for the science of economics. Indeed, the deep involvement of at least some of the influential Keynesians in the definition of anti-unemployment and anti-cyclical policies paved the way for their reconciliation with the equilibrium supporters and for the downgrading of the GT to the status of an exception in the framework of 'classical' economics. This movement was not immediately resisted by Keynes, who simply emphasised his main points (1937), without apparently understanding the general implications of the disputable interpretation.[16] By then, this movement was converging with that of the econometricians.

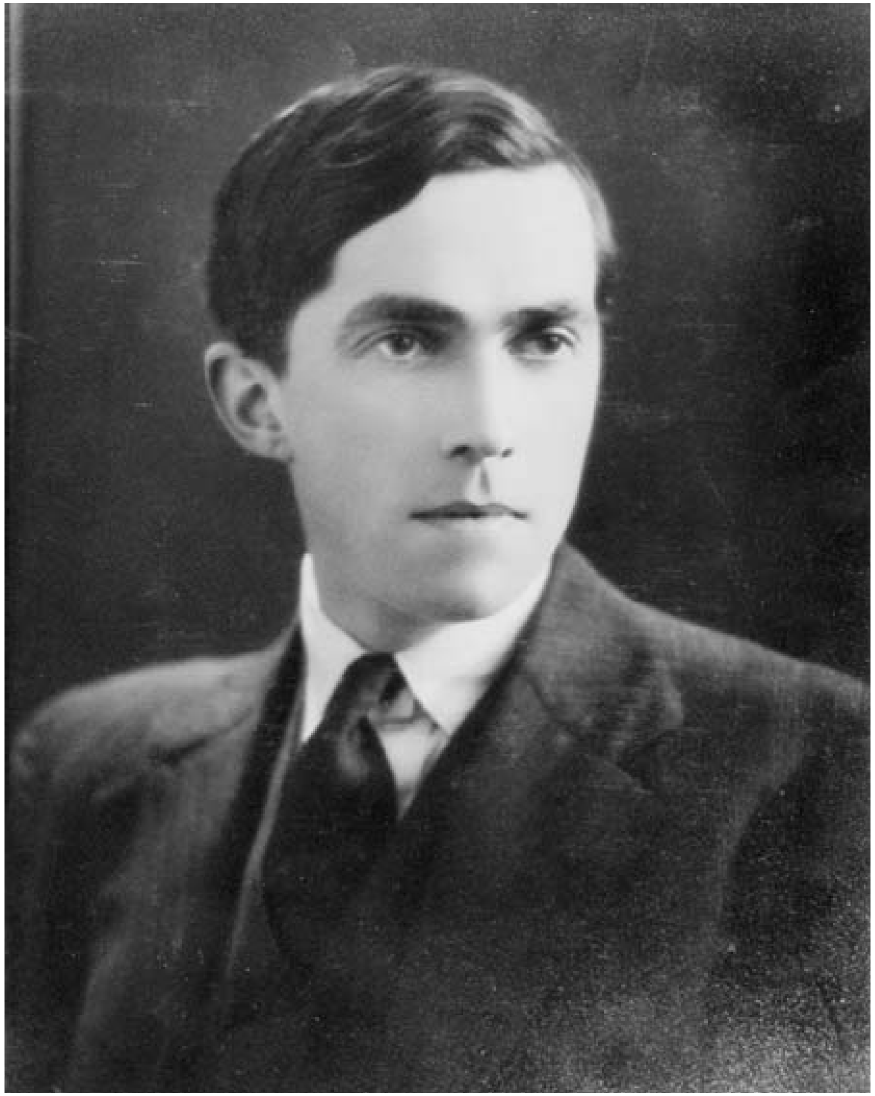

Figure 7.2 Roy Harrod. Taken when he went to Christ Church, Oxford (source: By kind permission of the Senior Common Room, Christ Church, Oxford).

Cambridge, 1938

Keynes's paper in the *Quarterly Journal of Economics* was not his only reaction against the dangers of encapsulating his (or others') theories in a limited formal model. In fact, the most aggressive and least successful of his controversial incursions into that terrain was only undertaken the following year, in spite of

his rather poor state of health. He was then asked to referee the books that Tinbergen was preparing for the League of Nations on the comparison of theories of the business cycles – a crucial question given its policy implications, as Tinbergen was quick to note[17] – and Keynes immediately ignited a fierce debate on the issue.

Keynes's criticism of Tinbergen was his most important contribution to the debate about econometrics. It greatly surpassed his early polemics on statistical inference, despite recapitulating some of its themes: the critique of the correlation techniques emerged from his 1907 dissertation, the 1910 controversy with Pearson and the preparation of the *Treatise on Probability*. There are thus two sides to the question. First, the 1939 critique represented a reaction against the growing formalisation of the discipline and the imposition of mechanical metaphors. Second, Keynes's argument was defeated and subsequently ignored since it was out of phase with his own passivity in relation to the 1936 debate and since some of his disciples were directly engaged in 'reconciliation'. Furthermore, Keynes's methodological remarks about statistics, which were at the core of the Tinbergen debate, were not understood, scarcely discussed and mostly despised,[18] since he was seen by many as an outdated economist in so far as the more fashionable and promising techniques were concerned.

Indeed, what Keynes most feared was the inability of the mathematical language to express clear theories and to concentrate on the issues, and therefore the danger of getting lost in irresponsible arithmetical mazes.[19] Some econometricians understood fairly well that Keynes was challenging 'the introduction of probability terms [doing] violence to the nature of economic facts' (Haavelmo, 1943a: 13). But most of the pedestrian inhabitants of the province of economics were neither ready nor attentive to these epistemological quarrels, and were inclined to ignore Keynes's advice and concerns.

When he was asked to review Tinbergen's volumes, Keynes once again did not conceal his 'lack of familiarity with the matter'. He even advised his correspondent to look for the *imprimatur* of 'someone more competent in these matters than I am'.[20] In spite of his limitations, he maintained his deepest opposition to the general procedure, which he had already expounded in a previous letter to Harrod,[21] since 'to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought'.[22] But Keynes again missed the exciting news of the econometricians: the emerging methods promised further developments that the techniques he favoured could not even remotely match. Although there is a hint of such a feeling in his letter to Harrod, in which he envisaged alternative methods and concluded that 'however, I may be wrong. I have not studied his work as carefully as you have',[23] Keynes maintained his point of view. It is quite obvious that he saw the whole episode as a simple remake of his earlier debate with Pearson, and that he still considered his objections to be valid.[24] Therefore, when Keynes published the *Economic Journal* review of Tinbergen's work (September 1939), he repeated his criticism of correlation:

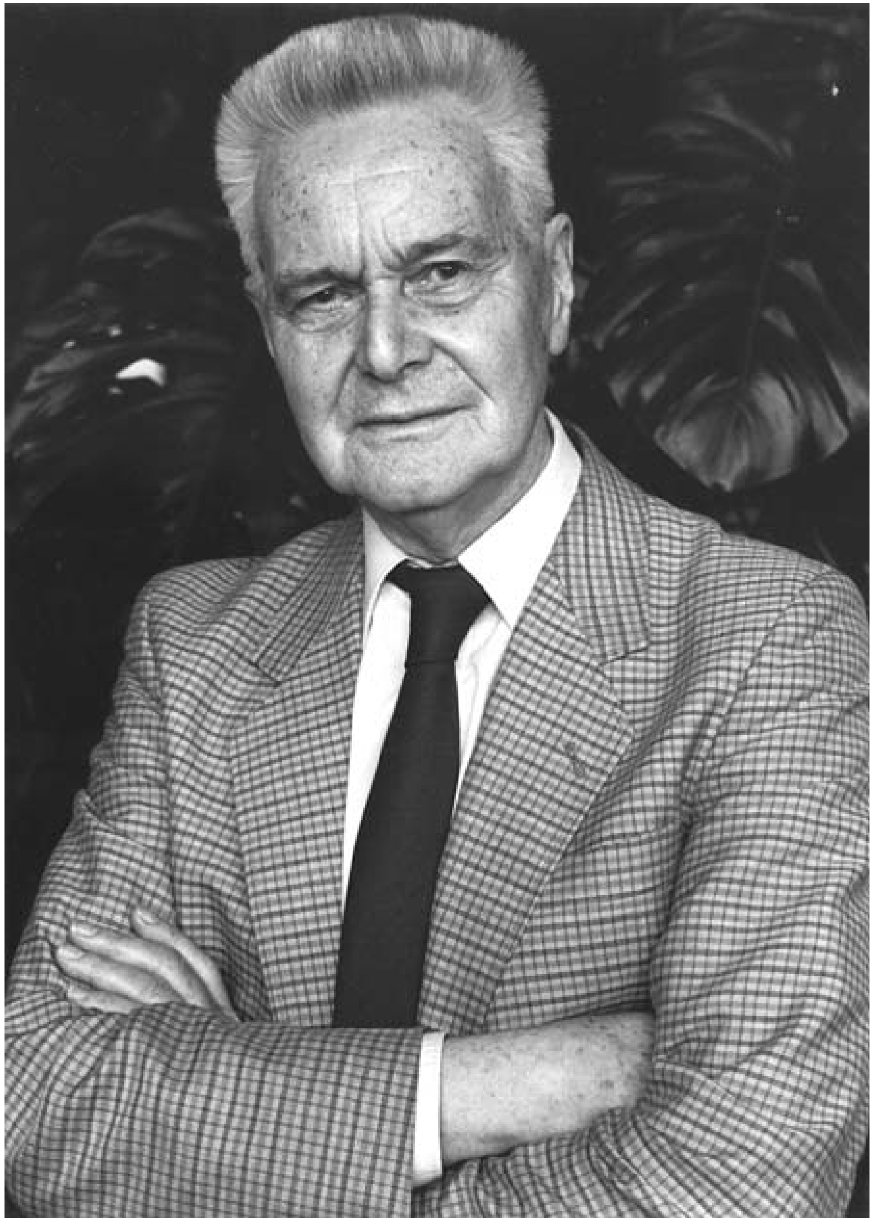

Figure 7.3 Jan Tinbergen (source: Levien Willemse).

> Thirty years ago I used to be occupied in examining the slippery problem of passing from static descriptions to inductive generalizations in the case of simple correlation; and today in the era of multiple correlation I do not find that in this respect practice is much improved.
>
> (Keynes XIV: 315)

With this background, one can understand that the econometricians did not much care for Keynes's critique: it was anticipated and summarised as the mere implication of a 'nasty' – as Marschak had put it in the preparation of the Oxford meeting – and permanently sceptical attitude towards mathematics and, therefore, as part of the old heritage of literary economics that they were struggling to free themselves from. The econometricians just felt that Keynes was again 'out of his depth', and that was all.[25] On the other hand, these twin movements of the development of the IS-LM formalism and the construction of the early econometric models, such as the one presented by Tinbergen, were part of an emergent culture in economic theorising which gave greater emphasis to formal elaboration, quite apart from the rule of intuition and the methodological precautions that Keynes argued for. This justified both his harsh criticism of Tinbergen and the general scope and epistemological implications he tried to impose on the discussion.

But, although defeated, he was not alone in the argument. The debate will now be briefly summarised, before considering three key additions to the debate, two which were published only recently (Lange and Marschak's text and Frisch's contribution to the Cambridge conference) and one which remains still unpublished (Divisia's review of Frisch's paper).

Keynes, Tinbergen and the 'old slippery problems'

Tinbergen's tests of the theories of business cycles reviewed earlier by Haberler were based on a model of twenty-two equations and thirty-one variables, calculated for the 1923–35 period for the US (several other series for different countries were used in the first volume). After preparing a model for the Dutch economy, this work was the second large-scale applied study with empirical data under the new research programme, and many problems of estimation were identified and discussed. It therefore represented an impressive performance and a major step forward for econometrics. Tinbergen used multiple regression in order to indicate the extent of the influence of the variables, and correlation to verify a theory as a whole. After estimation, he tested every equation for stability in different sub-periods. Although Tinbergen did not recognise the statistical problem of the estimation of simultaneous equations – that had to wait for Haavelmo and 1943 – he admitted that the specification of the equations was somewhat arbitrary and that it could not encapsulate all possible types of causality. Yet, he argued that the distinction between the impulse and propagation mechanisms was sufficient to provide a good estimation of the structure of the model and therefore to allow for the comparison of the theories of business cycles.

As is widely known, Keynes's main criticisms of these early econometric methods were based on the complexity, qualitative nature and interdependence of the variables describing real social phenomena, and on the irreducibility of the evolutionary processes to simple models. Consequently, Keynes suspected these methods which used non-experimental and unique sets of data whilst performing statistical tests primarily designed for analysing processes to which a well defined probabilistic theory could be applied. Moreover, in the world of organic systems, correlationist methods may fail and, since this is the case for most of the relevant economic variables, no general inductive claim is possible from these methods, according to Keynes's critique.

The main issue was indeed the application of the method of multiple regression to non-homogeneous series in real time,[26] and the consequent problem of misspecification: the method and the results are only relevant if the researcher is able to indicate all possible influences on the endogenous variable, if the theory is previously established and is correct, if there is no change whatsoever in the structure of the modelled system and if enough data are available to establish the correlation – a truly Laplacean set of requisites. On the other hand, since the method supposes homogeneity over time, the same structure must assume stable coefficients for the period under inspection, a dozen years in the case of Tinbergen. Keynes argued that this was not conceivable and that there was a trade-off between the length of the series needed for multiple correlation and the assumption of the stability of the coefficients, restricted to very short series (Keynes, XIV: 294). Consequently, the method was criticised in private letters as a 'mess of unintelligible figuring', as some sort of 'black magic' or 'charlatanism', a 'nightmare', a typical product of 'alchemy'[27] (ibid.: 289, 305, 320, 315). Consequently, Keynes argued that the method did not even merit close attention, given Tinbergen's assumption that 'the same formula is valid over a long period of years. If this is seldom or never the case, is it worthwhile to bother about the details of the method?'.[28]

For Keynes, the treatment of time was the *experimentum crucis* for the method – and, indeed, for any inductive statistical method – and he considered that Tinbergen failed to provide any meaningful alternative or even a modicum of progress in relation to the old correlation exercises, merely putting old wine in new bottles. This explains both the cursory reading of Tinbergen and the rudeness of his review, as he explained in a letter to Lange:

> Does not every case to which Tinbergen has applied his method assume that the same formula is valid over a long period of years? If this is seldom or never the case, is it worthwhile [bothering] about the details of his method? For this is not merely a casual assumption but one which is intrinsic to the whole way of proceeding.
>
> (Keynes to Lange, 10 April 1940)

It is quite possible that, even allowing for Keynes's well-known scepticism about the applications of mathematics to economics, this rather unfriendly criticism took Tinbergen by surprise.[29] Nevertheless, he acknowledged some of

Keynes's conditions for the use of the method arguing that they could be solved under certain drastic restrictions:

> in so far as one agrees:
>
> a that the explanatory variables chosen explicitly are the relevant ones;
> b that the non-relevant explanatory variables may be treated as random residuals, not systematically correlated with the other explanatory variables, or
> c that the mathematical form of the relation is given, certain details on the probability distribution of their 'influences' can be given.
>
> (Tinbergen, 1940: 141)

Tinbergen was cautious about the misuse of the method, and he accepted that it could not provide statistical proof for a theory; but still he maintained that empirical data could disprove a theory, something which Keynes could not accept either (Keynes, XIV: 307). But he insisted that the new methods could deliver crucial statistical evidence and therefore the necessary information for policy choices. Therefore, they were irreplaceable. And, of course, Keynes's cursory dismissal of Tinbergen's massive and innovative effort horrified and rapidly mobilised all the econometricians: finally, the whole debate turned out to be dangerously close to a pointless waste of arguments, since not only did Keynes misjudge Tinbergen's work but the early econometricians also misunderstood Keynes's criticisms. His prime motivation was indeed the same as it had been twenty-eight years before, and was strengthened by his awareness of the great complexity of real economies, which could not be encapsulated by methods designed to analyse fixed conditions and repeated samples. But this was not understood by his opponents.

Since in the social realm one cannot assume the 'principle of limited independent variety', Keynes's argument was that Tinbergen's method failed and could not be extended to the unpredictable reality of social and economic life. Moreover, correlation proves little if anything about causality, since the *ceteris paribus* conditions – the analogue used for laboratory control in physics experiments – may easily lead to the fallacy of the *post hoc*, *ergo propter hoc* argument.[30]

Many argued that Keynes both ignored and opposed the progress represented by Tinbergen's book, since he despised any advances in the mathematical formulation of economics. Stone was one of these,[31] and he also added another explanation for Keynes's attitude: he 'suffered from an irresistible urge to overstate' (Stone, 1978: 12). This view had been previously stated by Harrod: 'he certainly had a tendency in general conversation to *épater le bourgeois*' (Harrod, 1951: 468). Even if this may be true, the debate proved that what was at stake was a decisive question about the need for alternative conceptual formulations as the basis for the application of mathematics to the subject matter of economics (O'Donnell, 1997: 132), namely the notions of change, uncertainty and complexity in real time processes.

Hendry and Morgan, who side with Tinbergen,[32] recognise that the crucial problems – the completeness of the set of causal factors, the inter-connection between variables, homogeneity over time and the constancy of parameters – remain a 'greater threat', although arguing that they are not necessary conditions for the inquiry into 'structural autonomous relations' (Hendry and Morgan, 1995: 55). For this or some other reason, the econometric mainstream, which was to alter substantially the daily methods of economic inquiry, ignored the crucial criticisms levelled by Keynes.[33]

As a consequence of the whole debate, the crucial epistemological point came to be lost in the battle of harsh criticisms and massive counter-attacks. Yet, this issue was already quite clear by that time: in opposition to Keynes's concept of organic unity and evolution, Tinbergen suggested that economic laws could only be intelligible as legitimate statements about stability, as measured by the constancy of the parameters. He even emphatically added that such constancy distinguished science from storytelling and that only law-like descriptions made for the advancement of science:

> Even if we assume curvilinearity in our relations and 'coefficients depending on other variables', etc., we come back, in the end, to coefficients that are constant. But that is essential for any theory that really deserves the name.
>
> [. . .] Describing phenomena without any sort of regularity or constancy behind them is no longer theory. An author who does not bind himself to some 'laws' is able to 'prove' anything at any moment he likes. But then he is telling stories, not making theories.
>
> (Tinbergen, 1940: 80)

Measurement versus literature, exactness versus divagation, lawfulness versus ignorance and econometrics versus metaphysics: wasn't this a very challenging appeal?

Econometric debates at the 'little League of Nations meeting'

Unfortunately, this crucial point about the constancy of parameters and the treatment of time soon became a mere hidden implication of the discussion – in spite of its centrality to the divergence with Keynes. In fact, the postulate of the constancy of the structure of the equation and its parameters was not easily accepted at first, and its triumph dates from the later development of econometrics after Keynes's challenge. It was not even fully accepted by two of Tinbergen's most courageous supporters, Marschak and Lange, who tried to extend the controversy in the pages of the *EJ*. But Keynes rejected their offer[34] and their paper remained unpublished until 1995 (Marschak and Lange, 1940). It represents an important work, because of both its argument and its authors, who were assisted by none other than Haavelmo, Yntema and mostly Mosak.

The authors were convinced of the far-reaching consequences of both the debate and their own contribution: 'The difference between our article and Tinbergen's concerns not the subjects raised, but the way in which they are treated. Frankly, I think that our treatment is much superior and thorough, and that Tinbergen does not do full justice to his own case.'[35] As a consequence, Marschak and Lange decided to defend as well as clarify Tinbergen's programme. Like Tinbergen, they argued that these methods provided the only adequate means of developing the Keynesian programme:

> Since we are both in profound agreement with the economic theories of Mr. Keynes, we are anxious to prevent the readers of the *EJ* getting from Mr. Keynes's review the impression that his theories are not capable of empirical and statistical verification.
>
> (Marschak and Lange, 1940: 390)

Just as Meade, Harrod and Hicks at the Oxford meeting tried to formulate the ideas of *GT* in such a form that they could be verified, Marschak and Lange argued that overcoming Keynes's resistance against empirical and statistical verification was the pre-condition for the acceptance of his theory. From this starting point, Marschak and Lange proceeded to rebut the main arguments put forward by Keynes, explaining how his objections could be circumvented. First, the possibility of refuting theories by statistical tests was defended and the category of 'significant' variables was introduced: if the researcher could provide a complete list of these variables, a non-trivial request, he or she was supposed to avoid the argument about the necessity of including all possible minor causal factors. But 'significant' variables were those for which a statistical correlation with the dependent variable existed, and consequently correlation was taken as an indication of causality (ibid.: 391). Second, in order to deal with qualitative variables, Marschak and Lange suggested their ordering by rank, allowing for their inclusion in the model. Finally, the assumption of linearity was accepted as a mere 'first approximation'.

But the limited validity of the inference was nevertheless accepted due to the historical nature of the data: at least in this case some important instances of the non-constancy of parameters were acknowledged – and this was indeed the crucial point for Keynes. Here Marschak and Lange touched upon the decisive question: 'The real difficulty is presented by the case when it appears plausible to expect that the parameters connecting the factors listed (including time) are subject to sudden large changes, either during the period observed, or in the future' (ibid.: 392). Although this was still a marginal observation and not part of their central argument, such recognition is decisive.

In the preparation of the paper, Marschak and Lange largely discussed this topic through successive versions of the manuscript. Mosak was consulted about the matter at least twice, and the accepted conclusion was that Keynes had touched upon a decisive issue, 'since the elimination of time from the correlation problem might be interpreted as working with an incomplete list of factors'. Consequently, they concluded:

> that it is impossible to reconstruct the original equations from a statistical knowledge of their solutions, unless special hypotheses about the shapes of the curves, parameters, etc., are made. It is here that economic theory comes in as a necessary factor in the analysis. I think that on this ground we probably would have to yield to Keynes more than I was inclined to do in my original manuscript.
>
> [. . .] I think this point might be added to the manuscript, and in consequence the results would appear more conciliatory to Keynes than my first draft.
>
> (Lange to Marschak, 12 January 1940)

Some weeks after this letter, Lange again wrote to his co-worker in response to a second note by Mosak and argued that Yule's solution to the treatment of time was not satisfactory, since it implied this to be a purely 'separate variable'.[36] Finally, Marschak conceded that 'As the problem is, to my knowledge, not yet solved, I don't think we can go any further.'[37] Since the problem could not be solved, it had to be ignored, at least provisionally. As a consequence, the crucial problem of the nature of time and change in the historical series was generally avoided in the paper submitted by Marschak and Lange. Indeed, the reference to the question was reduced to a Salomonic solution: provided that the correct functional relation is given, time could be included in the list of variables and the interdependence between observations could eventually be eliminated (Marschak and Lange, 1940: 393). At the same time, the authors recognised that only 'limited inductive claims' were possible from this operation, since constancy over time could not be asserted:

> We share Mr. Keynes's views as to the limited inductive claims which can be made for the results of Professor Tinbergen's statistical analysis, both on account of the lack of a proof of constancy over time of the statistical relationship investigated,[38] as well as because of the impossibility of evaluating the effects of factors which were not subject to significant changes during the period under discussion.
>
> (ibid.: 397)

The reason for considering these difficulties was closely related to the intrinsic limitations of economics and social sciences as explanations for social evolution: since all inductive generalisations are historically and geographically limited by the *ceteris paribus* condition (ibid.: 397), institutions and varying social motivations may imply changes in the laws describing specific patterns of economic processes:

> It is only in the framework of given and constant social institutions and historically conditioned patterns of behaviour that most of the 'laws' of economic theory are valid.

> [. . .] The historical character of the empirical material of the social sciences subjects the regularities discoverable in the social world to much narrower limits of time and space within which they hold.
>
> (ibid.)

Yet, Marschak and Lange thought that, within these narrow limits, one could provide tentative statistical conclusions.[39] Therefore, they tended to reduce Keynes's logical and epistemological problem regarding the nature of time and the importance of historical change in economics to the narrower question of measurement and inductive techniques. This problem would be addressed some years after this episode: indeed, historical variability was soon to be brought into the picture under the form of a fully developed and controversial probabilistic theory extended to time series analysis. And although Marschak and Lange were not the prime movers of this course of action, even if they participated in the movement, they were certainly aware that its absence was the major shortcoming of Tinbergen's analysis and indeed said so (ibid.: 392). As a result, although in both their correspondence and their paper they accepted the primacy of the problem of time for statistical inquiries, their favoured solution – avoid the problem, then later on solve it with a probabilistic theory ignoring the constructive role of time – moved away from Keynes's line of argument.

Frisch shared the same concern with the problem of identifying the true causal relations of evolutionary processes but, unlike Marschak and Lange, he did not believe in the virtues of the extension of a probabilistic approach. His scepticism was, in fact, the main feature of the paper he prepared for the Cambridge conference. Frisch had followed the preparation of Tinbergen's report very closely: they had met in the autumn of the previous year, when Tinbergen travelled to Oslo to present his findings. Moreover, they were friends and shared the same view on the evolution of economics and the tasks of econometrics – and incidentally on the nature of Keynes's contributions: a couple of years before the episodes discussed in this chapter, Frisch had asked Tinbergen for a survey for inclusion in an early issue of *Econometrica* that 'should also include theoretical work of the "literary" type, as well as work of a semi-mathematical type which is sometimes produced by the English writers – Pigou, Keynes, etc.'.[40] Frisch and Tinbergen were close associates and accomplices in the endeavours of econometrics and yet Frisch's critique deviates from the attitude of other econometricians.

Frisch's contribution to the Cambridge meeting remained unpublished until recently (memorandum of 17 July 1938). It was not even discussed at the conference as the text only arrived some days after its closure: it was 'rather hurriedly written', the day before the opening of the Cambridge conference of 18–20 July 1938 (Frisch, 1938: 407). Nevertheless, this memorandum later enjoyed a wide circulation amongst econometricians and was 'tremendously influential' (Hendry and Morgan, 1995: 57). It was a more critical account of Tinbergen's conclusions than that of Marschak and Lange,[41] and furthermore it implied both technical and epistemological requirements that the available methods were unable to meet: Frisch stated that the equations could not stand as a test for business cycles and discussed what they 'really mean' (ibid.).

Frisch argued that Tinbergen estimated the parameters from the structural form and consequently ignored the problems of identification and multicollinearity, therefore reaching far too sweeping conclusions, and crucially that the true causal relations could not be demonstrated. In spite of the fact that some of these technical problems were addressed and solved in econometrics in the following years, the main point, for Frisch, was that the procedure could only obtain the quantification of coflux equations and could not achieve the identification and estimation of the autonomous equations that represented the true structural causality in the cycle (Frisch, 1938: 416–17). Once the economic data had been given, even for homogeneous processes over time, the real equations could not be recovered. As a consequence, no explanation and no policy conclusions were obtainable from the estimation of the models. Of course, no refutation was possible either (ibid.: 419) – a conclusion which remarkably coincided with Keynes's own point.

For Frisch, this conclusion was in keeping with his previous criticism of indirect steering mechanisms of the Keynesian type. And it had a radical implication: if the true causal relation, the autonomous structural equation, cannot be estimated, policy makers cannot base their projections on the use of traditional but defective tools, since they may just suggest fictions. Only highly autonomous equations could shed a light on reality, but that required other information than that of the equation system itself:

> The higher this degree of autonomy [built on knowledge obtained outside the system], the more fundamental is the equation, the deeper the insight which it gives us into the way in which the system functions, in short, the nearer it comes to being a real explanation. Such relations form the essence of a theory.
>
> (ibid.: 417)

For Frisch, this required information from outside the system, obtained from interviews and experimentation (ibid.: 418) – which of course the other econometricians were not available to concede.

Furthermore, if Tinbergen's method could only establish coflux equations, no test of theories and no refutation was possible; consequently, 'the lack of agreement between these equations and those of pure theory cannot be taken as a refutation of the latter' (ibid.: 418). And since 'if the results of our investigations are to be applied for economic policy purposes – for reforming the existing economic organization – it is obviously the autonomous structural relations we are interested in' (ibid.: 418), theorists could not avoid imposing further restrictions on the system they were considering.

Tinbergen reacted by arguing that the use of a priori theoretical considerations would guarantee that the obtained results were autonomous (Tinbergen, 1939: 421). In private correspondence during this debate, Tinbergen proved to be uneasy about Frisch's remarks:

> I am impardonably late in thanking you more personally for the trouble you took to prepare an important memorandum on statistical business cycle research after I sent you my League of Nations reports. Although I think I am of a different attitude towards some of the chief issues, I nevertheless am much impressed by your statements and I am trying to take account as much as possible of them. I wonder whether you will publish such a note later on?.[42]

Half a year later, Tinbergen explained why the points made by Frisch could not be considered by his fellow thinkers:

> There was opposition [after discussions in Geneva with Koopmans and Polak, who worked at the department of economic research of the League of Nations] against including – even as an appendix – our correspondence about autonomous relations etc., etc., since the purpose of the volume is to be readable to a greater circle of people. But in the text I have gone into various of the questions you raised, in simple wordings and, of course, referred to you. I think the essence of your remarks is in it.[43]

Frisch was not impressed. Moreover, his argument was deeply rooted in another consideration: the estimation of true economic relations should not be diverted by unsound methods. In fact, he was arguing in favour of combined methods in order to proceed to extensive planning. According to Frisch, such a measure was also required to match the challenge imposed by the pressing social needs – those of the Great Depression and those derived from the 'monstrosity' of the war itself and from the necessary reconstruction of the devastated countries.

In that sense, in his first paper to be published in *Econometrica* after being released from a German concentration camp and having returned to his duties as editor of the journal after the war, Frisch included an appeal to econometricians to turn their attention to the fulfilment of social priorities. This implied that economists should engage in direct policy making, and therefore that they should draw up plans. As he wrote later on, in 1958:

> I have personally always been skeptical of the possibility of making macroeconomic predictions about the development that will follow on the basis of given initial conditions. . . . I have believed that the analytical work will give higher yields – now and in the near future – if it becomes applied in macroeconomic decision models where the line of thought is the following: 'If this or that policy is made, and these conditions are met in the period under consideration, probably a tendency to go in this or that direction is created'.
>
> (quoted in Andvig, 1995a: 11)

Note the implicit distance in relation to the modelling practice based on the presumption of the constancy of the parameters and the analytical value of the estimated equations, as well as the argument for experimenting changes in the control parameter of the system the economists were monitoring – and consequently the close proximity to Keynes's decisive point on non-homogeneity over time, albeit for disparate reasons. In fact, Keynes addressed the problem in a rather different way, since he restricted himself to the short term and to the use of known behavioural patterns, even if not completely quantified. Keynes argued for indirect controls whereas Frisch supported direct controls; one favoured intuition and wisdom and the other a quantitative approach; nevertheless, both accepted that institutional change altered the structure of the economy, and that such a change should be guided in some way.

For some authors, this implication, together with all his future work, simply meant that Frisch had abandoned econometrics (Epstein, 1987: 127). In fact, it implied rather the contrary, namely that econometrics had abandoned him, since his own view of the programme was clearly defined from the early days, in the sense of using analytical tools to investigate and intervene in the social arena, and that had been the founding concern of the econometric movement, social engineering. Frisch wanted to develop scientific tools to prevent new waves of misery and unemployment and he never abandoned this purpose. Such an undertaking would require planning, economic activism and whatever means were necessary for creating welfare. In other words, economics should always be a 'moral science', to use Keynes's own words – and this view was shared by Tinbergen and others from amongst the early econometricians. And that was paradoxically why Tinbergen's statistical and modelling efforts did not convince his friend and colleague.

Enter Divisia, one more sceptic

The third piece of evidence regarding the econometricians' discussions – besides the failed paper by Marschak and Lange and the memorandum that Frisch had prepared – is Divisia's review, which reflected the discussion at the Cambridge meeting itself. Immediately following the conference, Divisia was asked by officers of the League of Nations to referee Frisch's review of Tinbergen's books. He did so in an as yet unpublished memorandum written on 14 November 1938, strongly recommending the publication of both the paper and Tinbergen's books.

In a letter to Frisch with a copy of the memorandum, Divisia clearly indicated that he shared the same type of reservations about generalisations made from statistical investigations: 'I ask myself if, even only in some particular cases, statistics is ever able to establish a correlation having, in relation to theory, more value than a simple indication.'[44] The review indicates that there was an intense debate at the conference on this question, namely about the epistemological and technical implications of the new methods, and that Frisch's point of view was shared by some of the participants:

> As far as its application to the work by J. Tinbergen in particular is concerned, I think that the observations made, which are certainly very important, do not add a great deal to those presented about this subject at the Cambridge meeting. Everybody is agreed, I believe (and this author above all), on the utility of clearly formulating some reservations about the results from the computation of correlations. The Frisch memorandum calls for precision and emphasises these reservations.
>
> (Divisia, memorandum of 14 November 1938, my translation)

In particular, the representation of cycles, the theme of Tinbergen's research, was given as an example of the possible lack of meaning of the observed correlation:

> I will go further than Frisch does about the possible defect, known by everyone, of the theoretical meaning of certain stated correlations.
>
> [. . .] As far as I am concerned, I believe the absence of meaning of these correlations to be extremely general, particularly in the case of oscillations.
>
> (ibid.)

Divisia presented a curious example as an illustration for his argument: suppose we have two sinusoidal curves, perfectly correlated. Then we may also find a good correlation between their derivatives of whatever order, given some lag, even if the theoretical meaning of one and the other coefficient are quite diverse. As a conclusion, he states that 'this leads us to the well-known truth that statistical observation does not by itself provide the explanation of phenomena' (ibid.). This implied an argument for greater care in relation to statistical proofs and explanations and, in general, to inductive inference from data. Indeed, Divisia shared with other scientists of the time a moderate scepticism about the ability of statistics to uncover real relations: in a previous book, he had argued that causal relations were quite different from statistical correlations, even if with the help of statistics a rational reconstruction could be obtained (Divisia, 1928: 207). Furthermore, the existence of multiple causality in social processes created further difficulties for statistical estimation (ibid.: 209).

Divisia's remarks display a notorious awareness of the epistemic problems of statistics, although he did not match the depth of Marschak, Lange and Frisch. In spite of his doubts about the reach of statistics, he still insisted – as the others did – on the importance of producing mechanical models in order to develop theoretical insights and models that expressed their fundamental consensus:

> To come back to the very important and interesting work done by Tinbergen, I recommend its publication since, from the point of view of the fears expressed by Frisch, it gives us some guarantees; it overcomes the framework of a simple statistical investigation and it is oriented in the sense of an indispensable mechanical explanation.
>
> [. . .] I am furthermore under the impression that with the development of new statistical research in this area, the need will be clearly felt for such mechanical explanations in order to coordinate the numerous observed ele-

> ments; given the condition that researchers should be theorists and not empiricists, which is in fact the case.
>
> (ibid.)

These remarks echoed a long conversation that had been held between Frisch and Divisia, mostly during the first years of the Econometric Society, when they were closer associates. From these years, their common interest was in how to assess economic cycles, and Divisia suggested a *pot-pourri* of theories and models for a paper for *Econometrica*:

> The functional equations explain a certain delay in a reactive mechanism; the theory of biological oscillations presented by Vito Volterra is based on a totally different idea; if I understand it well, the scheme of relaxation oscillations addresses the constitution of an unstable equilibrium which reverses from time to time; it may be the case that other mathematical schemes of oscillations also exist, corresponding to different ideas, such as the effect of random impulses on a pendulum.
>
> [. . .] In short, we must extract the quintessence of the econometric kitchen under the form of general ideas that many economists will be able to use or to criticise.[45]

Some days later, Divisia suggested a combined effort by Tinbergen (oscillations), Le Corbeiller (relaxation), Slutsky and Frisch (shock theory).[46] But such a synthesis was never made.

The econometricians supporting Tinbergen against Keynes or engaged in the development of the new methodologies acknowledged the difficulty of treating the crucial problem of time, and consequently uncertainty, causal complexity, historical change, social and institutional instability. They understood the nature of the question quite well and, despite sharing some doubts about the intended solutions, discussed a wholly new approach to statistical inference. Some of them were eager to develop this in the sense of a new vision of chance, randomness, probability and events in the social realm, and they did so. This effort was founded upon the consensus about the role of mechanical models as the privileged representation of reality and as the legitimate mode of explanation. And, since intelligibility required constancy of the structure and its parameters, no role therefore remained for Keynesian variables, which belonged to a different world. Lawfulness abhorred uncertainty and complexity. Consequently, the econometricians experienced and improved the new methods – and that is where they rejoined the reconcilers.

Alea jacta erat, the die was cast.

The reconciliation and mutation of econometrics

In this debate taking place in econometric circles, we have, in a nutshell, all the promises and problems arising from the early development of the programme.

Although their limits were acknowledged, mechanical explanations of reality were supposed to be decisive for the development of statistical information and theoretical understanding. This implies an astonishing balance sheet: Keynes's anticipated criticism was easily discarded, and his much publicised loss of interest in the developments of statistics and mathematics made such a rejection easier to perform. Despite this, however, the evidence from some econometricians suggests that to some extent they agreed with a number of crucial points made in that critique. This is the case with the central arguments regarding the non-homogeneity of 'samples' over time, the non-atomistic character of the economic variables, the role of institutional change and the inability of the method to detect the true causal relations. With just one possible exception, that of Marschak and Lange, who thought this could be overcome, these points were accepted (by Divisia) as frequent technical constraints on the computation and (by Frisch) as eventually permanent obstacles. But, in spite of this somewhat reluctant acceptance, the econometricians clearly came together in disagreeing with the non-mathematical alternative formulation that Keynes was defending, since they deeply shared the conviction that exactness was desirable, possible, attainable and even indispensable for the tasks of economics.

In other words, the decisive difference was epistemological: the early econometric programme was built on the solid foundations of the mechanical models. Certainly, the mechanical models did not necessarily imply equilibrium conditions: at the time, several alternatives were already available in mechanics for the study of disequilibrium – but equilibrium remained the condition for simple computation and for the introduction of comparative statics as the privileged tool for analysis. Therefore, the dominant view of the cycle as the summation of an equilibrating propagation mechanism and an exogenous impulse system restricted the theoretical analysis of fluctuations to rather simple mechanical models. Consequently, Keynesian uncertainty and institutional or historical change were ignored.

This was certainly the preference of Tinbergen, for whom the core of the explanation was the understanding and representation of a mechanism (Boumans, 1992: 74–5). It was also the preference of Divisia. And it was also the preference of Marschak and Lange, who rejected the alternatives, those 'half theories, relying unadmittedly on outside influences, on *dei ex machina*' (Marschak and Lange, 1940: 392fn.). For Frisch, a theory required a mechanical interpretation. It was in that sense that Frisch wrote a manifesto for the econometric programme with his seminal paper on the rocking horse, based on the impulse and propagation distinction that paved the way for the triumph of the mechanical cum probabilistic approach (Frisch, 1933a). It is also true that he did not follow this approach throughout his career: Frisch argued for mechanical representations with a probabilistic element, but eschewed such a combination in statistical modelling practice and adhered, as Tinbergen did, to a rather *ad hoc* probability theory.

But this debate also refers to a deeper issue: indeed, it was the first skirmish between realism and instrumentalism in time series analysis (Lawson, 1989:

236). Instrumentalism finally prevailed, and it was up to econometrics to guarantee its dominance: predictions were accepted as the criterion for legitimate scientific practice, since theories cannot be considered to be false or true, whereas realism alternatively asserts the independent existence of the objects of analysis and, as a consequence, confirms that the identification of causal structures is attainable.

Although econometricians admit the puzzling difficulties of the currently available methods,[47] they tend to inherit Koopmans' battling spirit in relation to the Keynesian type of critiques. Earlier in the 1930s, Marschak and Lange had understood quite well that the maturation of econometrics depended on the availability of a well-developed probabilistic approach. And that was the immediate future: the scene was set for the spread of the probabilistic paradigm into economics. It was time for Haavelmo and Koopmans, and for a shift in econometrics, which occurred under the auspices of the Econometric Society and the Cowles Commission, led by Marschak and Koopmans from the end of the 1930s onwards. Indeed, Tinbergen interpreted Haavelmo's later theoretical contributions as a correct and necessary rebuttal of his own work:

> I have never been very strong at it [mathematics in economics], you see, and I didn't like it much either. I used it as a tool and I tried to know the most important things, but I made almost elementary mistakes. Haavelmo pointed out that estimating a system of equation by least squares for each equation separately is mistaken. That already illustrates my relatively weak interest in mathematical statistical questions.
>
> (Tinbergen, 1987: 119)

A period was coming to an end, and Keynes's critique was a matter of the past. The reconcilers had managed to incorporate his theory into a geometrical and analytical representation leading to equilibrium. Their allies at the Oxford meeting were the young econometricians, who fought for a new approach to the development of economic theories. Yet their views differed: the econometricians favoured a rapid and decisive political intervention against the disequilibrating consequences of the market's self-regulation, whereas the reconcilers tended to think that drawing closer to the neoclassicals was the condition for the effectiveness of a new social policy. The 'synthesis' had begun as a political move, before gaining an epistemic status in economics.

Another paradox was related to the reduction of dynamics to mechanics, and to the role of the mechanical models that became the standard way of representing cycles and social evolution. For Frisch and Tinbergen, at least, the irreplaceable use of these models had no ideological implications whatsoever, and surely not the one implied by free-marketeers *à outrance*. Even later on, when some of the Cowles Commission members developed the project for the estimation of systems of structural equations, they intended it to prove the argument for the possibility of a Walrasian social reform, as opposed to the radical Hayekian liberal alternative. Therefore, equilibrium was not necessarily seen as *status quo*

ante, but could also be thought of as the framework for the comparison of different scenarios and for the choice of convenient social policies. Yet, reconciliation plus the widespread acceptance of mechanical equilibrium as the legitimate mode of theorising led to the 'synthesis': Hicks and Harrod were eager to 'reconcile', and an emergent culture of formalisation in economics, fully supported by the econometricians, eased their way to the neutralisation of Keynes's critique – it was then a matter of time and opportunity for the transmutation of reconciliation into an ideological perception of equilibrium. When, later on, econometrics was also transformed into Bourbakist axiomatics, neoclassical economics had prevailed.

Something was lost along the way: the caution and methodological reflections of the founders of econometrics, as well as the important features of their discussion on causality and the constructive role of time and complexity in real societies. This loss may be highlighted through the comparison between the econometricians and Keynes and the analysis of their debates. They had distinct understandings of the role played by mathematics in the development of economic theories, and by statistics in their confirmation. They also differed as to the epistemological role of the mechanical and organic analogies, and yet some prominent econometricians still wanted to use Keynes's theories and vision for policy making.

But, at the same time as Keynes's harsh critique, although not because of it, one of the founder members of the Econometric Society, no less a figure than Ragnar Frisch, was taking his first steps away from the econometric research programme, as it was being defined in the late 1930s. Consequently, he did not follow the dominant strategy for the construction and estimation of structural macro-models, much less that of axiomatisation, which inspired the later mainstream econometrics, and favoured the elaboration of decision plans. Frisch was himself followed by Tinbergen shortly afterwards and evidence shows that, although inspiring the use (and abuse) of mechanical models, most of these forerunners of econometrics – Frisch, Tinbergen, Roos, Marschak, Lange, Divisia – shared crucial doubts at some stage about the implications of the methods they were fathering.

Mainstream econometrics developed in the 1950s along another completely different path, based on a great wealth of sophistication and expertise, towards the thrilling world of axiomatic adventures in the equilibrium wonderland, naturally ignoring the puzzles of the first great debates. At that time, very few noted that the original pluralism in the emergence of econometrics was beginning to fade.

8 *Quod errat demonstrandum*

Probability concepts puzzling the econometricians

This chapter investigates the concept of error in economic theories, models and equations, beginning with the initial discussions on the nature of randomness and determinism – a crucial departure for econometrics. Indeed, when interpretations of probability and certainty were contraposed, after the 1930–3 discussions on the pendulum and the contribution made by Slutsky, it was obvious that the crux of the matter was that the theoretical status of the 'error' was not clear.

Consequently, economists divided into two groups in relation to the advocacy of the probability approach: enthusiasts and sceptics. While the impetus of the probabilistic revolution motivated some economists to favour its application, for many others this was neither acceptable nor feasible. The dispute concentrated on interpretations of the foundational dichotomy of law and 'chaos', or order and chance. Contrary to the other discussions that were the subject of previous chapters, in this case the pressure for innovation and mutation emerged from within the inner circle of Ragnar Frisch's group.

At night all cats are grey?

The concept of error in economics is paradoxical. There is an obvious discrepancy between concepts such as 'error', 'shock', 'residual', 'perturbation', 'disturbance', 'innovation', 'stimuli', 'noise', 'aberration' and so many others used to describe one of the core operational terms in economic models. It suffices to open any handbook of statistics and evidence will emerge of the pervading epistemic ambiguity of these distinctive concepts, which create a constellation of colliding meanings and semantic instability.

Johnston uses the concepts of both 'disturbance' and 'error' to describe discrepancies between the values expected from a model and the really observed values. These discrepancies are explained by heterogeneity among agents, given all the possible small influences on their behaviour, aggravated by the unpredictable randomness in human diversity (Johnston, 1987: 14–15). In other words, the 'error' is a feature of the model and the price of its limited power of explanation. Judge and his collaborators also explain the error term as being simply the unexplained part of reality, given the model: $y_t - \beta = e_t$ (Judge *et al.*, 1988: 160–1). But they add another different argument: the random

vector represents the unpredictable or uncontrolled errors associated with the outcome of the experiment and consequently, 'the random vector is often referred to as the noise' (ibid.: 179–80). Noise, of course, is much less than the unexplained divergence between real data and the prediction of the model.

Maddala equates the concepts of 'error' and 'disturbance' and defines three possible origins for that error: the unpredictable randomness in human behaviour, the large number of omitted variables and the error of measurement in the endogenous variable (Maddala, 1992: 64–5). Griffiths and his collaborators refer to the same explanation, adding the possible approximation error provoked by the assumption of linearity (Griffiths *et al.*, 1993: 175–6). Greene defines the error as the aggregation of omitted variables and errors of measurement (Greene, 1993: 142–3). Gujarati and Harvey attribute the error to factors outside the model, and Cuthbertson *et al.* to deviations from the model (Gujarati, 1992: 7; Harvey, 1981: 2; Cuthbertson *et al.*, 1992: 1).

This short overview includes seven different explanations for the error term, namely:

1 measurement errors,
2 influence of omitted variables,
3 intrinsic randomness in human agency,
4 theoretical misspecification of the model,
5 functional misspecification,
6 general inadequacy of the model, and
7 in general, irregularities, which Frisch called 'aberrations'.

This heterogeneity of reasons highlights the problems with the use and misuse of the listed concepts: although some of these names for 'error' are clearly synonyms, the fact is that others are contradictory or diverse. Furthermore, the proposed explanations are also partially contradictory. While some of the arguments situate the 'error' in the universe of the model (residual) and state that it is observable, others emphasise that it is to be found in the nature of reality (disturbance) but remains unobservable. Some are intrinsic (error of measurement), while others are extrinsic to the model (unpredictable random behaviour of humans). Some are eventually corrigible (neglected influence of omitted variables, approximation error imposed by the assumption of linearity), others are not (heterogeneity among agents). Some refer to variables defined in the experimental universe of the model itself (stimuli), whereas others refer to features attributed to reality (perturbation). Some refer to relevant exogenous causes (shocks), while others argue that they are irrelevant (noise), although it is their very irrelevance that defines their useful statistical properties. The concept of error hides a forest of deviant meanings.

But these discrepancies did not pass unnoticed. Goldberger argued that there is a substantial difference between the interpretation of the model of the residual and that of the model of the disturbance. The two models are confronted:

> Judge *et al.*'s ε is simply the disturbance vector, the deviation of the random vector y from its expectation $\mu=X\beta$. In that style, for a scalar random vector y with $E(y)=\mu$ and $V(y)=\sigma^2$, one might write $y=\mu+\varepsilon$, $E(\varepsilon)=0$, $E(\varepsilon^2)=\sigma^2$. There is no serious objection in doing so, except that it tends to give disturbance a life of its own, rather than treating it as merely the deviation of a random variable from its expected value. Doing so may make one thing of μ as the 'true value' of y and of ε as an 'error' or 'mistake'.
>
> For example, Judge *et al.* say that the disturbance ε 'is a random vector representing the unpredictable or uncontrollable errors associated with the outcome of the experiment', and Johnston says that 'if the theorist has done a good job in specifying all the significant explanatory variables to be included in X, it is reasonable to assume that both positive and negative discrepancies from the expected value will occur and that, on balance, they will average out at zero.' Such language may overdramatise the primitive concept of the difference between the observed and the expected values of a random variable. In any event, we will want to distinguish between the *disturbance* vector $\varepsilon=y-\mu$, which is unobserved, and the *residual* vector $e=y-\hat{y}$, which is observed.
>
> (Goldberger, 1991: 170–1)

Between the mere statistical tool and the disturbance with a life of its own, there is a world of difference. Furthermore, there are strong implicit ontological statements in this story, since a limited concept of order requires the 'true' value to be $E(y)$, not y, which allows for the attribution to ε of the denomination of a real 'error'. Indeed, these interpretations are deeply rooted in the history of economics and correspond to different and sometimes alternative visions of statistics.

The next section briefly presents some of the main contributions for the introduction of the concepts of probability and error in economics, arguing that this epistemic instability was clearly detected and discussed in the first period of the installation of econometrics. So, let us look back.

Hic sunt leones, or the danger of the unknown

The nature of chaos and order, or of randomness and structure, is a mystery that has been the subject for several generations of scientific disputes. This difficulty is highlighted by successive contradictory denominations and by the pervasive presence of mythological interpretations and reinterpretations.

According to the Bible, one of the various matrices of western cultures, 'Chaos' prevailed at the beginning of time, but then God came and order was created. Other mythical accounts share that same view of disorder turned into order. Yet, as the tale goes, even when order was imposed – an exogenous order imposed in any possible way – an essential feature prevailed in the management of human affairs: order resorted to chance as frequently as it needed to do so. Matthias was chosen by lots to complete the twelve Apostles (Acts 1: 26), and the

Almighty did not hesitate to indicate guilty people by lots: this was the case with the trial of Jonathan (1 Sm. 14: 37–43), of Jonah (Jon. 1: 1–10) and of Achan (Josh. 7: 10–23). Lots intervene everywhere and at all times: the Roman soldiers cast lots for Jesus's tunic (John 19: 23–24 and Ps. 22: 18); Julius Caesar uses chance to decide on his destiny and that of the Empire – the dice decide, *alea jacta est*. Again, one of the founding fathers of the Catholic Church presents the argument for chance as an expression of order: according to Augustine (Ps. 30: 16, serm. 2) '*Sors non est aliquid mali, sed res, in humana dubitatione, divinam indicans voluntatem*', lots are not bad in themselves, for they indicate the Divine will when man is in doubt (Ekeland, 1993: 9). Later on, some of the reference literature gives new examples: Rabelais, in *Gargantua and Pantagruel*, makes the honourable Judge Bridlegoose pass sentences by rolling dice.

Of course, the use of chance is as old as history. Games of chance were to be found in any old civilisation: lots, cards and dice pervade all narratives of antiquity. Institutions used it as well as laymen, and they were certainly required to do so: according to Stigler, there has been, at least since 1100, evidence of 'institutionalized numerical allowance for uncertainty', with the *Trial of the Pyx*. The London Mint, in order to check the quality of its procedures, used the *pyx*, a box containing a random sample of coins, whose weight was then compared to the standard control values. It is certain that there was staunch resistance to combining measurements taken under different circumstances, under fear that an error would contaminate all the measurements, rather than be compensated (Stigler, 1986: 3). Sampling was not easily understood or accepted, although it was recognisably the only accessible method for control in large-scale production. Order meant taming chaos, and that was the work of probability methods and concepts.

The history of statistics and of the definition of probability goes far beyond the limits of this chapter. It is a long and illustrious history, ranging from the puzzles established by the Chevalier de Méré (1654), passing through Pascal and the correspondence between Leibniz and Jacob Bernoulli on the law of large numbers (1703), to the definition of normal distribution by de Moivre (1730) and the establishment of the principle of maximum likelihood by Daniel Bernoulli (1778). Leibniz, the 'first philosopher of probability', defined it as the degree of belief warranted by evidence, whereas, in 1675, Huygens wrote the first textbook on probability, defined as the stable relative frequencies (Hacking, 1975: 185).

Yet it was only in the early nineteenth century that a theory was provided for the distribution of errors in measurement with Gauss (1809) and the first formulation of the central limit theorem the following year by Laplace. Almost simultaneously, but independently, the concept of error was introduced into practical methods of statistics (Klein, 1997).

In parallel, in 1805, Adrien Legendre established the first approximation to the OLS method. Given

$$a_i=-b_ix-c_iy-f_iz-\ldots+E_i$$

where E_i is the error, which should be nullified. Although the author recognised an element of arbitrariness in the 'distribution of errors among the equations', this method was supposed to come close to the truth: 'By this method, a kind of equilibrium is established among the errors which, since it prevents the extremes from dominating, is appropriate for revealing the state of the system which most nearly approaches the truth' (Legendre, 1805: 72–3).

Legendre's OLS method was immediately adopted:[1] in ten years it became the standard method. But the method implied no formal treatment of probability and was precisely defined in relation to a specified scientific field: errors of measurement in relation to an acceptably *true* law of the universe. Indeed, it depended on the verification of the Newtonian laws and was generalised as part of the Laplacean vision of determinism:

> Within the context of post-Newtonian scientific thought, the only acceptable grounds for the choice of an error distribution were to show that the curve could be mathematically derived from an acceptable set of first principles. As the inverse square law was the touchstone of mathematical astronomy, so the principle of equally likely cases was that of mathematical probability. If a choice of a curve of errors was to be found acceptable, it must be reducible in some sense to a description in terms of cases supposed equally likely, or indifferently indistinguishable. Both of Laplace's derivations fall within this paradigm.
>
> (Stigler, 1986: 110)

In this framework, the notion of 'error' depended only on the limits of the apparatus of observation, since the theory would necessarily provide the correct coordinates of the astronomic object. In so far as Newton's laws were accepted, the concept of error was therefore precisely defined: it could have no other origin than the measurement itself – it was indeed an error, in the full sense of the word. There were no mixtures of causes, no new variables, no extrinsic influences, no undefined agents, no strange and surprising behaviour to generate the error. The model had few degrees of freedom and it was supposed to be able to describe exactly the state of nature and its evolution. The error is just an error and science aims at omniscience.

Nevertheless, the application of these concepts to social sciences was not trivial. But it was powerful enough to challenge the resistance: the characteristics of order in a population were deemed more valuable than disorder and differences between individuals. Quetelet, a Belgian astronomer who inspired the mathematical methods in statistics, argued that the behaviour of individuals was fundamentally unpredictable, but added that the aggregation of evidence and measurements describing the behaviour of a large crowd would necessarily uncover a law of behaviour – certainly one of the first 'certainty equivalents' in modern social sciences. That equivalent is the law of the distribution of errors in the deviations from the average. This powerful result gained credit in the scientific community: it could be empirically checked in a number of instances and it

allowed for measurement, control and prediction. The law of errors was assumed by Karl Pearson to be the normal distribution, and the consequence of such a claim was to affirm the primacy of order over chance: random variations were recognised to exist, but were domesticated, and consequently variation could no longer challenge the capacity of science to uncover causality.[2]

The subsequent semantic instability of the concept was consequently alien to its origin: it emerged later on from the extension to the social sciences. Of course, in social sciences there is no equivalent to the Newtonian laws, no single causality, no general authoritative equation representing the trajectory of a system, not even a single authoritative theory for the discrimination of the variables and their functional form. Consequently, the error became a 'residual', i.e. it was accepted that it would depend on the theory and its model determining the measurement. This consequential conceptual shift dominated the introduction of the modern concept of statistical 'error' in social sciences and in economics in particular.

In fact, there are deep differences between the concept of error in astronomy and this new social concept: the *error* is only equal to the *residual* if one can assert as a dogma that the model is true. In the Newtonian world, error is an exact measurement of the deviation from the correct orbit, established without a shred of a doubt by theory, since it defines invariant mass points on which exogenous forces act, giving the balance between forces that determines the position and momentum of the bodies. The causality of the recorded deviation, consequently, can be unquestionably attributed either to the error of the apparatus of measurement itself or to other ignored forces at work, influencing gravitation. Alternatively, in the framework of social sciences, the residual is a derivation of the model, but it interprets a state of nature, an irreducible variation impinging perturbations on the system.

It must be added that, for many economists, this simply could not be accepted, since economics could not mimic astronomy and physics. For others, however, the analogy established the paradigm of social sciences uncovering the very structure of order. In this sense, the mechanics of the universe would be inconceivable without order, and the very concept of order excludes chance and surprise: 'Happily the universe in which we dwell is not the result of chance, and where chance seems to work it is our own deficient faculties which prevent us from recognising the operation of Law and Design' (Jevons, quoted in Aldrich, 1987: 236). The deep-rooted tradition of mechanical determinism in economics abhorred chance. General equilibrium and neoclassical economics consequently favoured order. But order was itself redefined as being so powerful as to emerge even out of disorder – and that was at the core of the probabilistic revolution.

Of course, this was not exactly what the theory was proposing at that time, since no assertion was being made about reality, but simply about its possible representation and about the measurement of the model's adequacy. The evident consequences of this conceptual divergence – as errors were treated as residuals – were not ignored, and were widely discussed. Marshall, among others, voiced his opposition to the estimation method, since 'I regard the method of Least Squares as involving an assumption with regard to symmetry that vitiates all its applications to economic problems with which I am acquainted'.[3]

It is clear that, just as the early explorers of the sixteenth century had written when approaching *terra incognita*, many economists felt that they were facing the danger of the unknown: this part of the map is the territory of dangerous beasts, *hic sunt leones*.

The building blocks of the probabilistic revolution

After the first references had been made to modern probability theory in economics, it was kept at bay for more than a decade (*circa* 1930–44). Pareto, Mitchell and many other economists involved in business-cycle research identified cases of statistical deviations from normality, and Persons and Robbins challenged the probabilistic methods under the argument of a lack of homogeneity over time, anticipating Keynes's critique of Tinbergen. So did Morgenstern, under the argument of a lack of homogeneity in data (Morgan, 1990: 235–6).

Nevertheless, it was Frisch, the econometrician, who became one of the leading voices for the resistance based on doubts and privileged alternatives. Like Tinbergen, his preference was for a complete deterministic system, whose endogenous variables were able to simulate a realistic image of the cycles.[4] Completeness was the necessary and sufficient condition for a system of equations to explain an economic process. Consequently, Frisch favoured the notion that a deterministic system was the best way to describe the functioning of the economy and, in his famous paper on cycles, written in 1933, the inclusion of an error term was not even theoretically justified, being used just for the sake of a better fit to reality. Finally, Frisch introduced the error term as a representation of laboratory stimuli impinging on a deterministic system tending towards equilibrium: this was how far he went on the introduction of the probability concepts. Yet, he did not explain that error term: 'The concrete interpretation of the shock e_k does not interest us for the moment' (Frisch, 1933a: 200–1), he argued. Later on in the same paper, the erratic shocks are presented as a 'source of energy in maintaining oscillations', and, in his model for Schumpeter's forced pendulum, innovations are that source of energy. Following on from the early rhetorical models of pendula for explaining cycles, such as those devised by Fisher (1911) or Yule (1927), Frisch also used the insights from Slutsky (1927), who had investigated the summation of purely random shocks. One divergence went unexplained, however: indeed, the two references used by Frisch in order to explain the nature of shocks, that of Slutsky and that of Schumpeter, were clearly diametrically opposed to each other. As argued in Chapter 7, Frisch did not fully understand Schumpeter's arguments on the nature of innovations under capitalism, in spite of their lengthy correspondence and discussion on the matter, and consequently was unable formally to represent the model that Schumpeter had in mind.

When probability came to the province of economics, two main approaches were available: that of astronomy and that of biology. For astronomy, things

were apparently simpler: errors in measurement were possible but could be easily corrected. Since the analytical universe was composed of independent observations, these could be repeated for the sake of precision and the true model was supposed to have just a few degrees of freedom. In biology, however, evolution and consequently time-dependent observations predominate, but several are available at each point and therefore the crucial question is the relationship between the sample and the population.

At that time, during the course of the first third of the twentieth century, the discrepancies between data and theory were explained: (i) as measurement errors, i.e. errors in variables; (ii) as omitted variables, i.e. errors in equations; and (iii) through a probabilistic approach, which was more general, given the fact that both (i) and (ii) assumed a deterministic system (Morgan, 1990: 193, 241). Frisch clearly favoured (i), arguing that sampling theory could only be applied under the conditions of controlled experiments, whereas Koopmans soon argued for (ii), the omitted variables approach.

Ragnar Frisch argued strongly for the alternative of errors-in-variables and favoured the introduction of the concepts derived from astronomy, opposing the application of the probabilistic approach championed by R.A. Fisher. Indeed,

> he felt that probability and the sampling approach to statistical analysis, developed for use with experimental data in the work of Fisher, was not appropriate for the non-experimental data of econometrics and so he developed his own method of statistical analysis.
>
> (Hendry and Morgan, 1995: 40–1)

Consequently, he developed the confluence method for addressing multicollinearity and the identification problem, and the bunch-maps method for variable selection and model choice, leading to the instrumental variables estimation later introduced by his colleague Reiersol.[5]

Within such a framework, random events are presented either as shocks or as stimuli, but in both cases are simply described outside the model. Furthermore, Frisch tried to propose a new theory of the shocks, but never obtained a rational explanation for their existence. Other young econometricians shared this scepticism. That was certainly the case with his disciple, Haavelmo, who knew and for a while shared Frisch's resistance to the introduction of the probability concepts, and yet it was left to Haavelmo to alter the balance of forces in favour of a sampling approach, essentially with his 1944 thesis on 'The Probability Approach in Econometrics'. Until then, statistical analysis and Least Squares methods had been used, but the probability framework was not generally accepted (Morgan, 1990: 229).

In contrast with Frisch, despite accepting that no experiments were made in economics and that only passive observations were possible, Haavelmo and Koopmans suggested that the probability approach could be used nevertheless and adopted Fisher's view, for which it is assumed there is a hypothetical infinite population, with the actual data being regarded as a random sample of it. By the end of the 1940s, this was largely accepted and marked the second phase of econometrics, the period of 'mature econometrics' (ibid.: 242).

Frisch resisted, but had no strong arguments – his resistance was centred on the denial of the core assumptions of the new approach, and consequently there was scarcely any common ground for the conversation. There is evidence that he never shared the new vision of his colleagues, but also that he looked and could not find any more ammunition for the battle, other than his *prima facie* rejection. His resistance was inconsequential: no bridge was built between these views – if any was intended – and the differences remained a dividing line within the first generation of econometricians. During that period, two strategies vied to overcome the limits that Frisch imposed on statistics and probability: Tinbergen, on the one hand, and Haavelmo and Koopmans, on the other hand, were the main proponents of such strategies.

Samples and quarrels

During the 1930s, Frisch frequently inquired about statistical procedures that could represent alternative strategies to his own methods based on his early work on 'Changing Harmonics' and confluence analysis. This effort led Frisch to consult several researchers in statistics who used sampling and probabilistic inference. In general, these dialogues were inconclusive, paradoxical, confused, sometimes violent and sometimes superficial.

One of the first to be consulted was Alexander Aitken, whom, in 1930, Frisch had asked for technical advice from the point of view of the theory of matrices.[6] Aitken acknowledged the 'congenial developments' between his own work and Frisch's but did not offer any help.[7] Frisch insisted and indicated a list of researchers supposed by then to be using his time series method: Brouwer, computing Uranus longitude residuals; C. Cobb, US data for freight-car loadings from 1917; Thompson, wheat prices in Europe from 1536; Cleland, pig-iron production in the US from 1885; Edmiston, wholesale US prices from 1900; Wolf, rainfall in Boston from 1818; and finally Schumpeter, computing the 'longer cycles' in US prices.[8] Of course, if the relevance and accuracy of these computations is measured from the reference to Schumpeter, who concentrated on qualitative and not on quantitative analysis, it becomes obvious that the list was designed to impress the reader more than to summarise effectively the tests carried out on the time series method. In any case, the correspondence concluded with the list, and Aitken did not contribute any further to Frisch's inquiries.

A longer discussion was, however, engaged in with Schultz and Hotelling – although it also proved to be inconclusive. Henry Schultz was then at Chicago University and researched into probability, although he adopted a sceptical and amused stance:

> During this quarter, I have been spending a considerable portion of my time on probability and sampling. Probability, as you well know, is a puzzling

> field, *one in which we don't know what we are talking about and in which we nevertheless get correct results*'.[9]

Frisch, who could not usually share this sympathy for surprising methods, studied some of Schultz's applications and praised his work, but argued against the 'orthodox correlation methods', considered to be generally unsafe in view of multicollinearity,[10] the reason for his own method of confluence analysis. Otherwise, Frisch argued strongly that econometrics was needed in order to define general laws of social evolution, just as Newton had done for the universe, and consequently this required sound methods discriminating between essential and secondary features.[11]

Indeed, Frisch thought collinearity was not the only difficulty with the 'orthodox correlation methods', other questions being the assumption of normal distribution and the hostility towards intuitive and graphical methods, as he argued in a contemporary letter to Hotelling:

> I quite agree with you that a gap is to be filled in the analysis of significance and accuracy, but I feel that this gap cannot be filled only by the use of standard errors. It seems to me that the use of such parameters is sometimes dangerous in giving an air of exactness to the results which may not be quite warranted. Indeed, the significance of the standard errors thereby needs frequently to be looked into. They only show what deviations can be expected *if* the conceptual sample pattern is such and such, but the main problem frequently lies just in the question of what the simple [sample] pattern is. Assuming normal distribution of the universe from which the sample is drawn is, for instance, a very narrow hypothesis. If we attack this more fundamental problem of the sample pattern, then we may frequently with advantage be guided more by intuition than by mechanical formulae of standard errors. Often this intuition may find a better help in graphical or other short cut methods than in mechanical formulae. This is the reason why I think it is not quite fair to condemn these 'intuitive' methods solely on the ground that we cannot test their exactness with a numerical coefficient.
>
> (Frisch to Hotelling, 13 January 1933)

Frisch's objections were not challenged by a visit to R.A. Fisher, benefiting from his travels to lecture at the London School of Economics and to meet the Cambridge circle in 1934. Indeed, he was very curious about Fisher's work, since it represented the most advanced and innovative progress in modern statistics. Visiting his laboratory, Frisch saw:

> on the spot how they work with tools developed by R.A. Fisher. Of course I knew already some of R.A. Fisher's methods having read a few of his papers, but it was very stimulating to get into personal contact with him. I think I see now much better the underlying idea of their methods. I am sure that for such things as agricultural experiments and perhaps for certain types

of analysis of economic data their tools are very important. But on the other hand I cannot become convinced that these tools furnish a solution to those pertinent problems that refer to the degree of freedom in statistical material that is presented to us by *passive observations* (not by experiments as in agriculture). In the last year I have devoted considerable time and energy to these problems.

I have found that the usual standard error approach and the signification test furnished by the 'Student' t-distribution are no good as indicators of the problems we have to face when we study the hierarchy of linear dependence.

(Frisch to Hotelling, 23 March 1934)

The letter is the more significant given that Hotelling had worked with R.A. Fisher in England, as Frisch certainly was aware. In fact, even after this meeting while visiting England, Frisch remained defiant in relation to the unwelcome application to economics of statistical methods designed for experiments in agriculture; furthermore, linear dependence had entered into the picture, given the independence of variables interpreting social life, and there was no known vaccine against this problem that was immediately available.

At around the same time, Frisch prepared and published his analysis and alternative method for dealing with linear dependence, explicitly rejecting the sampling approach and lessening most of the work done in that direction: 'As a matter of fact I believe that a substantial part of the regression and correlation analyses which have been made on economic data in recent years is nonsense' (Frisch, 1934a: 6). In the introduction, Frisch discarded the probability approach: 'Indeed, if the sampling aspect of the problem should be studied from a sufficiently general set of assumptions, I found that it would lead to such complicated mathematics that I doubted whether anything useful would come out of it' (ibid.), and preferred instead an approach based on numerical experiments aimed at differentiating the meaningful regression equations. Two major criticisms were raised against the sampling approach. First, establishing the value of the coefficients is impossible if they are contaminated by randomness, since they are determinate but meaningless (ibid.: 5). Second, the application of sampling theory was restricted to data generated by experiments and did not include historical processes of time-dependent data (ibid.: 6).

Consequently, Frisch had plenty of motives for feeling that standard regression analysis was faulty: according to him, not only was the assumption of a normal distribution unwarranted, but more fundamentally the non-experimental nature of economic series prevented the extension of methods designed for analysing agricultural data.

Schultz ignored these radical objections and just considered the technical side of the comparison of results. Furthermore, as he looked into Frisch's methods, he suspected they would lead to the same results as the 'standard error approach'.[12] But by that time Frisch was not really interested in comparing his method with 'standard' statistics and simply stated that Waugh was doing 'some

work on that'.[13] Waugh, an agricultural economist, had been corresponding with Frisch since October 1931 and decided later on to come to Oslo University in order to study statistics. In January 1933, Frisch and Waugh concluded a short common paper on time series regression.

Later on, although sharing Frisch's views, Waugh was not convinced his mentor's critique of the 'standard' methods was either deep or clear enough: 'I am not sure that your discussion of the inadequacy of the classical sampling theory goes as far into this subject as it might.'[14] In spite of this, Frisch was very hostile to the current analysis of significance and therefore to the inference procedures:

I think that very few statisticians use the rule of regarding a regression coefficient as non-significant on account of the mere fact that this coefficient occurs in an equation where some other coefficient has a standard error more than, say, three times the standard error of this coefficient. In most cases I believe that each coefficient is considered separately. By going through the text books I think one will also find evidence that the technique of standard errors are as a rule developed with a view to judging each individual coefficient.

But, even though one would adopt the rule of disregarding a whole equation by the mere fact that one of its coefficients had a large standard error, it seems to me that the whole procedure is logically untenable. Indeed, the technique of testing 'significance' in this case amounts literally to: First drawing certain numbers out of a hat and then testing the significance of each of these numbers by drawing numbers out of another hat. Of course the probability that all the numbers first drawn should by this 'technique' turn out to be significant would be very small if the number of variables were great. The probability of getting a complete set of 'significant' coefficients would decrease as the number of variables increase. It would even be possible to calculate the probability of getting a completely 'significant' set. Since the probability decreases with an increasing number of variables one would of course claim that no great risk attaches to this procedure, because there is a very small probability of a positive conclusion; but this result is rather an accidental one. It does not seem to me that one can claim that such a procedure penetrates to the essence of the problem. A criterion of real significance ought to test the coefficients on their own merits, not depending on the randomness of their standard errors.

(Frisch to Waugh, 11 April 1935)

As a consequence of these views, neither Frisch nor Waugh could be accused of sympathising with Schultz's use of classical inference. Furthermore, Schultz had other reasons for his scepticism: both his own and Frisch's methods should be compared under the impression that they could eventually be misleading, since we are 'engaged in a scientific fishing expedition and we don't know what the net will bring up', in particular when 'we don't know which regression makes sense'.[15] Frisch had not the same sense of humour, strongly emphasising that, in the particular case of that fishing expedition, his method was less inclined to yield nonsensical results. And so he suggested an experiment to settle the issue,[16] instead of engaging in joyful exchanges on the philosophy of statistics.

Schultz took pains to experiment both methods and responded violently for once, accusing Frisch of being unable to practise what he preached:

> It is of course true that some statisticians have drawn erroneous conclusions from correlation analyses in which the independent variables were too highly correlated with one another, and that your approach would have exhibited these high inter-correlations. But so would the least square-standard error approach. In fact, I was criticizing these statisticians long before your methods were published for not using least squares standard errors (where they were applicable) and relying solely on free-hand graphical analysis. *I regret that you have never deemed it advisable to make really significant, practical comparisons of the advantages and limitations of the two procedures.*
>
> [. . .] I remember how much I was surprised when I discovered that your method failed me just where I needed it most [he had tried out Frisch's methods on series of barley, corn, hay and oats production]; namely, in those cases in which the standard errors were relatively large. What is wrong with these experiments? [. . .] Both tests failed to support your claim. You announced your method without first subjecting it to a practical test and now that the results of tests are being submitted for your consideration, you dismiss them in silence (I find not a word in your letter about the equations which are contained in my letter of March 4), and ask for more experiments. *When you have given evidence that you are willing to take honest experiments more seriously I shall be glad to cooperate with you in constructing new test cases and in getting to the bottom of the issue.*
>
> (Schultz to Frisch, 10 April 1935, my italics)

This accusation was partly unfair, since Frisch had devoted much effort to perfecting and testing his analytical methods, but yet it had some support from the fact that he despised the alternatives mostly on the basis of an a priori assessment. Frisch simply did not believe methods based on supposedly unexplained assumptions.

Following Schultz's tempestuous attack, Frisch retreated and responded with an apology for any eventual misunderstanding, undertaking, as a consequence, to study his colleague's conclusions.[17] Schultz gratefully acknowledged the letter: 'I am delighted to learn that you intend to take up in the near future the points at issue between us. You may be assured that I will follow your work with much interest.'[18] But no work followed, since this marked the end of their correspondence on the substance of the matter.[19]

Flirting with probability

One of the scholars who has most contributed to our understanding of Frisch's early ideas on econometrics, Olav Bjerkholt, suggested that they were not exactly anti-probabilistic but that the author chose not to develop his views on the issue. As proof of Frisch's interest in the probability approach, Bjerkholt points to a long letter to Schumpeter on the Leontief discussion (Bjerkholt, 2005: 497f.).[20] Considering 'a certain static theory' postulating structural supply and demand relations and the parameters a_{ij} in a determinate system, Frisch stated that:

> $$F_1(x_1, x_2, \ldots x_n, a_{11}, a_{12}, \ldots = 0$$
> $$F_2(x_1, x_2, \ldots x_n, a_{21}, a_{22}, \ldots) = 0$$
> $$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
> $$F_n(x_1, x_2, \ldots x_n, a_{n1}, a_{n2}, \ldots) = 0$$
>
> the set of quantities a_{ij} being the constant parameters in question. The problem of determining such a set of parameters for actual data is an interesting example of an econometric problem.
>
> Now we have the curious situations that if the material at hand fulfils our assumptions it is impossible to determine these constants a_{ij} that express the nature of our assumptions, because in this case we would only have a single observation, namely, the one corresponding to the solution of the system. But if our assumptions are not fulfilled, then it may be possible to determine what they were, that is to say, now it may be possible to determine the constant a_{ij}.
>
> Suppose, for example, that the functions $F_1, F_2 \ldots$ contained also another set of variables, $\xi_1, \xi_2, \ldots \xi_m$, m being at least equal to 1. Our set of structural relations will take on the form
>
> $$F_1(x_1, x_2, \ldots x_n, a_{11}, a_{12}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0$$
> $$F_2(x_1, x_2, \ldots x_n, a_{21}, a_{22}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0$$
> $$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
> $$F_n(x_1, x_2, \ldots x_n, a_{n1}, a_{n2}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0$$
>
> Furthermore, let $\Omega(\xi_1, \xi_2, \ldots \xi_m)$ now be the frequency distribution of the set $(\xi_1, \xi_2, \ldots \xi_m)$. Then to this frequency distribution of the set there corresponds by (3) a certain frequency distribution $w(x_1, x_2, \ldots x_n)$ of the set $(x_1, x_2, \ldots x_n)$. And this latter distribution is known from observation. We see that now we really *do* get variation in the set $(x_1, x_2, \ldots x_n)$. This we may call *the principle of at least one-dimensional indeterminateness* (since m must be at least equal to 1).
>
> (Frisch to Schumpeter, 13 December 1930)

In other words, the uniqueness of the data prevented estimation, unless the addition of stochastic variables allowed for variations in the set of the endogenous variable. But Frisch did not develop this notion, and on the contrary he soon returned to scatter analysis. By 1934, Frisch was to state dramatically that the alternatives were indeterminacy or meaningless estimation, and abandoned any attempt to apply probability in his own work.

Consequently, the treatment of the nature of the error term was always superficial. In the Yale lectures, Frisch stated that

> we can express *any* function of time as the sum of a linear function and a *remainder* term. The nature of the remainder term will then indicate the departure from linearity which the function $X(t)$ exhibits. That is to say, we express $X(t)$ in the form $X(t)=A+Bt+R(t)$ where A and B are constants and $R(t)$ is the remainder term.
>
> (Frisch, 1930: 445–6)

The error is the result of the 'departure from linearity', i.e. it is a consequence of the difference between the data and the model itself. Two years later, when preparing his PPIP, Frisch had nothing more to add.[21]

In 1939, Frisch provided a different definition of 'error', recapitulating a previous discussion that he had had at a conference: 'The first to suggest that the shocks may be measured quantitatively by "errors" in the rational behaviour of individuals was, if I am not mistaken, Professor Divisia in a discussion at the Leiden meeting of the Econometric Society in 1933' (Frisch, 1939: 639). It is obvious, from the *Econometrica* summaries of the conferences, that the issue was frequently discussed. At the Namur meeting (1935), it was raised again:

> The definition which I formulated on that occasion may (when expressed a little more elaborately than in the *Econometrica* summary) be phrased thus: A shock is any event which contradicts the assumptions of some pure economic theory and thus prevents the variables from following the exact course implied by that theory.
>
> (summary in *Econometrica*, January 1937: 89)

As always, Frisch preferred the deterministic mechanical models, i.e. the assumption of pure theory.

In any case, after the end of the 1930s, the flirtation with probability was over. It had been a conflictive flirtation, full of misunderstanding and ignorance, leaving behind memories of disappointment and bitterness. After these decades devoted to the effervescent creation of econometrics, Frisch slid away and just presented his critical remarks on every possible occasion, which were few. The first was in 1951, in his 'Reminiscences' on Schumpeter, who had just passed away, Frisch referred to his friend's insistence on the primacy of theoretical insights over the confined tests of significance, which

> have a clearly defined meaning only within the narrow confines of the model in question. I wish it were more clearly and more commonly recognised by all model builders that all the shrewd mathematical tests are of this relative sort. . . . The final, the highest level of test can never be formulated in mathematical terms.
>
> (Frisch, 1951a: 9–10)

But it was in 1970, in one of his last papers and after being awarded the Nobel Prize, that Frisch voiced his clearest and most violent criticism against what he saw as the development of econometrics. He first criticised the simplified assumptions of the typical estimation procedures, namely linearity: 'Economic life and technical possibility are – just as the pattern of river-beds and bank steepnesses we find in the concrete shape of a country – too diversified to be classified according to some rule derived from oversimplified assumptions [e.g. of constant technology]' (Frisch, 1970c: 159).[22] But this was not all:

> What is the relevance of the intrinsic paths and turnpike type of theorem of the kind I have mentioned? To be quite frank I feel that the relevance of this type of theorem for active and realistic work on economic development, in industrialized or underdeveloped countries, is practically nil. The reason for this is that the consequences that are drawn in this type of theorem depend so essentially on the nature of the assumptions made. And these assumptions are frequently made more for convenience of mathematical manipulation than for reasons of similarity to concrete reality.
>
> In too many cases the procedure followed resembles too much the escapist procedure of the man who was facing the problem of multiplying 13 by 27. He was not very good at multiplication but very proficient in the art of adding figures, so he thought he would try to add these two figures. He did and got the answer 40, which mathematically speaking was the absolute correct answer to the problem as he had formulated it. But how much does the figure 40 tells us about the size of the figure 351?
>
> This example is not intended as a joke, but is meant to be a real characterization of much of the activities that are *à la mode* today in growth theories. In particular it is characteristic of the very popular exercise of investigating what would happen under an infinite time horizon.
>
> (ibid.: 161–2)

This type of procedure was called by Frisch 'epsilontic' exercises, mostly exercised by 'epsilontologists' playing with random numbers (ibid.: 162) – which is not kind to the widespread presence of ε. Moreover, forty years after considering that the nature of the shock, ε, was not relevant for the explanation of 'shock theory', Frisch simply rejected the probability approach as it was used in econometrics. To further aggravate the case, he called these procedures 'playometrics', accusing their supporters of engaging in 'engineering data' instead of delivering real statistics (ibid.: 162, 165). Playometrics meant that 'too many of us often used too much of our time and energy on the study of the keyholes in northern Iceland in the first half of the thirteenth century' (ibid.: 163). For someone who had argued his whole life in favour of econometrics as a tool for decision making and for bettering the human condition, no epilogue could be colder than these distant keyholes of the thirteenth century.

These reminiscences indicate how bitterly Frisch interpreted the efforts of his fellow contemporaries. In a sense, Frisch was dealing with his own legacy in econometrics. Indeed, he had provided the dichotomy between the stabilising system and the exogenous shocks, but rejected the full consequences of this approach, since he did not share the representation of economies as random drawings from the hypothetical universe of experiments. But then the three musketeers, Tinbergen, Koopmans and Haavelmo came along and introduced the generalised probabilistic approach. And, as in the old story of the musketeers, at least one of them, Tinbergen, did not continue along the road with his friends and retired to his empirical castle, looking defiantly out over the work of econometrics.

Tinbergen on cycles and shocks

Tinbergen, a physicist by training, used probability theory as a tool for estimating the parameters of his system of equations explaining business cycles. He was acquainted with modern statistics from his study of quantum physics, and his supervisor, Ehrenfest, himself a student of Boltzmann, worked on analogies between thermodynamics and economics although he never published anything on that subject (Boumans, 1992: 111). Just like Frisch's, Tinbergen's main concern in economics was the social implication of depressions.

In order to assess and control those processes, he used models of harmonic oscillators to mimic cycles, the implication being that 'supply and demand were exact relationships among the observables, with any lack of fit due to errors in variables or nonlinearities' (Epstein, 1987: 34). In that framework, Tinbergen understood exogenous variables as shocks: 'Tinbergen was primarily interested in estimating the coefficients of the lagged endogenous variables that determined the oscillatory behaviour of the system. The exogenous variables represented specific outside economic shocks that excited the equations' (ibid.: 171), and therefore the general class of exogenous variables could be divided into two categories: errors and other variables, all being treated on the same grounds. But this classification was unsatisfactory for everyone: it did not challenge Frisch's reservations and it did not share the points of view that both Koopmans and Haavelmo were endorsing. It allowed for structural estimation – which became the programme for econometrics for two decades – accepting the intellectual framework of the laboratory experiment, even though the empirical results were disappointing.

In 1939, Tinbergen published his famous study on the theories of business cycles based on an econometric model and estimation. It was an ambitious enterprise and Tinbergen trusted it could provide new ground for econometrics,

since 'it is the object of analysis to identify and to test direct causal relations' (Tinbergen, 1939: 8) – a project that very few were able to share at that time.[23] But that was the implication of his laboratory analogy, which Tinbergen developed throughout his life, as he conceived of exogenous variables as instruments and endogenous ones as targets.

This seminal contribution ignited two parallel lines of debate: Keynes criticised the econometrics methods, and the econometricians stood by Tinbergen; but, simultaneously, Frisch strongly expressed his own reservations on Tinbergen's research, and so, although not so emphatically, did Lange and Marschak (see Chapter 7). Once again, the core of the problem was the assessment of shocks, or errors, which consequently defined the nature of probability in economics. In his critique of Tinbergen's conclusions, Frisch proposed distinguishing between two operational concepts: that of 'nature' or 'constitution', the structure of the system represented by the equations, and that of 'disturbance', which denoted 'a deviation from that situation which would have existed as a consequence of the structure'. Disturbances could be conceived of either as 'aberrations', not affecting the subsequent states of the system, or as 'stimuli', affecting the future of the system (Frisch, 1938: 408). The concept of stimulus is clearly a reference to the laboratory framework. Furthermore, Frisch doubted that highly autonomous equations could be established and estimated, meaning that the real explanations for the phenomena were frequently inaccessible, since only coflux equations were deductible from data (ibid.: 416). Consequently, the programme for structural estimation was considered to be utopian.

When he published his book, Tinbergen carefully addressed this discussion and tried to establish a bridge between the concept of measurement error and that of error representing the influence of omitted variables and the problems of sampling:

> According to this method [the 'classical method' of Laplace, Gauss, R.A. Fisher], it is assumed that the unexplained parts – the residuals – are due to the circumstance that the 'explained' variate, though essentially a linear function of the 'explanatory' variates, contains an additional component representing the influence of neglected explanatory variates and may, moreover, be subject to errors of measurement.
>
> [. . .] The probable average magnitudes of these differences [in relation to 'true' values] are derived from the assumption that the disturbances in subsequent time are to be considered as 'random drawings' from the 'universe' of all possible values of these disturbances. In ordinary speech, small disturbances will be numerous and large disturbances will be few, their frequency obeying a simple law.
>
> (Tinbergen, 1939, I: 28)

In the same work, Tinbergen recognised that this stood in contradistinction to Frisch's views:

> Professor Ragnar Frisch, in his treatment of these problems, does not use the concept of some unknown 'universe' from which a 'sample' is drawn. He considers *every* variate as being built of a systematic part and a disturbance. The relations assumed between the variates are supposed to hold good exactly between the systematic parts and the regression coefficients in these relations are called the true coefficients.
>
> (ibid.: 29)

In spite of this, in his applied work, Tinbergen restricted his assumptions to the concept of error representing the omitted variables, as in the investment equation (ibid.: 38n.) and 'purely accidental causes, obeying to the probability law' (Tinbergen, 1940: 76).

The difficulty arose from the effort to explain the rationale for these omitted variables, the 'extra-economic factors' or 'autonomous factors'. What could eventually be represented in this pot-pourri of variables? Inventions, political events, abnormal acts, surprises. What more? Tinbergen was not unaware of the problem and tried to distinguish between two classes of events, according to their impact: 'These influences are considered in this analysis as non-systematic disturbances which act largely accidentally, in an irregular way, like lottery drawings. In general, such influences will exist whenever many mutually independent and small forces are acting, which will be the case in normal times. This is the approach to business cycle problems, which is known as the 'shock theory of cycles'. Some very exceptional events that do not obey these 'laws' will be generally known, so that they may easily be eliminated before the analysis. This has been done, e.g. with the English coalminers' strike in 1926 (Tinbergen, 1939, I: 38).

The difficulty is obvious from the nature of the 'shocks' considered here, as they are distinguished by Tinbergen according to their dimension and distribution and by Frisch according to their durability, with no hypothesis being formed as to any possible distribution. In spite of this difference, Tinbergen did not generalise the implication of the random drawing he suggested. Quoting Koopmans, he even suggested, that the 'classical method' and Frisch's were complementary and not contradictory (ibid.: 32–3). In any case, in a much later retrospective look at the problem, Tinbergen considered that his 1939 estimation did not match Frisch's treatment of the shocks:

> It [PPIP] was only a theoretical model and I did not understand the role of the shocks as well as Frisch did. But I think he was perfectly right, and of course one could indicate some of the exogenous variables playing the role of shocks. The most natural ones would be harvests or crops, and in fact they move as a random series. But there were other shocks as well. Too little effort has been made to identify which were the most important shocks in certain concrete cases. Theoretically, it was a very important concept.
>
> [. . .] On the other hand, I think that what interested economists most was not the shocks but the mechanism generating endogenous cycles, and it might very well be that we have overestimated the role of the mechanism. Maybe the shocks were really much more important. This problem has never been solved, because the War came along and after the War we were not interested in business cycles anymore.
>
> (Tinbergen, 1987: 125)

After a life dedicated to statistical economics, to econometrics, models and estimation, Tinbergen suspected that the error term was still ill-defined:

> The error term is introduced as a catch-all for less important independent variables and for measuring errors of both the dependent variable and the independent variables.
>
> [. . .] Essentially the introduction of an error term is a second best setup and in a way a *testimonium paupertatis*.
>
> (Tinbergen, 1990: 201)

The challenge by the younger generation

Tjalling Koopmans studied physics and mathematics with Hans Kramers, a theoretical physicist, and, after 1934, statistics with Tinbergen, the physicist turned economist. His dissertation was the first econometric work explicitly incorporating a probabilistic approach in the style of R.A. Fisher, which he intended to combine with confluence analysis, as Frisch had proposed. As a consequence, Koopmans decided to travel to Oslo in the autumn of 1935, in order to finish his Ph.D.

Tinbergen introduced Koopmans to his friend Frisch, stating that he was 'a very clever mathematician, studying economics now'.[24] Koopmans' own introductory letter to Frisch recognised the peculiarities of social data and statistical inference in economics, stating that his chief interest was to study

> the problems, arising from the circumstances that classical sampling theory does not regard cases in which observational series develop in time in such a way, that the probability distribution of the second term is not independent of the value attained by the first.[25]

In this sense, Koopmans wanted to study the difficult conditions of the application of classical inference to those series composed of time-dependent observations.

Frisch reacted by inviting Koopmans to study confluence analysis and reaffirmed his difference in relation to R.A. Fisher's approach, which he had discussed with the author himself the previous year:

> With regard to the topic you suggest, here is my reaction. The problem you mention seems to me to resemble very closely those that have been discussed and more or less completely solved by English authors like Student, R.A. Fisher and his school and the group of mathematicians connected with

Figure 8.1 Tjalling Koopmans. Taken probably in 1952 (source: Anne W. Koopmans Frankel).

the Galton laboratory. I do not know how much you know of this literature and how deeply your setting of the problem penetrates, but my first impression was that – at least the set-up mentioned in your letter – does not seem to be very promising of yielding some fundamentally new results. Of course, I may not fully have realised your intentions, but at least I think you ought to point out in what sense the results you are looking for should extend beyond the results obtained by the above mentioned group of mathematical statisticians.

To me it seems that the point where sampling theory now needs to be developed is not so much along the lines you suggest as in the direction of studying the limiting cases that arise when the set of variables considered are *nearly* connected with more than one linear relationship. In other words, what happens when the set of observational variables become multiply flattened? You may know that this has been the topic of a book which I have recently published.

[. . .] Here there is room for much further work, particularly in the direction of developing sampling distributions of the parameters involved.

[. . .] The essence of this problem comes in when a frontal attack is made on the basic problems connected with multiply linear connections. Maybe you would like to devote some energy to these kinds of questions.

(Frisch to Koopmans, 11 April 1935)

It is clear that Koopmans devoted a lot of energy to all these questions, but did not follow the path Frisch appeared to prefer. While in Oslo, Koopmans attended the autumn lectures, but was also able to present his views at a seminar on sampling theory and the Neyman–Pearson methods.[26] The seminar was proposed by Frisch, who attended with Haavelmo, Reiersol and Lutfalla. The young researcher insisted on the possibility of incorporating Frisch's methods into the framework of probability theory. It is obvious that Frisch did not accept this proposal and Koopmans sadly noticed the fact:

At his request [of Frisch, 'this giant of mathematical economics'] I gave some lectures on the new ideas in statistics then being developed in England by R.A. Fisher, J. Neyman and others. However, I did not suceed in persuading him that probability models were useful in assessing the significance and accuracy of econometric estimates. I, in turn, departed impressed, but not persuaded by his econometric approach either.

(Koopmans, 1975)

As Koopmans was researching at the Oslo Institute of Economics and the discussion was already underway, Frisch even tried to recruit some help for his own work. The opportunity arose when Paul Hoel,[27] whom he had met when lecturing in Minnesota in the spring of 1931, asked him about the possibility of coming to Oslo to study statistics. Frisch answered him at length and invited him to come and to compare both approaches:

At the present time Mr. Tjalling Koopmans, of Amsterdam, is here working on a doctor thesis in mathematical statistics. He is particularly interested in building a bridge between the approach in my book 'Confluence Analysis' and the R.A. Fisher sampling approach. The difference between these two points of view is this. In sampling theory, in order to test the significance of a statistical observation, one puts up the fiction of a 'universe', that is some big collection from which the actual observations are 'drawn' in a more or less 'accidental' manner. Whatever assumptions one makes are made in the form of *assumptions about this universe*. This point of view is fruitful, it seems to me, in problems concerning experiments that *can be controlled*. For instance, agricultural or biological experiments. But this theory is very inadequate when it comes to applications in economics, or in social sciences in general, where we most of the time have to accept observations that are presented to us without our being able to influence the results to any considerable extent. In these cases all the problems of confluence analysis crop up, and these can, it seems to me, be better treated by another type of analysis, namely, an analysis where the assumptions being produced *are assumptions about the sampling itself*. For instance, one may assume that each observation is a sum of a systematic part and a 'disturbance', and then introduce assumptions concerning what has been the connection, or lack of connections, between the disturbances *in the sample*. In this way one arrives at identities, exact upper and lower limits, etc., not results which are formulated in probability terms. One does have a means of investigating how a particular constellation of assumptions entails a particular consequence for the result obtained. This analysis of the effects of *alternative assumptions* is very important for applications to economics.

This is of course a very rough outline of the difference between the two approaches. If I should give a fuller statement, I would have to explain that, in some sense, the notion of probability comes in my approach and that, after all, there may be some points of contact between the two approaches. But it would lead too far to go into this in a short letter. I mention it in order to suggest to you a field of research, which, I think, is particularly important and very intriguing.

(Frisch to Hoel, 15 October 1935)

In spite of Frisch's reservations about this 'very inadequate' application of the experimental framework to economics, Koopmans championed these ideas and enlisted in the group of young econometricians dedicated to conquering the profession for the sampling approach. The crux of this approach was the simulation of the empirical and experimental framework of the laboratory and the application of the concepts forged in that framework.[28] Koopmans presented his views as an extension of Frisch's, and indeed he was able to benefit from the rocking-horse and pendulum models:

Following Frisch, each of the variables may be conceived as the sum of two components, a 'systematic component' or 'true value' and an 'erratic

component' or 'disturbance' or 'accidental error'. The systematic components are assumed to satisfy the regression equation exactly . . . the error component is taken as error in the literal sense of the word.

(Koopmans, 1937: 5–6)

But this error 'in the literal sense of the word' was not sufficiently described. Consequently, it is not surprising that Koopmans kept defining the 'disturbance' simply as a cocktail of the omitted variables:

The investigator specifies a number of behavioural equations, the variables entering into each, a simple mathematical form for each equation, and a rather wide class of probability distributions for the disturbances of the various equations. The disturbance in any one equation is here looked upon as the aggregate effect of many individually unimportant or random variables not explicitly recognised in setting up the behaviour equation in question.

(Koopmans, 1957: 200)

Koopmans and essentially Haavelmo built their contribution to econometrics on the assumption that the concept of sampling error was the key to generalised procedures of estimation, and consequently was much more important for economics than pure measurement errors in models with few degrees of freedom, such as those applied to astronomy.

Koopmans himself did not present a convincing explanation for these shocks, and even eventually challenged the concept of the economic time series as random drawings from a hypothetical universe:

In a great deal of problems variables are developing in time in cyclical oscillations, apparently to a large extent governed by some internal causal mechanism, and only besides that influenced, more or less, according to the nature of the variable, by erratic shocks due to technical innovations, variations in crop yields, etc. At any rate, they are far from being random drawings from any distribution whatever.

(Koopmans, 1937: 277)

Nevertheless, he argued that 'it may be better to have some point of support obtained by the use of a set of simplifying assumptions, than none at all' (ibid.: 278) – an accepted *testimonium paupertatis*, in any case. The strategy of the argument was, therefore, to accept the inadequacy of the laboratory approach but to use it as a simplified representation of the economy.

For Koopmans, this adoption of the laboratory framework could be extended to the use of the instrumental notion of exogenous variables in order to represent causality:

The term 'causal connection' is used in this sense of a certain quantitative relationship having a character of necessity as opposed to pure chance.

> [. . .] Important and recognized accidental influences in individual economic decisions are not excluded but are assumed to balance approximately where a great number of economic subjects is concerned.
>
> (Koopmans, 1941: 160)

Consequently, exogeneity was introduced as a core conceptual feature of the statistical model.[29] The acceptance of the laboratory framework for the statistical investigation of economies defined the epistemic status of the new econometrics and provided the model and the techniques for its extension – Haavelmo would later provide the argument for that strategy and it would triumph.

The late 1947 debate on ‘Measurement without Theory’ is paradigmatic of this process of seduction and confrontation in economics. Koopmans took the initiative of reviewing Burns and Mitchell’s 1946 *Measuring Business Cycles*, and suggested economics was still wandering from the Kepler stage (measurement, as in Burns and Mitchell) to the Newton stage (establishment of general and fundamental laws, Koopmans, 1946: 161). Consequently, estimation was presented as the necessary procedure for the identification of a structure, which would provide the law-like theory. In that sense, Koopmans argued that the nature of randomness differed according to the object of each science: in astronomy, randomness is a description of measurement errors, whereas ‘in dynamic economics, the phenomenon itself is either essentially a stochastic process or needs to be treated as such because of the great number of factors at work’ (ibid.: 168–9).[30]

Rutledge Vining, a visiting fellow at NBER, took over the burden of maintaining the debate and argued that it was less about the applicable statistical technique and more about the specificity of economics as a science. Indeed, the Columbia school led by Mitchell and Mills argued for a very specific stochastic approach, predicated upon the evidence of non-Gaussian and asymmetric distributions and conceiving of the economic facts as intrinsically irregular and differentiated, instead of shocks impinged on an equilibrating mechanics (Mirowski, 1989b). Denouncing Koopmans’ methods as a ‘straitjacket’, Vining argued that the difference was over the unit of analysis, the individual agent or the business cycle as such, and the adequacy of Walrasian theory: ‘I think we need not take for granted that the behavior and functioning of this entity [the population] can be exhaustively explained in terms of the motivated behavior of individuals who are particles within the whole’ (Vining, 1949: 77, 79, 82). In his reply, Koopmans defended this precise point and, in his rejoinder, Vining argued for analogies from biology instead of those from physics such as his opponent’s (ibid.: 87, 92). Although the battle was probably motivated by the need to dispute the prestige and favour of the scarcely funding institutions, it had a major impact – which was not evidently favourable to the econometric camp.[31]

In spite of the advances made by the econometricians’ campaign, by the end of the 1940s, they were still faced with widespread scepticism.[32] Just after the ‘measurement without theory’ debate, in 1949, the NBER organised with some universities a major conference on business cycles. At this conference, where

‘historians’ (the NBER researchers) and ‘statisticians’ (the Cowles Commission staff) collided, Schumpeter undertook the task of arguing for the historical method and representing Mitchell, who had recently passed away. In his dual and uncomfortable condition as the author of *Business Cycles* and a distinguished member of the Econometric Society, Schumpeter began with a defensive declaration: ‘I have no wish to advocate the historical approach to the phenomenon of the business cycle at the expense, still less to the exclusion, of theoretical or statistical work upon it’ (Schumpeter, 1949: 308). But he then repeated his main definition: ‘Economic life is a unique process that goes on in historical time and in a disturbed environment’ (ibid.).

History is needed for the inquiry into exogenous, occasional events, but also and essentially into the very organism of the cycle:

> For historical research is not only required in order to elucidate the nature and importance of the non-essentials dealt with so far, but also in order to elucidate the underlying cyclical process itself.
>
> [. . .] But it would not be quite correct to say that historical analysis gives information as regards impulses and dynamic [theoretical] models as regards the mechanism by which the impulses are propagated. . . . Very roughly this is so and I should be quite content if my audience accepts the thesis that the role of the econometric model . . . is to implement the results of historical analysis of the phenomenon and to render the indispensable service for describing the mechanics of aggregates. But the econometric models do more than this – they ‘explain’ situations which in turn ‘explain’ or help to ‘explain’ impulses. And the reverse is also true.
>
> (ibid.: 311–13)

This is a well-known argument, not only because of its search for an incisive counter-logical pedagogy – listeners should be led to accept the historical method for the precise reason they were opposing it – or because of the acceptance of some sort of Frischian formalism of cycles, but also because it indicates to what extent Schumpeter was engaged in the defence of the role of historical research and qualitative methods. And certainly Schumpeter’s final piece of advice did surprise his audience: ‘To let the murder out and to start my final thesis, what is really required is a large collection of industrial and locational monographs [including historical change and the ‘behaviour of leading personnel’]’ (ibid.: 314).

It is well known that his arguments did not change the course of history, and that the econometric revolution was already very clearly on its way. But his arguments, ‘letting the murder out’, surprised some of his colleagues, namely Samuelson, Goodwin and Machlup,[33] but they did not prevent the challenges of the econometricians to the historical method, nor did they forestall the fresh breath of air that general equilibrium models represented.[34] The triumph of econometrics *à la* Cowles was tainted with scepticism, denial and bifurcation.

From trench to trench

Trygve Haavelmo shone as the brightest of the few students of economics in Oslo. Consequently, he was hired to the faculty staff when he was just twenty-one years old – the year Frisch finished his PPIP (1933). For six years, Haavelmo shared the exciting atmosphere of the Oslo Institute, accompanied the complex computations that Frisch was engaged in and lived through the intense exchanges between Koopmans and Tinbergen, as well as the debates with the Cambridge circle.

Haavelmo travelled to Britain with Frisch for the 1936 Oxford Econometric conference, mostly dedicated to reinterpreting Keynes's *General Theory*, at which Neyman was present and presented a 'Survey of Recent Work on Correlation and Covariation' (see Chapter 6). By then, Koopmans had already presented his seminar at the Oslo Institute, and both travellers were aware that new ideas were on the market. But they were eager to know more: in October 1937, Frisch spent one night at Neyman's and, as a consequence of the dialogue, wrote a note, 'The Neyman–Pearson theory of testing hypotheses', on his guest's theory, for publication in *Econometrica*. It is not a rigorous treatment of the method and it is obvious that Frisch had by that time just a pale idea of what this approach was. But he was certainly curious about it, and asked Haavelmo to stay in London with Neyman and Pearson and study sampling theory (Bjerkholt, 2005: 510).

Haavelmo met Neyman again in California and studied with him for a couple of months, learning 'how to do econometrics'.[35] This was an important development in Haavelmo's life: after a first contact with Neyman, he was sent to Geneva to follow the work of Tinbergen and attend a conference on probability theory, and Frisch also advised him to spend some more time with Marschak. In a matter of only a few years, Haavelmo had studied with Frisch, Neyman, Tinbergen and Marschak and then Neyman again – the world tour of econometrics.

When the threat of war began to deepen, Haavelmo took a Rockefeller scholarship and left for the US, where he remained for the whole period of the conflict and where, from 1940 to 1944, he produced the masterpiece of his introduction to the probability approach in economics – between the ages of twenty-eight to thirty-two. He shared with Frisch a vision of an activist economic policy, and saw econometrics as the tool for that – indeed, there was no major split between Frisch and Haavelmo over the perception of economics when he departed to the US, in spite of Haavelmo being more inclined towards the Keynesian *General Theory* than Frisch was (Bjerkholt, 2005: 526). When returning to Oslo after the end of the war, Haavelmo collaborated with Frisch on the organisation of the courses and they both prepared a rather conventional econometrics textbook. After that, he did not contribute anything more substantial to econometrics.

According to Bjerkholt, there was a difference of attitude between Haavelmo, when he was beginning his career, and Koopmans, who came to Oslo under a clear strategy: 'Koopmans in his dissertation *adopted* probability theory in econometric estimation, in contrast to Haavelmo's all-out effort a few years ahead to *adapt* probability theory to econometrics' (ibid.: 505). Consequently,

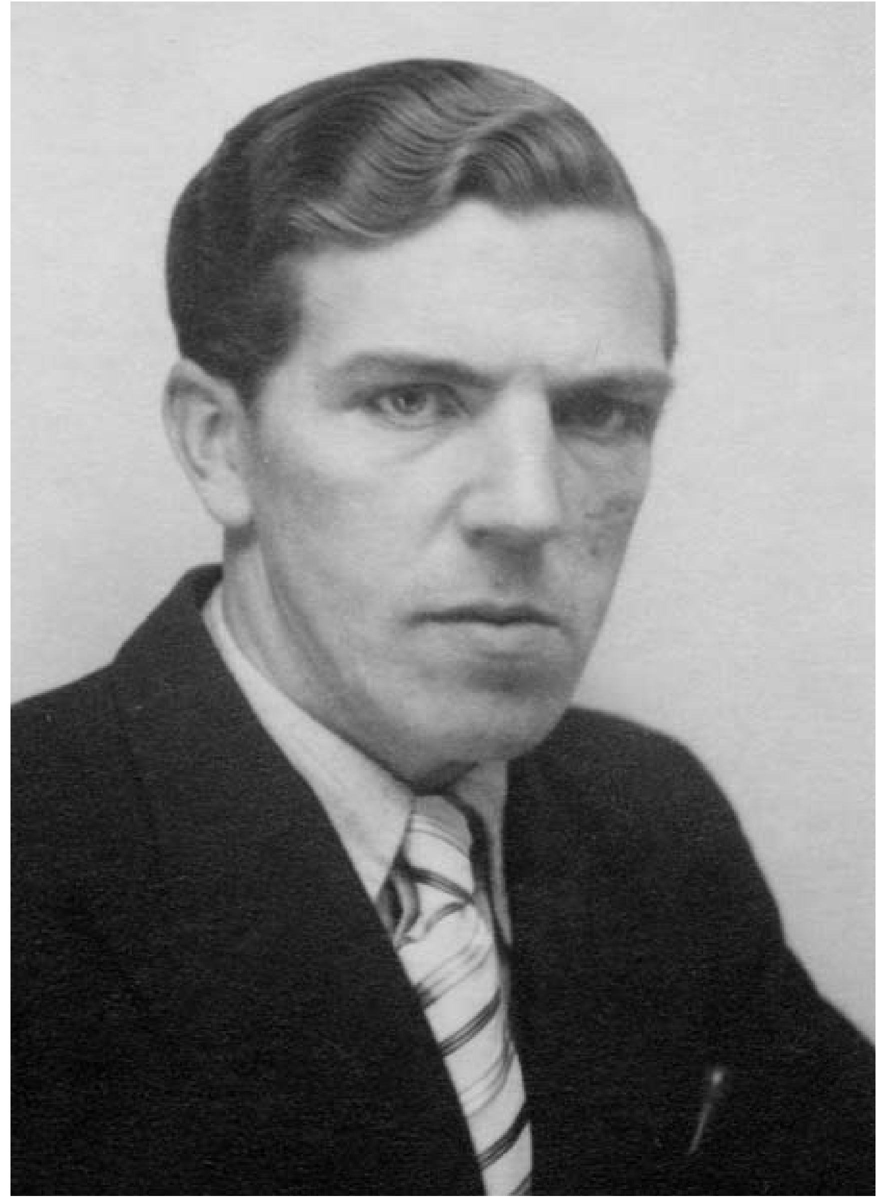

Figure 8.2 Portrait of Trygve Magnus Haavelmo. Taken 10 June 1939 (source: Department of Economics, University of Oslo).

Haavelmo contributed most to the triumph of 'mature econometrics' and obtained a decisive theoretical impact but soon left the movement, whereas Koopmans' position was that of its administrator, remaining at the heart of the American econometric movement all his life.

Just before leaving for the US, in May 1939, Haavelmo presented a paper, 'On the statistical testing of hypotheses in economic theory', to the Third Nordic Meeting for Young Economists held in Copenhagen. It was the very first presentation of what was to become the new research programme. It is very 'Tinbergian' in tone, since the main topic is how statistical tests can enable discrimination among theories:

> Anyone who has worked in economic theory knows how it often is the case that several different 'correct' theories can be put forward to explain the same phenomenon. The differences are in the choice of assumptions. One comes all the time to cross-roads where one direction *a priori* seems as plausible as another. To avoid it all becoming just a logical game, one must at each step have these questions clearly in view: Are there realistic elements in my reasoning, or do I operate in a one hundred percent model world? . . . It is here that the requirement of statistical verification comes to the rescue, prevents the reasoning for running astray and forces a sharp and precise formulation of the hypotheses. The statistical corroboration saves us from many empty theories.
>
> (quoted in Bjerkholt, 2005: 518–19)

Yet Haavelmo conceded that many economic problems evade precise quantification:

> The circuit of problems relating to the testing of hypotheses is not exhausted by the question of the *degree of precision* in the agreement between data and a certain hypothesis. The key problems in the hypothesis testing lie actually prior to that stage in the analysis. It turns out – as we shall see – that many hypotheses cannot at all be verified by data, even if they are quantitatively well defined and realistic enough. Yes, we can be led astray if we try a direct quantification.
>
> (ibid.)

But this did not deter the author and he proceeded to outline the conditions for the statistical treatment of time series, based on the decomposition between trend and cycle:

> In our formulations of theoretical laws we operate always with things of such nature that they *can be thought of as repeating themselves*. This holds both for static and dynamic formulations of laws. The most important economic data are given as time series, thus a quite particular series of successive events. Is it possible to test laws for recurrent events on the basis of such time bound variations? . . .

> Economic time series usually have two features that strike the eye: one is the one-sided straight development, the trend, the other is certain variations *around* the trend. Often we can track the cause of the trend back to certain slowly changing things (e.g. changes in population size or structure), things that are outside the range of entities included in our hypotheses and also seem to be independent of the variations we wish to study. In such case it is natural to take the trend as a *datum* in the analysis and consider the things that happen *apart from* the trend. This is the rational basis for a statistical elimination of trend in our observations. It is unacceptable to make a purely mechanical trend elimination without a concrete interpretation of the trend's emergence. . . .
>
> When our test data are series with marked trend movements, it could be asserted that the hypotheses we can get verified, will not be laws for recurrent events, but only a description of a historical path. If that viewpoint had to be accepted *in general*, it would be a severe blow for the attempt of establishing economic laws. But we don't have to accept this negative position. The cause of the trend is either outside our system of hypotheses, and if we can state the causes, we are allowed to eliminate the trend and consider only the residual variation, which has the character of recurrence. Or, the trend derives from the structure of the system under consideration, it is the outcome of an analysis of free variations and has its explanation by the *same* system of hypotheses which led to variations of recurrent nature.
>
> (ibid.)

The presentation of the cycle as the residual from the trend extraction suggested a new research programme, but it had still to be greatly refined. Haavelmo himself very soon provided the framework for such a development.

The first step was to reconsider Frisch's model of the rocking horse, suggesting an alternative interpretation of cycles. In a 1940 paper, Haavelmo wondered 'what type of errors we have to introduce as a bridge between pure theory and actual observations', and considered two types of errors: (1) additive errors superimposed on the theoretical time movement of the variables in a deterministic process generating the cycle; and (2) 'another way is to introduce the errors explicitly in the original set of fundamental equations describing our model', as Tinbergen did. In that case, 'the unexplained residuals enter merely as errors of estimation, and they may be small', although playing a 'much more fundamental role': without errors there are no cycles (Haavelmo, 1940: 312–14).[36] Haavelmo intended 'to explain observed cyclical movements by the combination of a structure which is noncyclical, but which contains inertial forces, and outside influences of random events'.[37]

The next step was to provide the foundations for the use of probability as an inner characteristic of economic processes. This Haavelmo proposed in a 1941 mimeo, *On the Theory and Measurement of Economic Relations*, prepared in Cambridge, Massachusetts, which is the basis for his influential 1944 paper. The paper introduces the Neyman–Pearson strategy as the basis for the probabilistic approach and acknowledges the author's long collaboration with Frisch as well as comments and suggestions from Schumpeter and Hurwicz. Haavelmo boldly presented his aim, the reconstruction of the statistical foundations of economic theory:

> The application of such simple 'statistics' [tools of statistical inference] has been considered legitimate, while, at the same time, the adoption of definite probability models has been deemed a crime in economic research, a violation of the very nature of economic data. That is to say, it has been considered legitimate to use some of the *tools* developed in statistical theory without accepting the very *foundation* upon which statistical theory is built. For *no tools developed in the theory of statistics have any meaning* – except, maybe, a pure descriptive one, *without being referred to some stochastic scheme*.
>
> (Haavelmo, 1941a: ii)

According to Haavelmo, this was necessary in order to establish laws, the very purpose of theory, because, 'I think, such a phrase as this: In economic life there are no constant laws,[38] is not only too pessimistic, it is simply meaningless' (ibid.: 22):

> Our hope in economic theory and research is that it be possible to establish constant and relatively simple relations between dependent variables, y (of the type described above), and a relatively small number of independent variables x. In other words, we hope that for each variable, y, to be 'explained', there is a relatively small number of explaining factors the variations of which are practically decisive in determining the variations of y.
>
> (ibid.: 35)

As the determination of these approximate laws required statistical inference, Haavelmo concentrated on defining the conditions for the application of the sampling approach. He rejected the widespread reluctance based on a very narrow concept of probabilities: the simile of random drawings of lottery numbers and the concept of each observation being independent of the previous ones from the same population. He addressed these objections:

> From this point of view it has been argued that e.g. most economic time series do not conform well with any probability model, 'because the successive observations are not independent'. But there are no reasons for such a narrow limitation of the application field of probability schemes. It is not necessary that all observations should follow the same one-dimensional probability law. It is sufficient to assume that the *whole set* of, say n, observations may be considered as *one* observation of n variables (or a 'sample point') following a n-dimensional *joint* probability law, the 'existence' of which may be purely hypothetical. Then, one can test hypotheses regarding this joint probability law, and draw inferences as to its possible form, by means of *one* sample point (in n dimensions). Modern statistics has made considerable progress in solving such problems of statistical inference.
>
> (ibid.)

And the solution was:[39]

> There is no logical difficulty involved in considering the 'whole population as a sample', for the class of populations we are dealing with does not consist of an infinity of different individuals, it consists of an infinity of *possible* decisions which might be taken with respect to the value of y. And all the decisions taken by all the individuals which were present during one year, say, may be considered as one sample, all the decisions taken by, perhaps, the *same* individuals during another year may be considered as *another* example, and so forth.
>
> (ibid.: 12)

Haavelmo readily recognised the obstacles to the full extension of this model of inference, since induction requires the repetition of the experiment under constant conditions. He conceded that in economics there are just two types of experiments: (a) experiments planned by the researcher, picking a sub-group of the population, under limited conditions; and (b) experiments performed by Nature itself (ibid.: 10). Consequently, the crucial question was the determination of the available experimental framework, a problem not peculiar to economics that could be solved as the theory was defined:

> Here one would, first of all, think of the difficulties which arise from the fact that series of passive [historical] observations are influenced by a great many factors not counted for in theory; in other words, the difficulties of realizing the conditions: 'other things being equal'. . . .
>
> If one cannot clear the data for such 'other influences', one has to try to introduce these other influences in the theory, in order to bring about more agreement between theory and facts. Also, it might be that the data as given by economic time series, are bound by a whole system of relations, such that the series do not display enough variations to verify each relation separately.
>
> (ibid.: 28f.)

It was certainly not an easy task, but Haavelmo claimed it was possible, in spite of the peculiarities of social processes, to infer the probability law: 'The problem of estimation is the problem of drawing inference, from a sample point, as to the probability law of the fundamental probability set from which the sample was drawn' (ibid.: 75) – it was just a question of taking the risk.[40]

In particular, Haavelmo strongly challenged the idea that 'small shocks' could be added without any reference to an explicit model defined in a probabilistic approach, an obvious critique of Frisch's approach:

> Without further specification of the model, this procedure [assuming small shocks] has no foundation, and that for two main reasons. First the notion that one can operate with some vague idea about 'small errors' without introducing the concepts of stochastical variables and probability distribution is, I think, based on an illusion. For, since the errors are not just constants, one has to introduce some more complex notion of 'small' and 'large' than just the numerical values of the individual errors. Since it is usually agreed that the errors are 'on the whole small' when individual errors are large only on rare occasions, we are led to consider not only the size of each individual error but also the frequency with which the errors of certain size occur. And so forth. If one really tries to dig down to a clear formulation of the notion of 'small irregular errors', or the like, one will discover, I think, that we have, at least for the time being, no other practical instrument for such a formulation than those of random variables and probability distributions, nor is there any loss of generality involved in the application of these analytical instruments, for any variable may be 'probabilized', provided we allow sufficiently complicated distribution functions.
>
> (Haavelmo, 1943a: 457–8)

Furthermore, for Haavelmo, the probabilistic framework was necessary for two main epistemological reasons: for the generalisation of the useful statistical applications and for the correct representation of the 'nature of economic behaviour'. On the one hand, 'We need stochastical formulations to make simplified relations elastic enough for applications', and, on the other hand,

> the necessity of introducing 'error terms' in economic relations is not merely a result of statistical errors of measurement. It is as much a result of the very nature of economic behaviour, its dependence upon an enormous number of factors, compared with those which we can account for explicitly in our theories.
>
> (ibid.: 454)[41]

It is also important to notice that Haavelmo argued that the assumed distribution of probabilities was merely a feature of the model, i.e. of the imagination of the modeller, echoing some of Frisch's ideas: 'The rigorous notion of probabilities and probability distribution "exists" only in our rational mind, serving us only as a tool for deriving practical statements' (Haavelmo, 1944: 48). And

> since the assignment of a certain probability law to a system of observable variables is a trick of our own, invented for analytical purposes, and since the same observable results may be produced under a great variety of different probability schemes, the question arises as to which probability law should be chosen, in any given case, to represent the true mechanism under which the data considered are being produced.
>
> (ibid.: 49)

Furthermore, as he had argued before, splitting the variables into a 'theoretical part' and an 'error part' is 'relative to the particular system of theoretical equations we are concerned with in each case' (1941a: 13–14).

Consequently, the introduction of the probabilistic approach derived from utilitarian arguments: the forerunners did not argue that it was the only appropriate concept for interpreting reality, but rather that it was useful for representing it. Furthermore, as stochasticity was introduced through the concept of error, and statistical inference is only possible if the error has certain desired properties, the whole edifice was based on somewhat shaky foundations. But the revolution was proceeding: instead of Frisch's errors-in-variables, Haavelmo stabilised an alternative strategy of errors-in-equations (Haavelmo, 1941a: 18), which was the condition for a generalised probability approach.[42]

The impact of this shift in economic thought was impressive. Stone considered the 1944 paper to be the best exposition of economics after the Keynes–Tinbergen debate (Stone, 1946: 265). Allen praised the empirical framework defined by Haavelmo, who used the 'stream of experiments that Nature is steadily turning out from her own enormous laboratory' (Allen, 1946: 162). The Cowles group was jubilant with this new direction that econometrics was offered and, contrary to the previous case of the Keynes–Tinbergen debate, wholeheartedly adhered to the new approach. Few voices were raised against this strategy.[43]

But that was no longer the concern of Haavelmo. Surprisingly, although continuing to follow the work of his colleagues at a distance, he turned after the war to other problems, namely those of decision models following the Oslo mould. In spite of this change of direction and given his immense prestige, Haavelmo was still elected president of the Econometric Society in 1957 – only to deliver a very negative assessment of the state of the art in the discipline: 'the concrete results of our efforts at quantitative measurement often seem to get worse the more refinement of tools and stringency we call into play'. His Frischian programme for economics was openly stated: 'We have a very important task of formulating and analysing alternative feasible economic structures, in order to give people the best possible basis for choice of the kind of economy they want to live in' (Haavelmo, 1958: 354, 356).

Twenty years later, in 1989, recapitulating this lecture in his Nobel Prize acceptance speech, Haavelmo added that 'if I were asked today for an evaluation . . . I would probably use almost the same words, but I would give them a more drastic content': he suspected the adequacy both of the available economic theory and of the estimation methods (Haavelmo, 1989). In this sense, Haavelmo was as caustic about 'mature econometrics' as Frisch was in his later lectures. In particular, he felt economics ignored the nature of social life:

> I think it is not unfair to describe a major part of existing economic theory in the following way. We start by studying the behavior of the individual under various conditions of choice. Some of these conditions are due to the fact that the individual has to have contact in his economic affairs with other individuals. We then try to construct a model of the economic society

> in its totality by a so-called process of aggregation. I now think that this is actually beginning at the wrong end. Consider this: in the world of today there are more than five billion people. If they should try to live without being members of some society, I suppose most of them would be dead in a few weeks. There is of course the old moral question of whether the individuals are there for the sake of the society, or vice versa.
>
> [. . .] Putting it in a somewhat demagogic way I would say that without society there would be practically no individuals, and without individuals there would of course not be any human society.
>
> (Haavelmo, 1989)

In other words, economics should not deal solely with single agents but with society as such – just as Vining had argued against Koopmans. By this same token, Haavelmo suggested alternative models of social life. But this lay outside the scope of linear modelling and traditional estimation presuming structural stability. It was eventually outside the scope of econometrics too.

The rocking horse still kicking

The immediate consequence of the probability revolution in economics, the transformation of the programme of econometrics that reinvigorated the General Equilibrium theory, lies outside the scope of this book. It is enough, for the purposes of this story, to indicate how, in the 1940s and 1950s, the Cowles Commission became the centre for econometric research into structural estimation: a Walrasian model was adopted, based on the aggregation of agents, and consequently the cycle was defined as a deviation from the equilibrium. In a sense, this meant a return to the point of departure.

Indeed, two path-breaking changes had already altered the map of econometrics since the early 1930s. The first was Frisch's model of models for business cycles, the rocking horse or the pendulum. The rocking horse was a formal model whose mechanism could explain a damping cyclical response to an external excitation and, although he did not formalise the model of the pendulum, this was also supposed to allow for a forcing term and for exogenous stimuli. Consequently, stochasticity was introduced under the guise of these exciting shocks.

Haavelmo, incorporating stochasticity as the essence of the model itself, introduced the second change. Attaching less importance to the role of additive shocks, he argued that the structure of dynamic relations was by itself stochastic, and this was his criticism of both Frisch and Tinbergen. This consequently meant the abandonment of the model of the controlled laboratory experiment and the purely mechanical model. The very conception of the design of the experiment proved how far removed Haavelmo was from any meaningful translation of the rigid protocols into economics: the definition of the experiment should be based on the observation of the individual agent making choices, the model being an a priori hypothesis about the decision-making process (Haavelmo, 1944: 6). Haavelmo mercifully added that the construction of the theory should be such

that its design of experiments be close to that of Nature (ibid.: 25), but was wise enough never to include in this framework an extended definition of cognition, information and decision making by the agent. This was of course completely at odds with R.A. Fisher's concept for the design of experiments, strictly linked to the capacity of controlling the protocol: 'In considering the appropriateness of any proposed experimental design, it is always needful to forecast all possible results of the experiment, and to have decided without ambiguity what interpretation shall be placed upon each of them' (Fisher, 1935a: 12). The complete description of all possible results contrasted with Haavelmo's transfer of the protocol to the exclusive domain of the decision-making process, consequently ignoring the requirement of the complete description and interpretation of the possible outcomes of the decision. He was not engaged in making an experiment, but addressing a process mimicking an experiment, just for the sake of the extension of the statistical methods developed in and for the laboratory.

Of course, both the physicists and the biologists conducting such experiments could claim to refer to law-like assertions interpreting the determinate process or even explaining the production of variation. But economics could not establish any of these attributes. The solution to this conundrum was to create a new ontology, reconstructing economic data as a description of one of many different parallel worlds – and Haavelmo's dynamic stochastic processes were better suited for this purpose than the previous, although different, contributions made by Frisch and Tinbergen, both alienating randomness from their deterministic models and naturally excluding sampling from the parallel worlds. Later, Anderson and Hurwicz supported Haavelmo arguing that there were two types of stochastic disturbances: errors-in-variables and shocks-in-equations (Anderson and Hurwicz, 1949: 23).

Consequently, after Haavelmo's impressive *debut*, there was no further obstacle to the requirements of the Neyman–Pearson strategy: the explanation for the econometricians' preference for their method over Fisher's is inscribed in this ontological choice for the description of the universe of data. Since the Neyman–Pearson technology was based upon the acceptance of successive drawings of samples of the distribution, this fitted in with Haavelmo's redefinition of economic series. In contrast, Fisher rejected the likelihood of the repetition of draws and the definition of the error depending on the structure of the test. Furthermore, Fisher rejected the definition of the power of the test depending on the nature of the alternative hypothesis and opted for significance tests measuring characteristics of the sample itself and not dependent on the peculiar attributes of each test. He did not conceal his frontal disagreement with his colleagues:

> I find myself in disagreement with some modes of exposition of this new subject which have from time to time been adopted, that I have taken this opportunity of expressing a different point of view; different in particular from that expressed in numerous papers by Neyman, Pearson, Wald and Bartlett.
>
> (Fisher, 1955: 69)

The econometricians were on the other side.

Yet this was not all. Although Haavelmo's description of economic data matched the requirements of the Neyman–Pearson methods in a stochastic world of worlds, mechanical determinism was very soon back on the agenda.[44] This stochastic essence of economic dynamics was fundamentally opposed to one of the central tenets of the theory: equilibrium. Although this question was carefully addressed later on, in the 1940s there was no solution to this apparent contradiction between stochastic irregularity and general equilibrium. Subtly, econometricians accepted both the Neyman–Pearson methods and Haavelmo's foundations, obediently acquired their techniques but forgot about their implications, returning to the combination of external shocks plus neoclassical deterministic equations, or uninformative white noise plus equilibrium. In this sense, the rocking horse was everywhere, alive and kicking, long after it had been placed on the road to oblivion.

'I am not a moderate'

The recourse to Frisch's rocking horse and its endurance as a metaphor highlight the difficulties in accepting the epistemic and ontological claims of the new approach. Haavelmo's impressive exposition was greeted with enthusiasm but scarcely followed – it lent support to the widespread use of stochastic concepts but indeed it did not lead to a genuine reinterpretation of economic data as random drawings from imagined populations. In such a case, the essence of the process would be its stochasticity; instead of this, the prevailing argument was that equilibrium processes determined part of the economies whereas some variables escaped this framework and behaved randomly. Encapsulating all omitted variables – regardless of the adequacy of the model – as these random variables and supposing that they cancelled each other out, equilibrium was again reinstated as an approximation of the two dimensions of the rocking horse: propagation and impulse.

Consequently, Frisch's model was not only responsible for the introduction of the random error but also guaranteed its survival in practical empirical research. In spite of this, scholars are divided in their interpretation as to how this contribution related to the probabilistic approach. Epstein refers to Frisch's 'non-probabilistic thinking' (Epstein, 1987: 72n.), Duo argues that Frisch rejected probability (Duo, 1993: 10) and so does Morgan (1990: 234). Andvig, however, suggests that Morgan dramatises Frisch's divergence, particularly in relation to Haavelmo (Andvig, 1995a: 52n.), and Bjerkholt emphasises that Frisch did not attack Haavelmo's dissertation as its discussant and was closer to it than is currently suggested (Bjerkholt, 1995: xiv n.).

The textbook written by Frisch and Haavelmo between 1950 and 1953, 'Elementer av den matematiske statistikk' ('Elements of Mathematical Statistics'), provides a partial solution to this divergence. Although the textbook was never completed, Frisch concluded almost all his chapters, unlike Haavelmo. The book discoursed at length on probability theory, mostly inspired by the Neyman–Pearson theory, with few references to R.A. Fisher: after introductory

chapters on the core concepts of statistics and discrete and continuous distributions laws, two chapters present the theory of estimation and the testing of hypotheses. The last chapters, by Haavelmo, deal with regression, variance and time series.

It was up to Frisch to introduce estimation, prediction and the testing of hypotheses, including probability in the interpretation of time series, and he did so in accordance with Haavelmo's seminal contribution. The framework is rigorously established with a discrimination between empirical event and mathematical probability (Frisch, 1950a), and the central limit theorem is called upon to represent the convergence of the samples from the summation of stochastic variables, explaining the diversity of phenomena in social life.[45] Prudently, Frisch added that the central part of the distribution should be used as the best proxy for reality.[46]

Furthermore, Frisch presented the programme for estimation as the empirical determination of the parameter defining a distribution function and the use of significance tests, although considered that there Significance with major S was not reachable by statistical applications: 'Whatever the human instruments we use, our knowledge will never be complete' (Frisch, 1952a: 2).

This was the reason for important differences in relation to the standard theory of statistics. First, Frisch attributed the inductive power of statistics exclusively to the domain of the treatment of data and not to the probabilistic inference, and, in that regard, he deviated from other researchers:

> One may ask: What can we *obtain* from the logical consequences from a postulated probabilistic distribution? All the consequences are already inserted in the conditions of the model. We don't get anything 'new' from the use of this method. It is therefore right that all the logical consequences *were there* in the postulate of the primordial probabilistic distribution. But we didn't see them.
>
> (ibid.: 4–5)

Furthermore, in a second major divergence from statistical theory, Frisch also rejected the predictive power of inference: 'Prediction *doesn't* mean that *from an a priori known probabilistic distribution* we are allowed a probabilistic statement on the event' (ibid.: 11).

In spite of this vigorous scepticism, Frisch carefully explained to his students the legitimacy of the recourse to the Haavelmo foundation of probability in economics. Generally applicable methods[47] were adequate to conduct empirical estimation and in particular the testing of hypotheses, following the Neyman–Pearson method and the strategy of the minimisation of the error of the first type (Frisch, 1952c: 2, 19). Although he was not a prime mover, Frisch learnt in detail the methods that Haavelmo and Koopmans were proposing in the style of Neyman and Pearson, and he was ready to explain them, although he never used such methods in his work. And the criterion of his own research is the only one that is reasonable enough for the clarification of Frisch's attitude in relation to the sampling theory.

However, at the very same time, he also concentrated on significance tests. But, for the assessment of interviews, he defended the use of decision models:

> In the analysis of the results of such interviews special forms of statistical tests of significance are needed. And here my theme begins to converge towards the type of problems which come under the general heading of risk-theory as applied to econometrics. Indeed, decision-making based on a decision-model is essentially probabilistic in character, and one aspect of this probabilism is the statistical significance of the results of interviews used in the search for *autonomous* relations.
>
> (Frisch, 1952a: 6)

In this framework of decision models, not only did Frisch not apply the stochastic theory himself, but he also rejected its usefulness in general, considering furthermore that the recourse to probability was an obstacle to the acquisition of knowledge:

> [although recognising the importance of the stochastic approach] I have a feeling that at the present stage the minimum factor for programming at the national – or even international – level is a comprehensive analytical scheme where there is a great number of aspects included. We therefore have to economize on forces by provisionally neglecting the probabilistic refinement and reason as if we have certain structural equations to work with. In other words we assume that these structural equations themselves are constant. We must do the best we can and in the first approximation aim at a deterministic, i.e. non-stochastic model.
>
> (Frisch, 1962a: 100)

In successive instalments, Frisch elaborated on this rejection of the recourse to the 'probabilistic refinement'. In the seminar that he gave at the Vatican, in 1963, Frisch engaged in a debate with Wold and argued that

> as I have said, I am absolutely in agreement with him on the ultimate need for introducing the stochastic viewpoint. Wold rather had the feeling that we had to introduce the stochastic viewpoint already from the beginning. [. . .] My answer is: if we try to introduce the stochastic viewpoint from the beginning in these immense models we are facing an impossible task.
>
> (Frisch, 1963a: 1228)

The implication is that Frisch knew the Neyman–Pearson methods and also taught them, and it was out of neither ignorance nor contempt that he chose not to use them. Simply, he thought it was a matter of priorities and considered that the development of purely deterministic models was the crux of the matter in economic analysis.

In a lecture that he gave during the same period at the Japanese Keio University (9 June 1960), Frisch insisted again that 'of course, I am all for a thoughtful stochastic theory, but it must be formulated in such a way that you can express a hypothesis about the data generating mechanism' (Frisch, 1960: 10) – a totally excessive requirement for the rocking horse, as if the nature of the shocks could illuminate the propagation mechanism. The chairman, acknowledging a very clear lecture, kindly thanked him for being more moderate than expected: the fame preceded Frisch. The transcript indicates that Frisch immediately replied that 'I am not moderate.' He was not wrong about that – he was certainly one of the few econometricians rejecting the use of probability theory at that time.

Determinism and randomness

The primacy of the rocking horse in econometric analysis was a belated victory, since it vitiated Haavelmo's concepts. The summation of an equilibrium system and well-behaved random disturbances enjoyed widespread use in an attempt to establish the procedures of inference, control and prediction, and yet it refused the purely stochastic approach. Frisch was himself just preoccupied with the horse and less with the shocks, although most of the users interpreted the model as the rationale for both deterministic equilibrium and randomness.

But simultaneously with these developments, which extended over a period from *c.*1930 to the 1960s, the concepts of both dynamic equilibrium and randomness were slowly mutating. The concept of mathematics was itself the subject of intense disputes between the Hilbert programme – shared by John von Neumann and the axiomatic approach as it came to be in economics – and the challenge by other mathematical schools, as well as by information such as that posed by Godel's theorems on incompleteness.

In particular, some distinguished physicists challenged the universe of certainty and mechanical determinism in different fields of science, including their own. Some of the previous generation, such as Poincaré and Maxwell, had already argued that it was difficult to match equilibrium and change, or order and disorder, given the complexity and nature of the systems or the multiplicity of explanatory variables, generating perturbations from the working of the system itself. Maxwell, who suspected all causal explanations, argued in 1873 that it is a metaphysical doctrine that from the same antecedents there follow the same consequents, particularly given the presence of instability related to the large number of variables acting on a system.

In his *Science et Méthode* (1908), Poincaré detected cases for which 'a very small cause which escapes our notice determines a considerable effect that we cannot fail to see, and then we say that that effect is due to chance'. Moreover:

> Why have meteorologists such difficulty in predicting the weather with any certainty? Why is it that showers and even storms seem to come by chance, so that many people think it quite natural to pray for rain or fine weather, though they would consider it ridiculous to ask for an eclipse by prayer? We

> see that great disturbances are generally produced in regions where the atmosphere is in unstable equilibrium. The meteorologists see very well that the equilibrium is unstable, that a cyclone will be formed somewhere, but exactly where they are not in a position to say; a tenth of a degree more or less at any given point, and the cyclone will burst here and not there, and extend its ravages over districts it would otherwise have spared. If they had been aware of this tenth of a degree, they could have known of it beforehand, but the observations were neither sufficiently comprehensive nor sufficiently precise, and that is the reason why it all seems due to the intervention of chance.
>
> (ibid.: 67–8)

Previously, in his 1889 research on the three-bodies problem, Poincaré had already indicated that not all dynamic equations are integrable and, consequently, that there is no possible prediction of the trajectories of all systems. In these cases, new qualitative methods are needed to study differential equations: the world of astronomy was suddenly understood to be not so simple as Newton's laws and the measurement errors suggested. When it came to the new generation of econometricians, all these insights were already well known in physics.

Frisch was perhaps the economist best suited to follow Poincaré's concepts and insights, and he had the opportunity to do so, since during his time in France he had studied the mathematician's contributions. Indeed, Frisch's interest was mostly dedicated to his writings on chance and determination: amongst his many books by Poincaré, Frisch mostly marked the passages on chance.[48] He read the French editions of *Science and Hypothesis*, *Science and Method*, *The Value of Science* and *Chance*, just as he read many other contributors to statistical theory, such as Descartes, Galileo, Diderot and Borel. There is no indication that these readings were used by Frisch for any purpose other than philosophical reflection.

In any case, as astronomy ceased to furnish the model of scientific legitimacy, the debate in economics was turned upside down, since the first interpretation of errors had been derived from its solid foundations as simple errors of measurement. Furthermore, at the time when economists had translated it, this simplification was already being subject to challenge in physics: by Poincaré, although no economist had understood him, but also by Boltzman and others, as quantum physics and statistical mechanics defined the landscape of the long probabilistic revolution in the period leading up to 1920, and, in this case, all other quantified sciences understood them. But in economics this influence was more of a pretext than a context: *pace* Haavelmo, the generalised probabilistic approach was instead interpreted under the framework of the juxtaposition of strong equilibrating forces plus irrelevant shocks,[49] and consequently randomness was artificially insulated.

The definition of the error was therefore the issue, *quod errat demonstrandum*. Of course, chance could be interpreted as the 'intersection of independent causal consequences', and understood only in relation to human experience in

precise historical situations, chance being only an answer to a question posed by humans (Ekeland, 1993: 121–2).[50] But, in economics, the prevailing interpretation was that order and chance are strictly dichotomic, independent and simply additive in some form. And, although Haavelmo argued that a probabilistic framework was needed to describe the genesis of economic processes, this dichotomy became a dogma for the interpretation of time series.

Moreover, legitimate order was interpreted by neoclassical economics as a structure of relations representable in a Hamiltonian framework, in which everything is known and all events are exogenous. Consequently, errors should necessarily be considered as external to the system and their theoretical status was diminished as a consequence. Consequently, general equilibrium models tended to resist and to refer to the framework of conservation of energy: if instead economic systems were conceptualised as dissipative, then randomness would gain another status. In such a case, randomness is not a stream of 'errors' or 'perturbations', but part of the essential structure of events and relations, and that was the insight both from Maxwell and from Poincaré. In social sciences, there is yet another level of complexity, the intersection of institutions, strategies and choices.

Evolutionary biology developed one of the possible conceptual frameworks for the consideration of such complexity. Natural selection is defined as a two-stage process, emerging out of sexual combination and random mutation. This produces variation that is independent of adaptive advantage and selective pressure. And then there is a second process: natural selection through the external constraints – there are internal and external causes of evolution, and these are independent and parallel. Darwin himself did not know anything of genetic evolution to be able to conclude his theory in this sense, but he established the basis for this new science, and the later interpretation of Mendel's experiments provided the missing link in the theory.

It is worth noting that, eight years after the publication of *The Origin of the Species*, Darwin was challenged by statistical wisdom. Jenkins, a physicist from Glasgow, argued that it would be highly improbable that variation could overcome the conservative effects of inheritance, and that normally a regression to the mean would operate after mutation, imposing a conservative evolution of the transmission of traits. Galton, who shared this view of the role of the regression to the mean, nevertheless argued that discontinuous variation was still possible. Darwin maintained the same view: small changes could be positively selected, through a slow process of cumulative changes, *natura non facit saltum* – if he had known genetics, he would have been able to prove that no regression to the mean was possible.

It was in this context that two mathematical biologists whom we have met before, R.A. Fisher and a Cambridge professor, Yule, investigated evolution and provided a number of techniques to assess change and mutation, such as the analysis of variance. Fisher did not appeal to indeterminacy or exogenous stochasticity, simply considering a multiplicity of causes to determine mutation and the interplay of adaptation and selection. As a consequence, order, or necessity, and disorder, or chance, were interpreted as parts of the same universe of determination.[51]

My argument is that the very concept of *error* has been pivotal for the appropriation of different metaphors in economics. In fact, the 'error' lived through three major epochs. It was first defined as a measurement error, as in astronomy, claiming for economics the Laplacean certainty that physics was supposed to exhibit. This error was either too large or too limited to interpret social processes. Second, the 'error' was reconceptualised as a residual from the estimation of a model, requiring a well-defined and uncontested law as a reference for measurement: it is obvious that the 'residual' is only equal to the 'error' provided that there is an authoritative law describing the universe. Consequently, the notion of 'error' in economics was never stabilised, since something was lacking: the law itself is the problem. Moreover, general equilibrium imposes a restriction on change, the residual was necessarily treated as a 'perturbation' or 'shock' – an external impact on the equilibrating system, the third major interpretation of the 'error'. Economists need to err to be right, and if they get their errors right then they will be right and able to model with no error.

Economists never agreed upon each of these three interpretations, but the last one tended to dominate. Frisch rejected the comparison of economics with other sciences that were able to deliver controlled experiments, and as a consequence he rejected the assumptions about the sample, resisting probability altogether. Koopmans assumed probability just as a simplifying assumption with no necessary ontological statement. Tinbergen used these analytical tools, but remained suspicious of them. Haavelmo was indeed the first to assume stochasticity as the very nature of economic behaviour, but he used for his computations the framework of general equilibrium as a convenient representation of this economic system. This, of course, had tragic implications, since, in the context of Walrasian economics, change is supposed to come from outside the economy.

In biological evolution, however, errors arise from random mutations, and are selected through the interplay of social and natural forces. Exogenous and endogenous stochasticity are considered and, once a mutation is selected, the 'error' may generate a path-dependent trajectory – the name of which is evolution. The *error* is therefore part of a construction of change, which implies a powerful ontological claim on the nature of evolution. The population can be understood by following the rules of the game: replication, variation and selection. This identification of rules as a privileged tool for understanding dynamic processes is a candidate for an alternative investigative method in social evolution, based on populations, agents and institutions. R.A. Fisher argued for such a framework, but his opportunity for influencing economics simply paved the way for the shift of the theory towards the Neyman–Pearson methods.

Part IV

Theory and practice at the edge

9 Chaos or randomness

The missing manuscript

On Wednesday, 5 April 1933 at 17.45 – and one may presume it was precisely at that time – Ragnar Frisch gave a lecture in the Amphithéâtre Darboux of the Institut Henri Poincaré, 11, Rue Pierre Curie, in Paris. Its title was: 'Conclusion: The meaning of the social and mechanical laws. Invariance and rigidity. Observations on a philosophy of chaos'. It was the final lecture in a series of eight, dealing with mathematical economics and advanced topics of econometrics – indeed, the first lectures in the history of economics using the concept of econometrics.

It was a very important event, at least for three major reasons. First, the lecture was presented to a selected audience at the Poincaré Institute, the home

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M. Ragnar FRISCH, Professeur à l'Université d'Oslo (Norvège), fera HUIT leçons sur le sujet suivant :

PROBLÈMES ET MÉTHODES DE L'ÉCONOMÉTRIE

1. Fondements philosophiques de l'économétrie. La méthode axiomatique. L'utilité en tant que quantité.
2. Exemples de théories économétriques statiques et semi-statiques. Monopole, polypole. La notion de force.
3. Qu'est-ce qu'une théorie «dynamique» ? Propriétés des systèmes déterminés et indéterminés.
4. Exemples de théories économétriques dynamiques. Oscillations des systèmes clos. La théorie des crises.
5. La création des cycles par des chocs aléatoires. Synthèse entre le point de vue probabiliste et le point de vue des lois dynamiques déterminées.
6. La construction statistique des fonctions économétriques. Equations confluentes. Le danger des analyses à plusieurs variables.
7. La technique des séries temporelles. Décomposition des séries. Opérations linéaires et leur problème d'inversion.
8. Conclusion : La signification des lois sociales et mécaniques. Invariance et rigidité. Remarques sur une philosophie du chaos.

Ces leçons auront lieu à l'Institut Henri Poincaré, 11, rue Pierre-Curie, Amphithéâtre Darboux, aux dates suivantes :

Vendredi 24 Mars, à 17 h. 45 ; Samedi 25 Mars, à 14 h. 15 ; Lundi 27 Mars, à 17 h. 45 ; Jeudi 30 Mars, à 17 h. 45 ; Samedi 1er Avril, à 14 h. 15 ; Lundi 3 Avril, à 17 h. 45 ; Mardi 4 Avril, à 17 h. 45 ; Mercredi 5 Avril, à 17 h. 45.

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Le Doyen de la Faculté des Sciences,
CH. MAURAIN.

Figure 9.1 Poster announcing the lectures by Frisch at the Poincaré Institute in Paris. This was the very first series of lectures ever using the term 'econometrics' (source: Institut Henri Poincaré, University of Paris).

place of Emile Borel, allowing for a discussion among some of the best mathematicians of the time.[1] Second, it was delivered by one of the founders of the newly-born econometric movement and of the Econometric Society, and it provided a crucial opportunity for the presentation of its aims. Third, it dealt with chaos, a rather obscure notion, encompassing some of the fringes of the science: a contribution on chaos in economics at that time would have been an outstanding innovation. Given the fact that the emergence of this concept in our science is usually traced back only as far as the writings of von Neumann in the 1940s,[2] and that for a long time econometrics had been based on what we might call a linear orthodoxy, this manuscript may well represent a watershed in the unknown history of the discipline.

The first section of this chapter presents the outline of the argument of that paper, as far as one can gather from the partial evidence available. The topic is reassessed in the second section from the point of view of the debates surrounding the introduction of the probabilistic approach into economics, and these early concepts of chaos and randomness are then discussed.

The missing link

In spite of the fact that Frisch had prepared the texts of all his previous lectures prior to publication,[3] apparently he did not do the same with the conclusion. Indeed, the manuscript could be yet found neither in the archives at Oslo University nor in those of the Oslo University Library. Frisch was very meticulous and kept a comprehensive archive, which was classified and organised in a professional way by his colleagues and disciples, and the paper was not found there either. Nor could it be found at the Poincaré Institute itself or among the papers of those who were responsible at that time for the institute's management, such as Fréchet, whose papers at the *Académie des Sciences* do not include any version of this lecture. Available evidence points to the hypothesis that it was not committed to paper.[4] On the other hand, since there is no trace of any reference to this topic in his copious correspondence with his young colleagues and co-workers in econometrics, one may further conclude that this philosophically inclined dissertation is an absent item in the rich collection of Frischiana.

Of course, at least by that time, the study of nonlinear systems and, a fortiori, of chaos, was far removed from Frisch's main concerns. Most of his work in the late 1920s and in the 1930s was devoted to the establishment of econometrics based on linear specifications and therefore excluded complexity and chaos. Frisch became one of the main figures in the mathematisation of the discipline thanks to his disciplined, rigorous and insightful contributions, which did not include these badly behaved and non-computable systems of equations. Belonging to the early generation of econometricians, Frisch developed his life's work within a framework of simplicity or disorganised complexity (more about this later) and did not investigate the alternative of organised complexity.

We would thus be completely in the dark about Frisch's 1933 argument in Paris if he had not taken up the issue again, much later on, in 1951, in his Memorandum of 21 February, section 25F, corresponding to a lecture given at the Oslo Institute of Economics.[5] Moreover, the content of his 1951 lecture, based on the Paris lecture, was later emphasised in no less than his Nobel acceptance speech. This was delivered in 1970: Frisch had broken a leg in 1969, while climbing a mountain, and could not attend the reception in order to make his acceptance speech at the same time as the other scientists who were awarded the prize that year. Consequently, his speech was only delivered the next year, on 17 June 1970.

It was a fairly solemn occasion: Frisch and Tinbergen were the first economists to be awarded the Nobel Prize, and this represented a just recognition of their many years of work in defence of economics, econometrics and science. It was the perfect opportunity for Frisch to present an overview of economic science. And so he did, but the text of his speech is quite surprising. It includes brief sketches of arguments rather than any coherent orientation, and it ends rather abruptly after presenting some quite independent investigations and comments, as if Frisch were just delivering his agenda for future research. Perhaps this was all he intended: his intense capacity for work and devotion to new ideas was legendary, and after a whole life spent in the forefront of his science he felt, perhaps more than ever, compelled to provide new leadership, since he was so deeply disappointed with the course that econometrics was taking at that time.

That is probably why he stressed the need for a 'much broader perspective' and suggested a challenging discussion on the 'ultimate reality', 'in the sense of a theory of knowledge', concluding with a dramatic plea for wisdom, which was considered to be broader and more fundamental than knowledge. After arguing about the difference between intelligence and wisdom, atomic theory and the experimental nature of the principle of symmetry, Gell-Mann's concepts of the quantum world of elementary particles, matter and anti-matter, astronauts and Jevons, Frisch turned his attention to the problem of the 'Philosophy of Chaos', recapitulating his concluding Poincaré lecture.

The argument briefly runs as follows. Frisch considered the empirical distribution of two variables, x_1 and x_2, and their linear transformation into another pair of variables, y_1 and y_2:

$$y_1 = b_1 + a_{11}\, x_1 + a_{12}\, x_2$$

$$y_2 = b_2 + a_{21}\, x_1 + a_{22}\, x_2$$

Of course, if the Jacobian of the transformation is singular, whatever the distribution of x_i, the distribution of the pairs of y_i will be one-dimensional: y_i will lie in a straight line. Let us take a very simple example, in keeping with Frisch's argument. Let us suppose the following randomly distributed pairs of x_i:

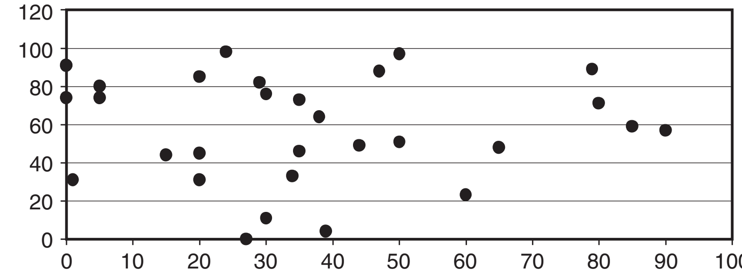

Figure 9.2 Empirical distribution of the Xi.

Then, if a singular transformation is applied to this distribution, we have

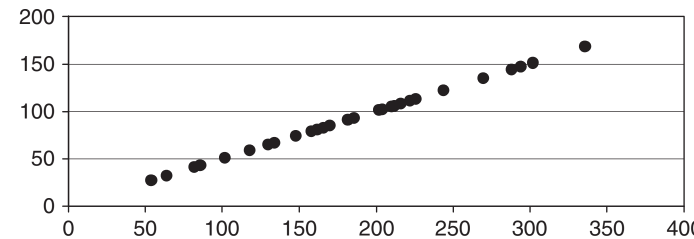

Figure 9.3 Linear transformation with a singular matrix.

The practical implication is that, given the outstanding evidence of a statistical correlation between y_1 and y_2, if one is tempted to consider y_i as the cause of y_j and to measure this relationship as the non-zero slope of the segment, the result is quite conclusive. Yet it is wrong: 'This "cause", however, is not a manifestation of something intrinsic in the distribution of the x_1 and x_2, but it is only a human *figment*, a human *device*, due to the special form of the transformation used' (Frisch, 1970b: 217). The statistical evidence is thus a trick of the imagination. Furthermore, Frisch proves that it is always possible to find a non-singular transformation that provides whatever strong correlation one may desire. The implication is that one can always manipulate claims of causality.

The reverse problem also poses a similar difficulty. If we just know the y_i, possibly 'with a false correlation', how can we be sure that our observation (Figure 9.3) is not derived from an empirical distribution such as that shown in Figure 9.2? The answer lies in the second part of Frisch's argument. The next step does in fact have a broader philosophical implication, since it relates to a specific theory of knowledge, presented as a paradox.

If the impression of regularity and correlation is derived simply from a specific transformation of real data, then the orderliness of nature is suspect. Indeed, the perception of regularities or the definition of covering laws may just be a human artefact: the transformation is the decisive feature, whatever the nature of the empirical distribution of data. It may be, then, that the epistemic primacy of laws in science is just a tautological consequence, a necessary but not necessarily correct self-confirming conception of Nature as something that is ordered. In that case, laws exist simply because we look for them with tools that provide spurious evidence of their confirmation.

But Frisch then proposes another possible interpretation. It may also be that the singular transformation that allows for regularity, lawfulness, order and its scientific interpretation is just a product of social evolution, an imposition upon reality – not a figment of the imagination, but a real effect of the human struggle to survive, to evolve and to dominate. Therefore, the transformation that imposes order would be a product of social reality and not merely of the imagination.

Biological processes, just like their interpretation in science in general, would then tend to create regularity and to impose this type of transformation: or, as Frisch suggests, there may be an evolutionary process selecting both the systems and the humans more prone to finding or creating regularities, i.e. generating a Lamarckian evolution rather than a Darwinian one:

> If the 'ultimate reality' is chaotic, the sum of the evolution over time – biological and scientific – would tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. How can we possibly on a scientific basis exclude the possibility that this is really what has happened? This is a crucial question that confronts us when we speak about an 'ultimate reality' Have we *created* the laws of nature, instead of *discovering* them? Cf. Lamarck vs. Darwin.
>
> (ibid.: 219)

Curiously enough, Frisch does not try to resolve this challenge. He immediately steps back from the philosophical implications of these hypotheses and then simply adds a general reflection:

> What will be the impact of such a point of view? It will, I believe, help us to think in a *less conventional way*. It will help us to think in a more advanced, more relativistic and less preconceived form. In the long run this may indirectly be helpful in all sciences, also in economics and econometrics.
>
> (ibid.)

This is paradoxical. First, the author toys with the idea that 'ultimate reality' is chaotic and that regularity is imaginary. Then, he argues that order may be introduced not just by intellectual artefacts, but also by the process of social action over Nature, and even that selection may act in such a way as to join together biological evolution and science in the choice of 'transformational' agents. Finally, he concludes that, whatever the ultimate reality in fact is, this may help us think in a less conventional way. This is the least one can say, since realistic and relativistic assertions are mixed together in the argument and it becomes impossible to distinguish between fact and fiction.

But the ending is even more puzzling, since Frisch suggests that what is needed is simply a commitment to the betterment of society. And he finishes the

section of his paper by quoting an Indian friend, a former ambassador: 'Understanding is not enough, we must have compassion.' There is science, but there is more knowledge than science. And that is the end of the story. His paper proceeds in the next sections to present a 'brief survey of the development of economics in the last century', followed by a detailed history of the foundation of the Econometric Society, and leading finally to a discussion about the measurement of the preference function, national and local plans and specific techniques for planning, without making any more references to the first topic.

Complexity, chaos and randomness in early econometrics

Since the early generation of the founders of econometrics were so deeply engaged in positivistic reasoning, the Poincaré lecture is quite exceptional since it introduces an interesting philosophical question about the nature of determinism. It even suggests a relativistic conclusion: there is nothing in science itself that asserts its own truth or provides a scientific foundation for the validity of scientific propositions. Regularities, expressed in science as laws, may be a figment of reality or else constructed reality – achieved either through a Darwinian process of selecting regularity-building agents or else through a Lamarckian process of striving for perfection and understanding[6] – and there are no means for discriminating between these two opposite conjectures.

The Poincaré and subsequent lectures are also outstanding documents since they highlight Frisch's great distance in relation to the introduction of the probabilistic approach into economics. For what he terms 'complete or pure chaotic distribution' is nothing more than 'randomness' in modern parlance. No chaos whatsoever is involved in this demonstration (Figures 9.2 and 9.3). Yet, the very use of this term – in 1933, in 1951, in 1970! – emphasises Frisch's mistrust of the role played by randomness in economic and statistical explanations.

In 1933, four months after the Paris lectures, Frisch concluded his major work on cycles: the paper for the Cassel *Festschrift*, 'Propagation Problems and Impulse Problems in Dynamic Economics' (Frisch, 1933a). The paper represents a damping and equilibrating mechanism that mimics cycles, to which Slutskian random shocks are then added for the sake of realism, as discussed at length in Chapter 6. The damping propagation mechanism, creating inertia and driving the system towards equilibrium, also performed the role of creating order and represented the filtering by the social system of these shocks from outside – from 'chaos'.

Nevertheless, kicks that maintained the oscillations were vital for the functioning of this rocking horse. Yet Frisch did not explain either the nature or the origin of these decisive sources of exogenous energy, which account for the movement. A brief discussion of the problem is provided at the very end of the paper, and Frisch, who had discussed this topic at some length with Schumpeter, suggested that one interpretation of this energy was possibly provided by the Schumpeterian innovations. But Schumpeter was not entirely satisfied with this representation of his model of cycles, to say the least, and there is an incongruent leap from the small, irrelevant and unexplainable Slutskian shocks to the major, systemic and explanatory Schumpeterian shocks (see Chapter 6). Lacking an explanation, Frisch simply ignored the fundamental question.

The same paper provides another conjecture for solving the Poincaré lecture puzzle. Discussing Slutsky's concept of the spurious cycles generated by the summation or averaging of random shocks, Frisch suggested a new interpretation: the economic damping mechanism is the real counterpart to the intellectual process of summation of the multitude of exogenous variables. Cycles therefore exist since they are the result of the social system (represented by the deterministic system of simultaneous equations) absorbing the exogenous shocks. Since, at roughly the same time, he was engaged in the preparation of the Poincaré lecture, this suggests that Frisch's normal answer should be that regularities do indeed exist, even if they are the product of social action upon the 'absolutely chaotic distribution' of events. For Frisch, 'chaotic' was the equivalent of 'indeterminate', or random.[7]

Frisch, like Tinbergen, was deeply involved in the concept of science as the semantics of determination: both were very sceptical about the permanence of randomness, and their original understanding of formal models was that they should be determinate and closed (Boumans, 1995: 129f.). On the other hand, this corresponded to the long-standing concept of positivist science: Laplace had developed the probabilistic theory but considered Nature to be purely deterministic, as did Kant or, later, Peirce. For them, randomness was the expression of ignorance, a 'figment' of human frailty. In the same sense, for Newton and Hume, all events flowed from necessary causes, and chance was just the name for a concealed cause. And until the end of the Second World War, statisticians considered in their majority that Nature did not create randomness, but that humans had invented the lottery for this purpose (Klein, 1997). Consequently, the econometricians of the early 1930s adhered to a strictly deterministic point of view, and Frisch was one of them, if not their forerunner.

Within this framework, an interpretation may be suggested for the Poincaré lecture. It broadened the scope of determinism, since, by implying that even if this was artificially created by the human striving after regularity, it was relevant as the very object of science: 'This search for regularities may well be thought of as the essence of what we traditionally mean by the word "understanding" ' (Frisch, 1970b: 219).[8]

In this sense, Frisch's lecture is further evidence of his mistrust regarding the development of statistical inference based on the properties of the random term, which did not play any role whatsoever in his work. There were two major reasons for this. First, Frisch did not believe that true causal relations could be detected through statistical means, and that was one of his major arguments in the debate with Tinbergen, which was sparked off by Keynes's criticism of the latter's work on a model of cyclical fluctuations for the US economy (see Chapter 7). Second, he feared the extension of the probabilistic approach to pattern detection in historical time series. Although he never voiced this objection and never publicly criticised his disciples' work in this area, he did not hide

his lack of comfort in relation to the courageous effort made by Haavelmo and Koopmans. This clearly indicates Frisch's attitude to the probabilistic approach to the statistical analysis of time series, and therefore his early opposition to the extension of the role of randomness into applied economic models. His notorious insistence on this point of view for several decades also proves that Frisch did not alter his opinion: randomness was equivalent to chaos and understanding it required its reduction to lawfulness and order – and this was what science was about.

The same may be said for Tinbergen, who shared the same belief in the primacy of deterministic models – the rationale for naming randomness as 'absolute chaos', suggesting its unknowable character. One simple example is provided here to prove this contention, and to show that for Frisch, as well as for Tinbergen, this stance did not alter for several decades. Even when a new concept and a rigorous treatment of chaos were made available (just as they were for randomness), Frisch and Tinbergen stuck to their earlier idea.

In his last years, Tinbergen responded to a challenge by Kurt Dopfer, who criticised his views and argued for a complexity approach in economics, by writing that he did not know the question well enough to argue about it. And then came the crucial phrase for the argument of this chapter, with Tinbergen adding that, unlike him, Frisch had come across chaos:

> Finally, some words on chaos theory. Here I simply have to admit that I never studied or applied it and that I increasingly have become aware of its potentialities. It is a relatively new area, although Ragnar Frisch came very close to it some decades ago . . . It is a subject for the future.
>
> (Tinbergen, 1992: 256)

Even in 1992, like Frisch in 1970, Tinbergen did not distinguish between chaos and randomness: both were outside the scope of his theorising, but what he could recall was Frisch's meandering efforts to discuss the matter. Yet, evidence was found that at least on one occasion, Frisch and his colleagues considered a nonlinear formulation, in order to represent a complex system of market interaction between two agents, and such a system implied chaos. This system was conceived of as a result of a challenge by Tinbergen and (mostly) Koopmans to Frisch's views as expressed in his famous 1934 paper on 'Circulation Planning', published in *Econometrica*.[9] But, at that time, Frisch merely expressed the nonlinear system under a mathematical form and did not attempt to solve or simulate it, although he was still able to understand that its behaviour depended crucially on the initial conditions and the values of the parameters. Indeed, the system is chaotic, but the three authors could recognise neither the fact nor the consequences, as discussed in the next chapter.

The problem, of course, is not the understanding of chaos: it is the misunderstanding of randomness. One could argue that both concepts have remained ill-defined, both in the past and today. And this is true. At least one must agree with Tinbergen's very last remark: all this is a subject for the future. Indeed, it is.

10 Is capitalism doomed?

A Nobel discussion

Three future Nobel Prize winners – although they had to wait for some decades, since at the time of their discussion it was not even attributed to economists – debated in 1935 nothing less than the future of capitalism. This chapter investigates a simple nonlinear dynamic model constructed by Ragnar Frisch in order to settle that discussion, which remained unpublished for decades. Although Frisch neither solved nor simulated the behaviour of the system, he eventually understood that at least some very complicated dynamics emerged from it.

Although this is not the only instance of his concern with the wild side of the street, throughout his life Frisch carefully avoided publishing any nonlinear model and argued that linear specifications were satisfactory. Yet, evidence shows that he looked around for something else. The current model is the proof that in this quest he found complexity.

Will capitalism collapse or equilibrate?

The paper which gave Frisch the Nobel Prize (PPIP) is a landmark in the history of economics: it was written and published in 1933, presenting an ingenious three-dimensional mixed system of difference and differential equations to account for several modes of oscillation (see Chapter 6). It represented a major achievement, remaining for a long time as the accepted definition of the dominance of linear systems and decomposition methods in the early econometric programmes.

Yet, the model investigated in this chapter is rather different. It was included in a three-page typewritten document, dated 1 October 1935, under the suggestive title 'The Non-Curative Power of the Capitalistic Economy – A Non-Linear Equation System Describing how Buying Activity Depends on Previous Deliveries'. The first and second of these pages are entirely dedicated to the explanation of the purpose of the exercise:

> During the Namur meeting of the Econometric Society [1935] a discussion arose between Tinbergen, Koopmans and Frisch. . . . Koopmans maintained that in reality the flexibility of prices would come in as an important element, which would probably counteract the tendency to contraction of

> activity that was displayed by Frisch's system [1934]. . . . This is of course nothing but a mathematical formulation of the liberalistic argument. Frisch took the position that, even though flexibility of prices were introduced, it would be quite possible to have a system showing exactly the same general features as the system [Frisch's 1934 model]. Indeed, he maintained that the flexibility of prices may even *aggravate the situation*.
>
> (Frisch, 1935b: 1)

The difference between Frisch and Koopmans on the role of the market would be a permanent dividing issue. They had the opportunity to discuss it while Koopmans was around in Oslo, by the same autumn of 1935 – evidence proves that Frisch was very attentive to Koopmans' innovations, at least as far as probability concepts were concerned. They eventually discussed other topics, such as the analysis of the characteristics of markets, as they did during the Namur conference. In any case, three decades after this night of discussion, both would exchange arguments exactly in the same sense at a seminar organised at the Vatican, and again Koopmans raised against Frisch and defended that market mechanisms and price information was enough to convey information for decision making and consequently that planning was not required (Koopmans, 1963: 1215–16).

While in Namur they had tried to settle the question and the three scientists adopted quite a singular procedure: Koopmans would formulate the assumptions and Frisch would represent the mathematical form of the model and discuss its solutions, with Tinbergen outside the direct confrontation. Indeed, Koopmans completed his part of the bargain, since he indicated the economic relations to be embodied in the model. Frisch defined it, although he did not explore the behaviour of the system: 'This I plan to do on a later occasion' (Frisch, 1935b), which never occurred.

From all points of view, this is an exceptional document. Here were three founders of modern mathematical economics and econometrics discussing the structure of capitalism and exploring new mathematical insights. Furthermore, in order to define a more realistic model of a simple economy of production and exchange, they constructed a nonlinear model, a quite uncommon feature at that time and definitively unusual in comparison with their previous and future work. In this framework, Frisch and his colleagues understood that they were forced to resort to numerical simulations in order to uncover its dynamics. Although there is no evidence that Frisch ever took up the issue again with his challenger after the formulation of the model, the paper confirms that at least the author suspected the emerging properties of the model, since 'even if flexibility of prices were introduced, it would be possible to find such a set of values of the constants in the equation (the influencing parameters), that would entail a contraction' (ibid.).

This discussion followed the publication of a simpler model by Frisch in one of the earlier issues of *Econometrica*: 'Circulation Planning: Proposal for a National Organization of a Commodity and Service Exchange' (1934a). In that paper, Frisch reacted with indignation to the most outstanding feature of the crisis of the 1930s: poverty amidst plenty, that 'monstrosity' that follows from the specific mode of organisation of modern industrial societies, as he wrote. Frisch concluded in a rather gloomy way: 'Under the present system, the blind "economic laws" will, under certain circumstances, create a situation where these groups [of producers] are forced mutually to undermine each other's position' (Frisch, 1934a: 259).

Frisch illustrated his argument with a simple model of an economy with one shoemaker and one farmer, each one producing for the other's consumption, and assuming that their decisions on production were made on the basis of the sales in the previous period. Accordingly, each one's sales determined his level of expenditure. Considering the level of prices to be fixed, the sales of the two agents, the shoemaker and the farmer, would be (symbols are adapted, namely according to those of the 1935 model):

$$S_1(n) = \alpha\, S_2(n-1) \tag{1}$$

$$S_2(n) = \beta\, S_1(n-1) \tag{2}$$

What ruled the dynamics of this very simple interrelationship was α and β, which Frisch called the 'coefficients of optimism': if the agents were in an expansive mood, trade would increase; otherwise, if they were in a saving mood, 'the whole system will gradually dwindle down to nothing' (ibid.: 263) – a Keynesian argument for the expansion of consumption. Frisch indicates the general solution:

$$S_1(n) = A_1\, \mu^n + (-1)^n\, A_2\, \mu^n \tag{3}$$

$$S_2(n) = B_1\, \mu^n + (-1)^n\, B_2\, \mu^n \tag{4}$$

where $\mu^n = (\alpha\beta)^{n/2}$, A_i and B_i depending on the parameters and initial conditions. In (3) and (4), the first element in the right hand side of the equation is obviously an exponential trend, whereas the second is a cycle with two phases. If $\mu > 1$, we have the cycle superimposed on a rising trend, but if $\mu < 1$, then trade vanishes. Therefore, cycles and the eventual collapse of the economic system were related to its mode of trading – the concrete form of organisation of the 'liberalistic' society – and not specifically to the very existence of the market as a social institution (ibid.: 272). As a consequence, Frisch suggested the urgent implementation of a national system of planned exchange using credit control, and later on argued that the ensuing events, such as the outbreak of protectionism and then of the world war, confirmed the insights and the importance of the action proposed according to his model.

As we saw, Koopmans strongly disagreed and argued that the introduction of flexible prices would modify the behaviour of the modelled economy and allow for the continuation of trade. And that is how the 1935 model came about: the

new version of the 1934 model was to include a specific form of flexibility: prices could be changed according to a parameter of action of the agents.

The model (equations 5–10) defines two agents ('*primus*' and '*secundus*'), producing and exchanging in much the same way as in the 1934 model. The very simple nonlinearity is introduced with the definition of sale for each of them: price times quantity (there are no stocks). This defines the first two equations, where the subscripts identify *primus* or *secundus*:

$$S_1(n) = P_1(n)\, Q_1(n) \tag{5}$$

$$S_2(n) = P_2(n)\, Q_2(n) \tag{6}$$

Afterwards, Frisch assumed that the supply of *primus*'s good was a (negative) function of the price of his own good and a (positive) function of the previous growth of sales of *secundus.* A minimum quantity is always traded, â and ê. Therefore, quantity is fixed by the market conditions and the demand equations are:

$$Q_1(n) = \hat{a} - \alpha\, P_1(n) + \gamma\, (S_2(n-1) - S_2(n-2)) \tag{7}$$

$$Q_2(n) = \hat{e} - \beta\, P_2(n) + \lambda\, (S_1(n-1) - S_1(n-2)) \tag{8}$$

Finally, Frisch hypothesised that the growth of prices, whose parameter was fixed by the seller, was a proportion of the previous growth in sold quantities:

$$P_1(n) - P_1(n-1) = \psi\, (Q_1(n-1) - Q_1(n-2)) \tag{9}$$

$$P_2(n) - P_2(n-1) = \xi\, (Q_2(n-1) - Q_2(n-2)) \tag{10}$$

This quite elementary nonlinear six-dimensional system of difference equations encapsulated Koopmans' and Frisch's argument about the nature of the evolution of a liberalistic economy.

Herod's judgement

In order to check the model, the values of the parameters and initial conditions are assumed as in Table 10.1.

Note that the system is not bounded and therefore the variables may have positive or negative values: the interpretation is that, under some circumstances, the agent does not sell and is forced to buy necessary inputs in order to survive, and that part of the market is perfectly exogenous. Stocks are assumed. Assuming as well that p_1 remains the same in $t-2$ and $n-1$ (0.09), the model generates large cycles at first and both *primus* and *secundus* eventually dominate the market for a short time, but then it stabilises with *primus* enjoying dominance (see Figures 10.1a and 10.1b).

Table 10.1 Initial conditions and values of the parameters

Initial conditions	*n–1*	*n–2*	*Parameters*	
S_1	0.21784	0.09	ê	1.1
S_2	0.10926	0.09	â	2
Q_1	2.1784	1	α	0.2
Q_2	1.214	1	β	0.4
P_1	0.1	0.09	γ	1.1
P_2	0.09	0.09	λ	3
			ξ	0.3
			ψ	0.2

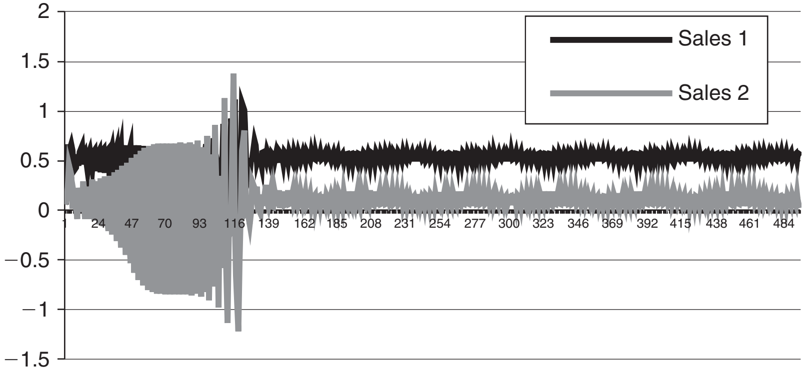

Figure 10.1a The behaviour of the model: the evolution of sales.

Note
All graphs for t=0,…, except if otherwise mentioned.

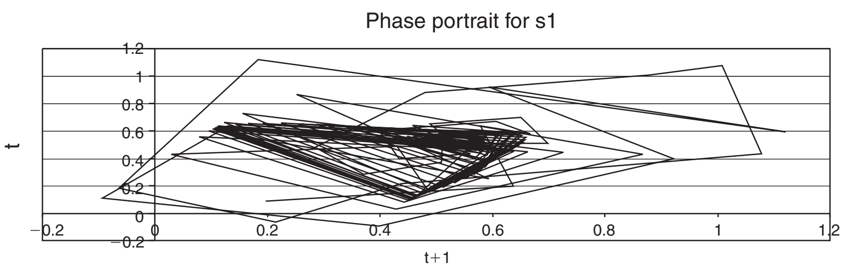

Figure 10.1b The behaviour of the model: phase portrait of the sales of *primus*.

Now, if the initial conditions are modified as shown in Table 10,1, with *primus* taking the initiative of increasing his price 11 per cent from t–2 to t–1, both agents will then tend to dominate the emerging behaviour in cycles, with alternating dominance (see Figure 10.2).

Let us suppose now that *primus* further refines his strategy and adopts an

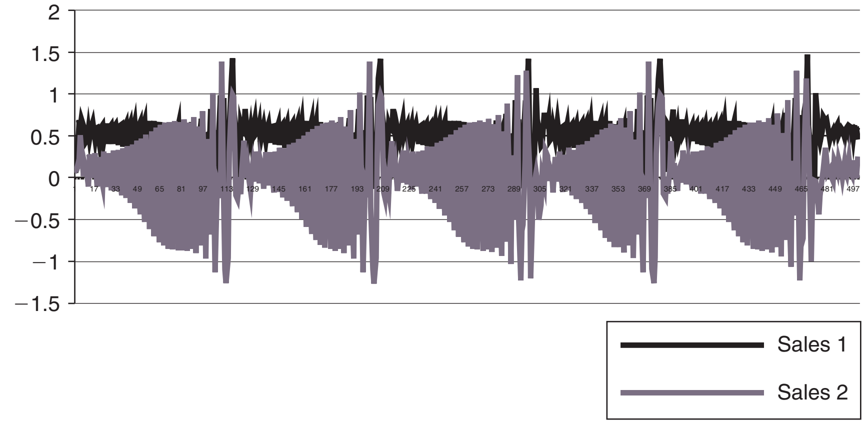

Figure 10.2 An aggressive intervention by *primus* at t–1.

inflationary policy, so that the parameter ψ (measuring the impact of the previous growth of sales on the subsequent rise in price) is slightly increased. Notice that the cycles in the sales of *secundus* are always larger than those of *primus*, given the chosen set of parameters and the actions taken in this story. Surprisingly, this results in the destruction of the structured market relationship, since after irregular but shorter cycles the system collapses[1] (after ψ>0.204455702) – see Figures 10.3a and 10.3b.

This behaviour evokes that of the 'Circulation Planning' model (equations 1–4), although we also have here a broader range of possibilities. The stabilisa-

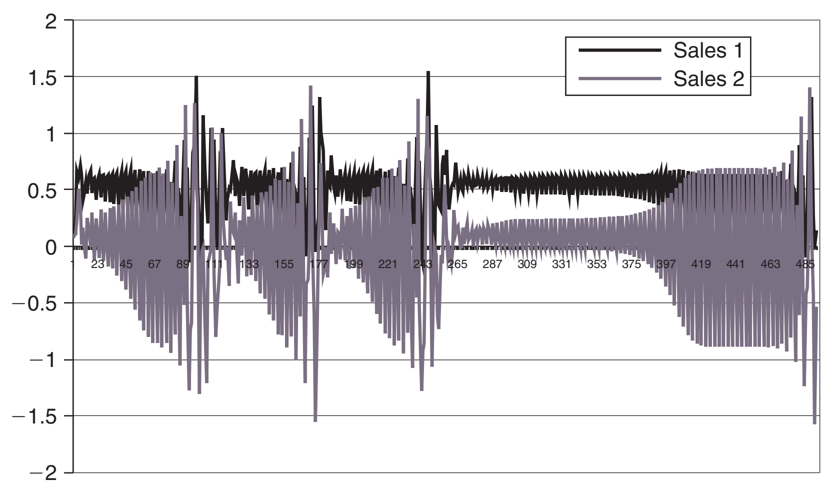

Figure 10.3a An irregular cyclical regime (ψ=0.202102994).

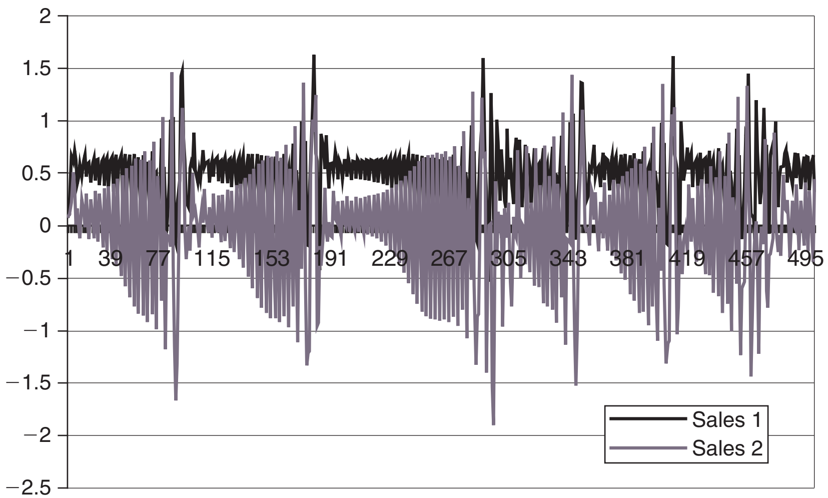

Figure 10.3b An irregular cyclical regime ($\psi = 0.204455702$).

tion of trade at a very low level is possible, although regimes of regular and then irregular cycles may also happen, and collapse follows if the same parameters are increased. The crucial difference is therefore that, in the 1934 model, collapse occurred because of scepticism about the possibilities of trade and here collapse is the consequence of aggressive action in the market. The substantial difference between the models is obviously the introduction of nonlinearities. This allows for different and richer patterns of behaviour: the increase of ψ establishes a route to chaos.

Suppose now that *secundus* reacts to the original change in initial conditions, taking a parallel inflationary measure, so that ξ is increased with $\psi = 0.2$. Notice that increases in ψ and ξ are the most accessible interventions by the agents in order to change their relative position, since they fix the prices whereas quantities are defined by the market conditions. *Secundus* gets a larger share of the market sometimes, but *primus* still dominates for most of the time. And, as ψ is increased, both parties are harmed, since large cycles and eventually a collapse of the market (after $\xi = 0.310531146$) are generated. Aggressive competitive strategies based on inflationary action lead to the destruction of the market (see Figures 10.4a and 10.4b).

Finally, under the initial conditions, it is supposed that there is an exogenous change in demand addressed to *primus*, and that γ (measuring the impact of the growth of sales of *secundus* in *primus*'s sales) increases slightly. In this case, although generally the sales of primus are superior to those of secundus, the reverse situation may occur for short periods. But, if γ is still increasing, the market will collapse after $\gamma = 1.1425530$. After that value of the parameter, collapse will follow.

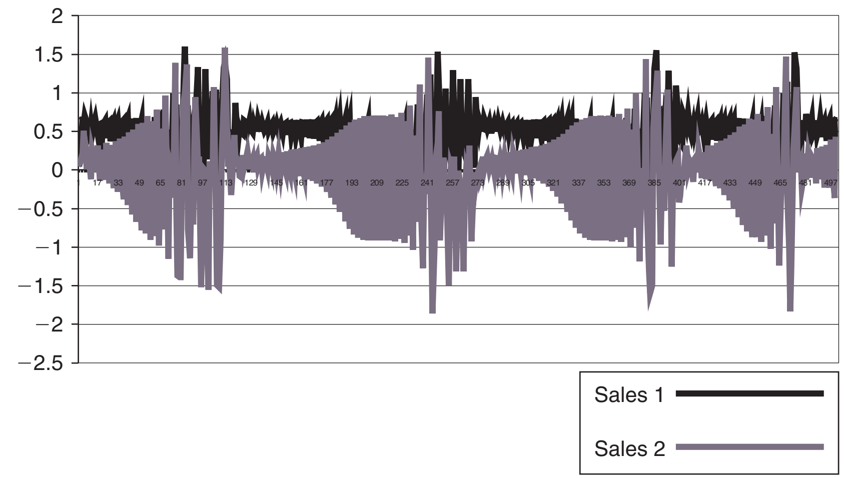

Figure 10.4a Inflationary regime provoked by the reaction of *secundus* ($\xi = 0.310531146$).

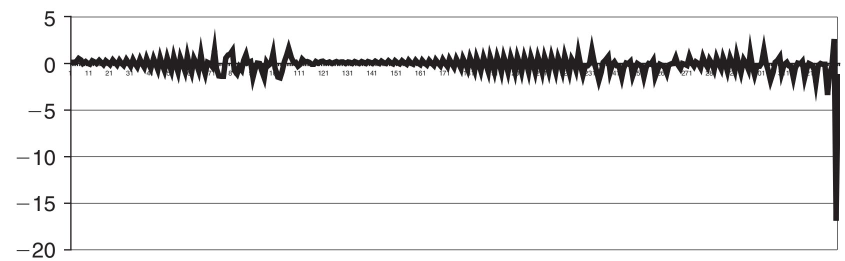

Figure 10.4b Inflationary regime provoked by the reaction of *secundus* ($\xi = 0.3105311467$).

Note
The system collapses after $n = 352$.

An extension: the virtue of coordination

This section provides a simple extension of the model, which was not considered by those involved in the discussion at Namur and afterwards. This extension proves that the model becomes much more robust if certain restrictions are introduced. In the case in question, the possibilities of chaotic outcomes, exploding oscillations and therefore collapse, still exist, but they are severely restricted in the phase space.

Let us consider that the space of the variables is restricted to positive values, meaning that the agents cannot assume debts in order to pay for the continuation of production, and simply that if they don't sell they don't produce, until a new demand is created. In that case, even an inflationary policy by *primus* and a response by *secundus* will allow for the continuation of trade. Figures 10.5 and 10.6 show such a situation.

With the increase of 11 per cent in the price of *primus*, which represents the same situation as before (see Figure 10.3a), a stable market is rapidly obtained under these new conditions. The same happens if *secundus* develops an aggressive response, similar to the one shown for the previous case.

In the previous case, collapse would occur after this value of the parameter is reached. Now, an aggressive policy can be pursued for much longer, generating several regular forms of irregular cycles with a clear dominance of *primus* or a hotly disputed situation.

In summary, in the previous case, an increase of 1.05 per cent and then a complementary increase of 1.17 per cent were studied, and it was proved that they led to collapse, under those initial conditions and with those values for the parameters. In the new context of the bounded version of the model – and here a specific restriction was chosen to simulate the system under a very simple form of rule, representing an institutional form of coordination – the same parameters can be increased 300 per cent before the collapse occurs. The range of possibilities is therefore much broader, and the model is more robust. This suggests, under all the provisos of modelling as a representation of complex societies, that

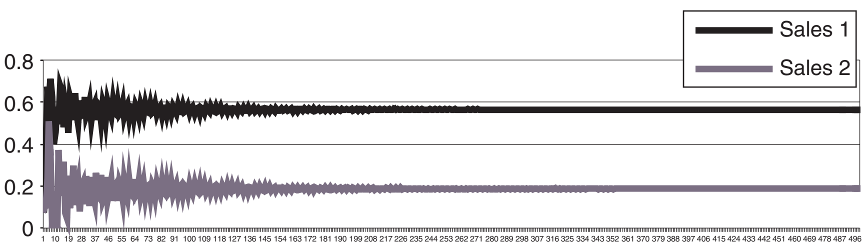

Figure 10.5 The virtue of coordination ($\psi = 0.202102994$).

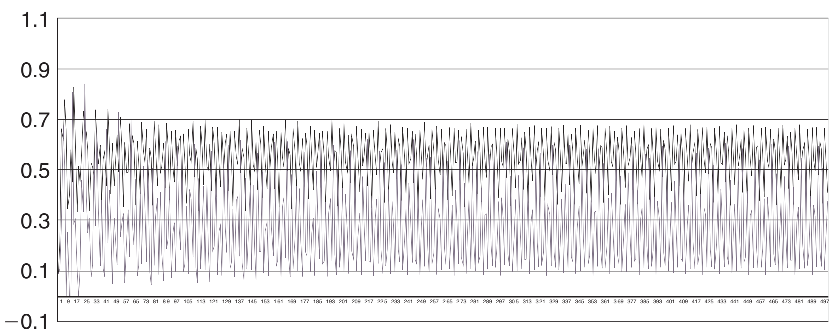

Figure 10.6 Coordination under threat ($\xi = 0.5$).

social forms of coordination may reduce the inherent instability of dense interaction in systems with a large number of different agents making judgements and taking strategic decisions.

It is quite obvious that evidence provides a shared judgement about Frisch's and Koopmans' argument. In the framework of the model – and this does not allow for any meaningful claim about reality itself – it is true that for some values of the parameters we may have a stable equilibrium in the market (Fig. 10.1), thereby vindicating Koopmans' opinion. But it is also true that, for other values of the parameters, cycles dominate (Figs 10.2 and 10.3); moreover, if the agents are profit-maximising, their action may eventually lead to the collapse of trade, thereby vindicating Frisch's opinion (e.g. Fig. 10.4). Still, collapse is brought about not by the lack of sales – not by contraction, as Frisch expected – but by the over severe oscillations that imply at some point the bankruptcy of one or both agents. Private vices are not compatible with public virtues, in the case of pure competition, but, in a managed economy, public virtue can impose itself through rules and other forms of coordination and consequently increase the fitness and chances of survival of the system.

Dangers of liberalism

For these and other reasons, this 1935 paper was very important for the history of econometrics. It provided the framework for an investigation about emerging behaviour in a very simple model, and namely of the conditions needed for equilibrium, for cycles, for chaos or for a catastrophe. And, last but not least, it also proves that Frisch, the apostle of linear, computable and parsimonious models, also dared to travel to the edge of chaos.

Contemporary correspondence also proves that Frisch was aware both of the nature of the technical and analytical difficulty and of the importance of the matter. At the same Namur conference, he met the young Pieter de Wolff, who presented a paper on linear equations and economic models and who was very interested in nonlinearities, and suggested the preparations of a common book on nonlinear difference equations. De Wolff was informed of the Frisch–Tinbergen–Koopmans discussion by both Frisch and his close friend Koopmans.[2]

In any case, Frisch was in a hurry, considering the theme as 'exceedingly important from the economic point of view', since he suspected these nonlinearities to be quite general in economics. He pressed De Wolff to take up the pen and begin 'our book' as soon as possible, even before the definition of its plan, since this could provide a solution to the Namur model:

> Even without having yet settled the question of the detailed programme of our book, you may of course start work, because there are certain parts of it which we know ought to be included and for which you would have to take the responsibility. I am thinking in particular of a chapter on integral equations and another chapter on non-linear difference and differential equations. Particularly the latter topic is exceedingly important from the

> economic point of view. The type of nonlinear equations that occurs most frequently is the one where product terms of pairs of variables (but not second power terms) occur, the product terms expressing the value; price times quantity of a certain commodity. Examples of this type of equations can easily be found. Tinbergen will be able to tell you about some of his problems leading to equations of the type considered. Enclosed you will find another example of the same sort, i.e. equations ensuing from problems discussed in Namur between Koopmans, Tinbergen and me. The line of attack of these equations would be to try to find out what can be said in general about the character of the solutions.
>
> (Frisch to Wolff, 15 October 1935)

A couple of months afterwards, Frisch sent a short note to De Wolff indicating that he had 'talked to a man' who suggested an idea for the solution of nonlinear differential equations, the recourse to iterative processes such as those used in the theory of atomic fluctuations.[3] Yet, the book was rapidly forgotten, and nothing came of it. De Wolff drew up a list of relevant items, but when, in 1939, he spent some weeks at the Institute in Oslo, Frisch did not mention the book anymore.[4]

Consequently, this intuition on the 'exceedingly important economic' problem was not developed any further. Two major reasons may have contributed to this. First, the participants at this debate did not have the means to study the behaviour of this system without indulging in an enormous amount of painstaking computation. Second, and perhaps this was the essential reason, Frisch and Tinbergen were deeply convinced that Koopmans' objections and the 'liberalistic' argument were wrong. Given their organisation, the market economies could not avoid or prevent major crises – and in the early 1930s there was quite a lot of evidence for that point of view – and therefore the Walrasian dreamland was just a figment of people's imagination. In his summary of the Leiden Conference (1933), Marschak expressed Frisch's denial that the 'profit maximization principle could be taken as a general principle governing the economic system' (*Econometrica*, 1(4): 192). This is how Tinbergen put it, even before Frisch prepared his short memorandum:

> In the first place the identification of the optimum situation with a Walras situation is, in my view, very questionable. Since it seems that Koopmans himself recognizes this it may be left out of the discussion. My main objection is, then, that the realization of the Walrasian situation is impossible when we have a permanently changing situation in addition to some elements that make absolutely impossible an immediate reaction of all variables to any change in data. Therefore, the Walrasian situation can only accidentally be realized. This is the reason why it seems better to discuss the desirability of a given stabilization policy without any connection with the Walrasian system.
>
> (Tinbergen, 1935: 308)

Considering Koopmans' future role as director of the Cowles Commission, this premonitory discussion proves to what extent the divergences among European econometricians affected their intellectual relationships. The consequence was straightforward for Frisch, as he had argued in his previous paper, 'Circulation Planning': economics should aim at producing the tools for economic intervention and monitoring. Economics, according to this view, ought to be political economy.

11 Prometheus tired of war

Econometrics emerged as a specific research programme in the 1930s. Those were terrible years, and the econometricians were fully aware of the tensions, dangers and desperation in the air. After the Great Depression, the economic collapse in the US and the social crises in Europe, war became a real and frightening possibility.

This chapter argues that some – if not most – of the early European econometricians made intense efforts to address the social responsibility of scientists in the midst of this turmoil. The intellectual atmosphere of that time favoured radical options: that was the case of many of the creators of the Vienna Circle, who equated logical empiricism with the promotion of social reform, such as Hans Hahn, Otto Neurath and the young Karl Menger, who would be one of the founders of the Econometric Society (Leonard, 1998: 5, 7). Furthermore, at least some of them consistently maintained their youthful attitude for the rest of their lives. This at least was the case with Ragnar Frisch and Jan Tinbergen: the founding years marked their assessment of economics, econometrics and the role of social science, and determined their estrangement from econometrics as structural estimation progressed and then as the axiomatic revolution of the late 1940s and 1950s triumphed. Some of the European econometricians followed a different path, such as Marschak and Koopmans, and the diversity of their strategies was related both to scientific and political circumstances and choices, including their visions of what science was about.

It must be added that many of the most influential economists were indeed deeply concerned by the social problems they were discussing, in not only theoretical, but also direct personal and political terms: unemployment was seen as the outcome of the structural imbalances, inequalities and injustices of capitalist society, and it was the urgent duty of economists as both scientists and citizens to try to correct these deficiencies. Consequently, the political and social values of each econometrician of that period are important for understanding their scientific careers, not only because these immediately interfered with their work, but also because they helped to build the framework for the selection of problems and answers.

Political economy under question

With the important exceptions of Schumpeter, Amoroso, Schultz, von Neumann and others, namely many at the NBER, a large number of the major actors in this drama were sympathetic to, or even in a few cases directly politically involved in, socialist or social-democratic movements and therefore inclined to accept the dominant view of an activist political economy. In fact, the end of the nineteenth century and the early twentieth century were radical years: changes in civilisation, wars and conflicts, the hurricane of progress and change challenged the scientists as other citizens. Many scientists, from different fields, were attracted to the ideas and ideals of that time.

J.B. Clark and Karl Pearson, the European connection

J.B. Clark, who had studied in Europe and became one of the founders of marginalism in the USA, was a moderate socialist: he taught a course on socialism, supported the creation of trade unions and did not hide his mistrust of markets.

Karl Pearson's life is a curious example of that same story. He adhered from the late 1880s to socialism, which should eject the 'endowed idlers' – this was the reason for his little respect for economics, charged of accepting the status quo (Porter, 2004: 69, 158). Educated in Germany, Pearson shared a house in Berlin with Frank Taussig, who was probably instrumental in his conversion to socialism – we met Taussig in the 1920s in the US, as a candidate to the Econometric Society (Chapter 2), at a time when he had become a supporter of free trade and a conservative economist, battling against trade unions and the legislation for a minimum wage. Although his socialism was inscribed in the Romantic tradition of Carlyle, Ruskin, Morris and his friend Bernard Shaw, Karl Pearson was fascinated by Marx and decided to translate *Das Kapital* into English. He wrote to the author on 9 February 1881, for that purpose, stating his 'firm belief in the soundness of the fundamental doctrines of Socialism'. Probably for doubts on the ability of the young Pearson to provide an adequate translation, Marx rejected the offer (ibid.: 74, 76–7). This was not enough to discourage Pearson, who proposed two years later a paper on the Marxian labour value theory to the British Association conference. But, as he was given just half an hour to present it, Pearson withdrew the paper, motivating Neville Keynes to complain: 'I should have been particularly interested in hearing you demolish us poor economists' (ibid.: 78). Faithful to the radical ideas of his youth, Pearson rejected both the distinction of the Order of the British Empire in 1920 and a knighthood in 1935.

Much later on, in 1934, Karl Pearson took his almost religious belief in eugenics to the extreme of praising Hitler: misunderstanding is a privilege of old age and Pearson died in 1936, without knowing the full extent of Nazi policy.[1]

Keynes, the liberal exception

Keynes himself was perhaps an exception in this group, since he was politically closer to liberalism, in the specific configuration of British politics: he was a centre-left supporter of the Liberal Party in the United Kingdom, an opponent of the Tories and vaguely sympathetic to Labour. In 1923, in his first editorial of the *Nation*, Keynes presented his politics: 'Our own sympathies are for a Liberal Party with its centre to the left' (quoted in Skidelsky, 1992: 136–7); at that time, he suggested the formation of a Labour–Liberal government. He supported the miners during the General Strike of 1926, a deep social conflict that led to a split in the leadership of the Liberal Party and the alienation of Keynes's own traditional political relations inside that Party (ibid.: 223). In 1931, he was more inclined to support Labour than Liberal candidates; only with the outbreak of the world war did he return to the Liberal tradition (Moggridge, 1992: 465). But it must be emphasised that his public position was largely consensual: in 1937, all three major parties offered Keynes the nomination as an independent candidate for the constituency of Cambridge, which he eventually refused after much hesitation (ibid.: 628).

As the most prestigious economist in Britain, Keynes had a say in all major political decisions, from the Great Depression to the end of the world war. Liberal in politics, he was nevertheless an active supporter or even the creator of the modern theory of public intervention through monetary and fiscal policy. In this particular regard, he was followed with great reverence by the other economists of his time.

The other members of the Econometric Society did not enjoy such an influence: the only ones who could claim a reputation, Irving Fisher and Joseph Schumpeter, were not at the apex of their career in the 1930s. Instead, the youngsters, Frisch, Tinbergen, Marschak, Lange, Neyman, Meade, Hotelling, Olav Reiersol and Lawrence Klein,[2] who were then starting out on their academic careers, were strongly motivated by the opposition to war and the world crisis and certainly leaned to the left, a fact that had a definite influence on their agenda.

Ragnar Frisch, against 'unenlightened financialism'

Ragnar Frisch was politically active most of his life: at first, his sympathies were for the Liberal Party, then for the right wing and very soon for the left wing of the Labour Party and beyond it. In the autumn of 1933 – a very full year for Frisch, then thirty-eight years old – he was one of the drafters of the party's electoral platform, a three-year programme designed to deal with the social crisis. Frisch opposed the idea of creating credit in order to fuel expansion, since he thought this to be insufficient and even non-Keynesian. When the Labour Party won the 1935 elections, he became a member of the Parliament's Monetary Committee.

The party dominated Norwegian politics from 1945 to 1961, with a large majority, and Frisch, although given no official position in the government, enjoyed an influence for quite some time on the country's major economic choices. In 1946 he was a member of two parliamentary committees, those dealing with foreign exchange and finance. But during the 1940s his relations with the Labour Party began to deteriorate, since he was convinced that it was following the wrong path in accepting the rules of the 'monetary plutocracy'. In 1955, this divergence led to a rupture, as his proposals for strict credit control were not accepted.[3] Frisch did not participate any more in high-level cooperation with the Norwegian government and indeed his opinion ceased to be considered in decision-making circles.

His most important divergence was over the government's decision in favour of joining the EEC, which Frisch abhorred: this would mean the triumph of 'unenlightened financialism'.[4] Tinbergen argued against his friend, in the 1970s and praised the participation of the Netherlands in the EEC (Tinbergen, 1974: 5–6). But Haavelmo took sides with Frisch in the Norwegian referendum (September 1972) and voiced his opposition once again when the referendum was repeated in 1994 in order to decide upon participation in the EU.

When he retired in 1965, Frisch continued his work on decision models and preference functions as the basis for planning. These ideas were put into practice in India and the United Arab Republic in the 1950s and 1960s: he stayed in India for a few months (autumn 1954 to spring 1955, with the Statistical Institute in Calcutta), but travelled more frequently to the United Arab Republic (1957–8, 1958–9, 1959–60 and later in the 1960s). In the late 1960s, he publicly opposed the Vietnam War and 'wholeheartedly' signed a call by Bernal for general disarmament,[5] to be presented to the World Congress for Peace. During the same period, he frequently intervened on international matters, endorsing the fight against dictatorships (Greece, Spain), colonisation (Guinea-Bissau) and apartheid, or supporting civil rights (Martin Luther King).

Jan Tinbergen and Tjalling Koopmans, the European debate on planning

Jan Tinbergen,[6] who studied physics at the University of Leiden, decided for political reasons to become an economist, since he wanted to prepare new solutions to the social crisis of the 1930s: 'My interest in economics was not primarily scientific, it was typically social' (Tinbergen, 1987: 119). The high school friend and then wife, Tine de Wit, influenced Tinbergen's conversion to socialism and he joined the Dutch Social Democratic Party in 1922, then nineteen years old, and studied Hilferding's writings and other Marxist authors (Jolink, 2003: 15, 21, 36). His political attitude involuntarily helped him to come into economics: as he refused to serve in the army given the tragic experience of the First World War, Tinbergen was forced to work in the civil service as a form of compensation. After Tinbergen had worked in the administration of a prison, his father – without his knowledge – obtained his transfer to the Dutch Central Bureau of Statistics, where he completed his period of civil service in 1927–8 (ibid.: 18). Ehrenfest, his supervisor, instigated Tinbergen to study economics, as he explained to Wicksell, in order to defend the need for an economic plan:[7] indeed, Tinbergen later used his skills to help in the preparation of the 'alternative policy package' of a 'Labour Plan' in 1936 (ibid.: 122).

From then on, his main topic of research was statistics and he voluntarily adhered to the econometric movement: his major contribution to the study of business cycles was discussed in Chapter 7. Yet, after the Second World War, Tinbergen followed Frisch in devoting his career to statistics, planning and development economics, taking up the position of director of the recently formed Central Planning Bureau (Hallet, 1989: 192). Epstein notes that 'Tinbergen led a movement after the Second World War that turned instead to the study of centralized economic planning that placed minimal reliance on advanced statistical techniques' and 'Tinbergen saw econometrics as the tool that would make possible effective intervention in the economy to carry out a Socialist program' (Epstein, 1987: 9). So did Frisch and Haavelmo (ibid.: 127).

Koopmans followed the example and guidance of Tinbergen, for a parallel reason:

> Why did I leave physics at the end of 1933? In the depth of the worldwide economic depression I felt that the physical sciences were far ahead of the social and economic sciences. . . . Then I learned from a friend that there was a field called mathematical economics, and that Jan Tinbergen, a former student of Paul Ehrenfest, had left physics to devote himself to economics. Tinbergen received me cordially and guided me into the field in his own inimitable way.
>
> (Koopmans, 1979)[8]

In his Nobel autobiography, Koopmans refers to his early 'explorations of Marxist thinking in my student years' (Kopmans, 1992).

Later on, in the late 1940s and in spite of the differences with both Frisch and Tinbergen on laissez-faire, as witnessed by the Namur and the Vatican debates (see Chapter 10 and the next sections), Koopmans developed in linear programming, which he insisted calling 'activity analysis', a parallel career to that of general equilibrium economist. He concentrated on the 'transportation problem', an old favourite of his, and considered activity analysis to be a necessary tool for economic planning, although he grew politically more conservative through his life (Mirowski, 2002: 287). Koopmans' interest in programming motivated extended visits to the USSR in 1965 and 1970, where he met Kantorovich, with whom he shared the Nobel Prize in 1975.

Jacob Marschak and Joseph Schumpeter, from Central Europe to the US

Marschak's history is more dramatic. Born in Russia, he was imprisoned for his Menshevik[9] activities against the Czar and after the 1917 revolution he was a Ukrainian official just to become the twenty years old minister of labour of the short-lived Menshevik-Cossack Republic of Terek in the North Caucasus. As this government was defeated, Marschak emigrated to Berlin in 1919 where he published a virulent attack on the Austrians on the socialist calculation debate and produced several contributions to Marxian economics. His socialist beliefs

forced him to abandon Germany with the rise of Nazism and move to Britain and then to New York, where he got a job at the New School for Social Research.

The other econometricians knew his views, and these gave rise to a long exchange between Frisch and Schumpeter over the choice of Fellows: Frisch criticised Schumpeter's opposition to Marschak for apparently non-scientific reasons, namely because of his anti-Semitism or anti-socialist motivations; Schumpeter answered that he did not object to Marschak being a Jew and a socialist, but that he was both at the same time and rather militant, and that he could eventually bring others to the Society.[10]

Four years later, Schumpeter used the same argument against the selection of Marschak as a research director for the Cowles Commission: Marschak is a

> highly competent and highly trained economist and statistician. . . . Of course, he is a Jew and a socialist and it is more than likely that he will try to draw other Jews and socialists after him. But the weight to be attached to considerations of this kind it is beyond my competence to pronounce upon. In the normal course of things I do not apprehend any difficulty on that score.[11]

It remains nevertheless obvious that the crucial point was his organised political activity rather than any other motive: instead, Schumpeter recommended Lange, who was also a socialist and whose wife was a Jew (which would mean trouble if returning to Europe, Schumpeter rightly argued).[12]

The dangers and difficulties that Marschak and his colleagues lived through mobilised many fellow econometricians. Frisch interceded with Bowley in order to get him a job in Britain.[13] After the confrontation with his friend Frisch over Marschak, Schumpeter, who was trying to set up a commission in the US to help German economists under threat, sent Mitchell a list of Jews in need of assistance and highly recommended Marschak, 'probably the most gifted scientific economist of the exact quantitative type now in Germany'.[14] He also wrote to the Rockefeller Foundation, asking for help for Marschak.[15]

Schumpeter stood out among this generation of younger econometricians, since he had had a previous failed political career, as Minister of Finance in the Republican government of Karl Renner from March to October 1919, as well as a career as a banker in Austria, which had resulted in bankruptcy. But what most distinguished him were his radical views: even when he left Germany for the US, he was not opposed to Nazism – although, of course, unaware of what was to happen later on. Schumpeter showed great ambivalence in relation to the dramatic events going on: 'I am often in a state akin to despair', he wrote to Frisch two days before the burning of the Reischtag.[16] As Hitler tightened his grip on power, he still stated: 'As to Germany, I find it very difficult to form an opinion. Recent events may mean a catastrophe but they also may mean salvation',[17] and 'I know something of the government which preceded Hitler's and I can only say that I am quite prepared to forgive him much by virtue of comparison.'[18]

In fact, even if he generously supported colleagues persecuted by the German government, he still refused to condemn it. Schumpeter, who lived in Germany until 1932, even advised some of his students to join the Nazi party and, in his farewell speech to the University of Bonn, included the following remark: 'What enormous subjective individual possibilities there might be for a young man of today if there were any who, not deprecating economic techniques, felt like a National Socialist', and defined Nazism as 'a powerful movement which is singular in our history' (quoted in Allen, 1991, I: 284–5).[19] Later, in his personal diary, Schumpeter asked himself why he had changed his attitude towards Germany from the First to the Second World War: 'I cannot understand at all this *revirement* of my sympathies [for Germany] since 1916' (in Allen, 1991, II: 139).[20] His diary includes many remarks supporting Hitler (in Allen, 1991, I: 288, II: 12–13, 58, 71, 92, 103).

In spite of this *revirement*, Marx was chosen by Schumpeter as his intellectual alter ego, and socialism was deemed inevitable: his 1942 book *Capitalism, Socialism and Democracy* also indicates some afterthoughts on the traditional political divide, since he feared that the inability of capitalism to innovate and renovate could pave the way for the victory of socialism. When Schumpeter died in 1950, a manuscript was found on his desk, including notes for lectures, under the title 'The March to Socialism', presenting his sombre view: 'the capitalist order tends to destroy itself and that centralist socialism is . . . a likely heir' (Schumpeter, 1950: 4).

Jerzy Neyman and Oskar Lange, from Poland to California

Jerzy Neyman was born in Poland and was educated in Ukraine. The Neymans suffered the perturbations of the fall of the Empire and in February 1918, when the Ukrainian nationalist government set a separate peace with Germany, the country was invaded by Red Army but the Germans sent rescue troops – which were cherished by the population, including his family, as Neyman recalled (Reid, 1998: 29).

Forced by the upheaval in the 1930s, Neyman emigrated to Britain and then to the US, and was actively involved in political campaigns. This manifested itself very early on, when he wrote a violent letter to Frisch, the editor of *Econometrica*, protesting against the inclusion of a paper by Amoroso on Pareto, which he accused of justifying Mussolini's rule in Italy – Pareto had been nominated Senator by the fascist regime. Frisch was surprised and defended the paper as a mere 'description of Pareto's work'.[21] In the summer of 1964, they were again involved in a political question, but, on this occasion, they were in agreement: Neyman was actively engaged in a campaign to give Martin Luther King the Nobel Prize for Peace, and asked for support. Frisch accepted the idea, but preferred a Kennedy–Khrushchev ticket, which did not enthuse Neyman – in fact, none of them got the Prize. Later, Neyman distinguished again as a supporter of the anti-Vietnam war movement (ibid.: 277).

Oskar Lange was also born in Poland, but from 1935 established at the University of Michigan, and then moved to Chicago by 1943. He always advocated

forms of market socialism, justified with recourse to a Walrasian and Paretian scheme used to prove the existence of a socially efficient optimum obtained by planning. Lange was chosen by the new after-war Polish government as its ambassador to the US (1945) and then to the United Nations (1946–9). He then returned to Poland, taking minor academic jobs, to be chosen after a period of obscurity to the distinguished role of chairman of the Polish State Economic Council.

The socialist calculation debate comes to Chicago

The socialist calculation debate was reviewed elsewhere (for a recent and challenging appraisal, Mirowski, 2002) and is not discussed in detail in this chapter. Nevertheless, it is useful to highlight its importance, given both the participants and the themes. The debate was first ignited by a 1908 paper by Enrico Barone, who argued drawing on Pareto that if prices are the solution of a set of equations in a Walrasian system, then either the government or market can achieve the same solution. Otto Neurath presented later a radical version of this argument, stating that the non-profit-seeking war policies proved to be more efficient than liberal alternatives, and was challenged by Ludwig von Mises in 1920. According to Mises, Neurath's socialist solution was deficient, since the public ownership of the means of production under a socialist regime implied the lack of prices for capital goods, and therefore the computation of prices could not be obtained. A cohort of Marxist Paretians, including Oskar Lange and Jacob Marschak, intervened from that stage of the debate: indeed, Marschak produced his first paper in economics on the socialist calculation debate in 1923.

Yet, it was with Hayek that a sophisticated liberal argument was elaborated, based on the information requirements for efficient computation. When Lange came to Cowles and Chicago, he fully endorsed a socialist interpretation of the Walrasian–Paretian framework, which was a further reason for Knight's hostility. The simultaneous equations estimation and the general equilibrium framework for the Cowles Commission was tributary to the socialist debate and the research directors were chosen among that lineage. That was both the case of Marschak and that of Koopmans, who presented the simultaneous equations approach as designed for an 'econometrically guided policy' (Mirowski, 2002: 245).

Pre-war concerns, considerations and failures

The Great Depression of 1929 became the central economic problem for the young generation of mathematically inclined economists, just as it was for Keynes. In the draft for a speech planned for the autumn of 1931, Frisch presented his great motivation: 'The depression is a sum of unhappiness and misery, and that is why something has to be done in order to stop this crazy and undignified dance that is the business cycle in a modern capitalist society' (quoted in Andvig, 1986: 299). In the same period, he offered an explanation of the current crisis in an article published in *Tidens Tegn*, a Norwegian conservative newspaper, under the telling title of 'Plan or chaos' (5 November 1931):

> One has to understand that the ongoing crisis is not a crisis of real poverty, but an organizational crisis. The world is like a ship loaded by the goods of life, where the crew starves because it cannot find out how the goods should be distributed. Since the depression is not a real poverty crisis, but one of organization, the remedy should also be sought through effective organizational work inside the apparatus of production and distribution. The great defect of the private capitalist system of production as it is today is its lack of planning, that is, planning at the social level. This cardinal point cannot be disputed.
>
> (quoted in Andvig, 1986: 87)

Frisch immediately added that the alternative to 'communism' was to 'let the government manipulate certain links in the machinery, such as the monetary and credit policies, trust policies, trade policies, and so on, with the conscious aim of indirectly steering the economy out of the existing chaos and into a situation guided by a definite social plan' (ibid.) – the alternative was a mild form of planning.

This text was not ignored. Hansen, who could read Norwegian, protested about the contents of the paper and argued that the crisis was not provoked by social organisation but by a lack of adaptation to change, and consequently that planning was at odds with such adaptation.[22] Frisch reacted prudently:

> Thank you for your letter of December 12 with your judicious remarks about planning and planlessness. Of course a planned economy has its tremendous difficulties and would also need adjustments to new situations just like the old economy based on private initiative, but I must admit that I, for my part, believe that some developments in this direction are nevertheless necessary and that they will come in one form or another.[23]

The Crisis Plan he proposed as a platform for the Labour Party in the following year was an encapsulation of this approach, favouring indirect steering mechanisms such as monetary and fiscal policy instruments in the Keynesian style, but also admitting they should conform to a plan. Frisch was to abandon this view over the following years, with a growing hostility towards the indirect approach and instead favouring direct control and planning, although always emphasising the democratic foundations of such a strategy. This could be achieved by the estimation of preference functions, namely through interviews, and the public debate and selection between the alternatives.

This attitude was not unpopular at the time. Indeed, the common ground for the US and European founders of econometrics was the definition of their mission as the prevention of unemployment and other socially damaging consequences of business cycles and their 'undignified dance'. There is also vast

evidence to indicate that they considered the programme to be feasible. A pamphlet – 'Econometrics: Towards Making Economics a More Exact Science' – published by the Cowles Commission in 1932, which was the first public presentation of the project, bluntly argued that this was indeed more important than the creation of the telephone and radio and as viable as these inventions:

> The objectives of the mathematical researchers now being undertaken by members of the Econometric Society are vastly more important than the radio or the telephone. Certainly, an appreciation of the practical value in understanding the causes of the business cycle has of late been amply driven home. And it is altogether possible that, through *econometrics*, a solution of this problem, which has baffled the world, thrown millions out of world, and caused untold distress, may ultimately be reached. It is the purpose of those who are interested in developing econometrics to turn to the powerful weapons of mathematical analysis on the business cycle, indeed on every aspect of our economic life, in an effort to establish or demolish existing theories, and to discover new economic truths.
>
> (Cowles Commission, 1932: 7)

From this point of view, the purpose of the Cowles Commission was to predict changes and impacts of policies, to understand these 'new economic truths' and to generate new alternatives. These purposes were generally shared by the econometric milieu. And for at least some of them, the effort to tame the cycle and to prevent unemployment was extendable to the attempt to address the general problems caused by the international drive towards war. This was of course mostly a European trend: anyway, the Europeans were closer to the war scenario than the Americans and could easily anticipate their involvement in it.

Tinbergen, for one, was very impressed with the call made in October 1935 by 350 psychologists in favour of peace. On 20 March 1936, he wrote to Frisch in order to suggest a new call against war, this time by the Econometric Society as such. According to Tinbergen, it should follow the line of declaring that the 'econometricians feel it as a first duty to raise their voices against the tendencies leading to the largest wholesale destruction of human welfare, the war'. In that sense, the econometricians should offer their services to all governments and international institutions, providing neutral scientific procedures for the division of world supplies and consequently preventing the reasons for war. It goes without saying that Frisch wisely considered the initiative to be doomed from the outset, and convinced his colleague to leave the Society out of that mess.[24] Yet he fully agreed with Tinbergen's views and both of them shared the same concern about the war.

Their final choice, the rejection of any involvement of the Society in this debate, did not imply that either of them considered themselves not to be worried about the dangers of war. On the contrary, they thought that formal rigour, leading to mathematically treatable models allowing for concrete predictions and sound policy advice, was the necessary condition for preventing social tensions, unfairness, despair, conflict and eventually war. Tinbergen's work for the League of Nations on the comparison of the models of business cycles reviewed by Haberler, which was at the origin of the famous debate with Keynes, is one of these contributions. Indeed, Frisch and Tinbergen were close scientific and political allies throughout their lives: they shared the same preference function.

Frisch strongly suggested, in 1934, that the root of the social problems was the very organisation of the market economy, since it lay behind numerous independent decisions taken by too many interdependent agents. The lack of coordination could lead to the collapse of the system – and he thought that this had in fact been the case since 1929.[25] His 1934 paper, 'Circulation Planning', argued for a new system of multilateral barter trade, as an experimental method for preventing sectoral or regional mismatches, under the rule of a multilateral clearing agency. By that time, Frisch was already deeply convinced that the Keynesian indirect steering mechanisms were inappropriate.[26] Furthermore, believing that the financial and banking system was highly responsible for the economic crises, he naively favoured non-monetary exchanges among agents. The opening phrase of the paper recapitulates the argument on the damaging economic organisation:

> The most striking paradox of great depressions, and particularly of the present one, is the fact that poverty is imposed on us in the midst of a world of plenty. Many kinds of goods are actually present in large quantities, and other kinds could without any difficulty be brought forth in abundance, if only the available enormous productive power was let loose. Yet, in spite of this technical and physical abundance, most of us are forced to cut down consumption. . . . A full recognition of the monstrosity of this situation is the first and basic condition for any intelligent discussion of ways and means to get out of the depression. Of course this implies the conclusion that the cause of great depressions, such as the one we are actually in, is in some way or another connected with the present form of organization of industry and trade. The depression is not a real poverty crisis, not due to an actual shortage of real values. This must be admitted by everybody, regardless of political color.
>
> (Frisch, 1934a: 259)

The organisation of the economy producing these disastrous effects is based on 'what might be called the *encapsulating phenomenon*' – simply the interference of interactions among agents. In order to model this phenomenon, Frisch simulates the exchange between a shoemaker and a farmer, each buying and spending a proportional amount to what they received in the last period (ibid.: 261f.). This very simple rule of behaviour produces different possible scenarios, one being the lack of effective demand and the collapse of trade – a crisis of coordination due to a lack of information.

In that case, Frisch suggested the intervention of an 'organizer' interviewing both agents in order to determine their orders, and to restore the market –

in other words, circulation planning (ibid.: 272). He accepted that this was easier to develop at a national level – and 'experience has amply proved that very little can at present be obtained by international agreement' – although a '*complete* solution would necessitate international co-operation' (ibid.: 260). This was the task of science, since 'these problems [planning in order to solve crises] are essentially *econometric*' (ibid.: 261).[27]

Yet, in spite of the vigour of the argument, the paper did not impress the political decision makers or even Frisch's academic colleagues. The article was just noted for its length – it is still the longest paper ever published in *Econometrica* – and was generally ignored by the econometricians. Roos, the secretary of the association, wrote to Frisch, who was himself the editor of the journal, protesting about the inclusion of such a long paper in *Econometrica* and stating that it is not 'a very vital contribution to economic knowledge'.[28] And as was shown by the next year's debate between Frisch, Tinbergen and Koopmans at the Namur conference, there was widespread scepticism about this line of research.

But Frisch nonetheless wanted to pursue it. In a letter to Cowles, he suggested a research project on 'economic control' applied to Norway and then presented a memorandum to the Commission on this very topic.[29] The outline of the research project suggested recourse to a simultaneous equations system, given that all variables influence each other, as well as the use of experiments and inquiries in order to define and compare the preferences. But, as Andvig noted, there was a mounting contradiction between these two forms of assessment: by the late 1930s, Frisch was more and more sceptical about the role and use of highly aggregated macrodynamic models and increasingly favoured detailed direct steering mechanisms and planning. But, concentrating as he was on the definition of social preferences from micro data, he never developed the macro side of his policy alternative (Andvig, 1986: 285). Furthermore, his vision of the crisis as the property arising from the non-coordination of agents' decisions favoured this mismatch between the micro and macro levels of analysis and intervention, as he rejected the primacy of the 'profit maximization principle [that cannot be] taken as a general principle governing the economic system' (*Econometrica* 2(2): 192).

Barter trade, an immediate solution

In spite of the doubts of his colleagues, this particular concept was not thought of as a contradiction by this founder of econometrics. This is why the 'circulation planning' project re-emerged much later when, in July 1962, Joan Robinson joined forces with Ragnar Frisch to present a 'Draft of a Multilateral Trade Clearing Agency' to a London conference. As the next section proves, there is widespread evidence to show that they considered the contradictions of capitalism and the market form of social organisation to be responsible for havoc, war and deprivation. For this reason, a new international organisation based on cooperation and fair trade was proposed as a viable alternative – as viable as the invention of the telephone, or indeed as the control of the business cycle had been considered by the creators of the Cowles Commission thirty years earlier.

Immediately after finishing 'Circulation Planning', Frisch prepared a short 'Memorandum on the Organising of a Commodity and Service Exchange', which presumably circulated only around the Institute at Oslo University (26 January 1935). The memorandum refers to an attempt to set up a direct commodity exchange 'without the use of money or with a minimum of such use', at Zurich in 1934, and argues that there are two substantial motives for 'strangulation' in a depression: the lack of credit for investment and underconsumption. Consequently, Frisch submitted that under such circumstances the creation of 'clearing authorities', with the power to fix interest rates and to direct economic activities, was the only alternative to depression.

This idea would re-emerge from time to time. At a conference at Columbia University after the war (16 January 1947), Frisch called upon mathematicians to lend their support, in order to gain some assistance in the programming problem of adjusting supply and demand to prevent future excess supply and maladjustment in international trade, of the type that had led to the previous war. 'For a successful application of a general policy of full employment and economic expansion, we are thus brought down to the problem of achieving a reasonable degree of consistency between demand and supply amongst countries' (Frisch, 1947: 3).

Although the mathematical solution was not obtained, Frisch considered that there was a preliminary problem: the definition of the institution and procedures capable of proceeding to the adjustment process. In 1962, he presented a 'Tentative Draft of a Multilateral Trade Clearing Agency' as an address at the British-Commonwealth-EFTA Conference in London, organised by a movement opposed to membership of the Common Market. 'It is not our business to find an alternative to something that is so shortsighted, so narrowminded and so muddleheaded as the Common Market', began Frisch. 'The basic flaw . . . springs from the fact that whether we like it or not there is in the world today a very profound *interdependence* between the nations', a complex economic interdependence requiring a 'technical apparatus of coordination', which the GATT, IMF and OECD were unable to provide (Frisch, 1962b: 3–4). This new institution was the multilateral trade clearing agency, whose creation he defended.

Such an agency, bringing together national states with a considerable degree of sovereignty, was to follow nine principles. First, each country would formulate its own targets and, second, given the inconsistency of the targets, the agency – with the use of 'electronic computers' – would redefine a new set of national targets as close to the original ones as possible. Third, the national states would accept this process of (fourth) targets defined in volumes and (fifth) for short periods. Sixth, the trade balance would only be measured in the long run, (seventh) equating the volume targets and, eighth, using exchange rates as mere means of calculation. Ninth and finally, the initial trade would be extended to a larger set of goods after the first experience.

Joan Robinson endorsed this proposal. In order to avoid what could be a negative sum game among national independent policies with balance of payment difficulties, the recourse to all possible intermediation was justifiable, particularly that of mathematics: 'The moment has come when it is absolutely necessary for us to take a positive policy and in this the matrix algebra will be quite a useful adjunct to solving the problem in a rational way' (Robinson, 1962a).

Frisch took note of this incentive and again insisted with his proposal: in 1963, he prepared a new memorandum on the creation of a 'A Multilateral Trade Clearing Agency', to be submitted to a conference that the United Nations was preparing. He criticised the trade tariff wars going on, instead of the convivial alternative of a new international institution managing an automatic system of balancing between countries. Recapitulating the 1962 principles with greater technical detail, Frisch added that some work on the computing demands of the project had been going on at the Oslo Institute and that Myrdal had helped on the 'more practical aspects'.

The project was, of course, as naive as it was well intentioned, and it had no impact on the international institutions or, to be fair, on the economic profession either.

After the war: turning to a new generation of decision models

When the Econometric Society re-established its transoceanic operations after the end of the war, some pressed for its immediate intervention in relation to the pressing agenda of the day. Frisch published an editorial in *Econometrica* explaining why the 'social responsibility' of the mathematical economists would help to solve the problems of misery and unemployment, using their knowledge to prevent the causes of wars and the misfortune of the population. But, by then, he was more isolated than before, and some changes made to the agenda of the meetings suggest that the topic did not matter that much to his fellow econometricians.[30]

By then, the sense of disillusion had most definitely worsened. The devastation of the world war had confirmed for Frisch the structural characteristics of the disorganised market with too many independent decision makers and too much competition leading to catastrophe, but at the same time his proposals for new modes of trade were ignored. 'Circulation Planning' was proposed after the Crisis Plan of 1933, but the leaders of the Labour Party could not accept his strategy for a system of national multilateral barter trade. According to Andvig, this isolation determined the next step in his career: 'The experience which would turn him into the path of macroeconomic decision models was probably initially of a political nature' (Andvig, 1988: 497).

In any case, the objections against the indirect steering mechanisms had been firmly established in previous contributions by Frisch, since he believed neither in stable behavioural mechanism nor in the available instruments for economic policy – those were the arguments that he had put forward against Tinbergen's *magnum opus* in 1938. Decision models appeared as a suitable alternative, since they did not assume structural stability and, on the contrary, their purpose was to study the effects of shifts in the preference functions.[31] At that time and since his 1928–9 lectures, Frisch had been working on national accounting, just like Keynes, Kuznets, Leontief, Lindahl and others, providing the tools for economic policy (Bjerve, 1998: 533). Macroeconometric planning was therefore a major alternative to mainstream econometrics, and this was the reason for the opposition of those calling for the axiological neutrality of econometrics.

Furthermore, the decision models were supported by tradition in Norway. Immediately after the end of the occupation, the new democratic authorities had to decide what to do with the planning scheme imposed by the Nazis:[32] Frisch was in favour of keeping mechanisms of direct control, namely of imports, while Haavelmo favoured a policy of low interest rates but with a rationing of credit, both accepting the need for a directive public policy. Stubborn as he was, Frisch dedicated his career to proving the adequacy of the new generation of decision models he had been suggesting since the end of the war.

A first model – indeed, he called it a 'submodel' – was proposed in 1949, in a memorandum for the UN Sub-Committee on Employment and Economic Stability, which was chaired by Frisch. The author dramatised the situation:

> we are facing a race between economic research and economic facts. It is no exaggeration to say that it is a race of life and death. Disaster for the millions will follow if economic and social research and their application do not win. If they lose we will from time to time witness monstrosities as extreme as we had in the 1930s, only with this emphasis on different aspects of the situation.
>
> (Frisch's Memorandum, 18 April 1949)

As a consequence of this challenge, Frisch argued for 'price-wage-tax subsidy policies' in order to maintain 'optimal employment'. In the following years, he suggested three other models using the Leontief input–output technology: the 'Median model', planning income flows and consumer demand; the 'Refi model', considering real plus financial flows and determining how to finance real investment; and the 'Oslo Channel Model', in order to establish optimal national investment planning.[33]

The motivation for these models was the same as that presented in 'Circulation Planning' and in the memorandum for the United Nations. At a lecture given in Paris, Frisch argued in exactly the same sense that the reasons for war were still present and that monstrosities were still possible. The lecture was on the 'Use of Models for the Elaboration of a Rational Economic Policy' and was presented on 17 October 1950, at the Ecole Nationale des Ponts et Chaussées, in Paris. A 'rational political economy' was presented as that which could prevent the repetition of war, conflict and social misery: this new sort of economics would lead the way to a reform of the profession and the redefinition of its purpose: 'The research here sketched is inspired by a leading idea: democracy,

in order to survive the current crisis – and to survive as a democracy – needs to recur to a new type of economic analysis' (Frisch, 1950a: 474–5). Now, this 'new type of economic analysis' could only benefit from comparison with the previous great depression and the situation that had led to the dramatic devastation of the war:

> In order to define precisely the problem, I consider as my point of departure the economic situation as it existed in the thirties. Massive unemployment in most countries led to a monstrous situation. Amidst abundance, the buying power decreased. Food and other means of consumption were deliberately destroyed, while people prayed. This experience leads to a simple but fundamental conclusion: the need to prevent those monstrosities. No solution to any economic problem is admissible unless it satisfies such a condition.
>
> (ibid.: 475–6)

For Frisch, what had been at stake – and what remained on the agenda – was a life-and-death question, and that was the measure of the responsibility that would be laid at the door of the economists and policy makers:

> We live through a true race between economic research and the economic facts. It is not exaggerated to say that the framework of this race is a question of life and death. It would be a disaster for millions of men if economic and social research and its applications do not triumph. If they lose, we will testify, from time to time, similar monstrosities as those we just denounced.
>
> (ibid.: 476–7)

This allowed for the conclusion: in order to fight unemployment, the previous ten or twenty-year-old theoretical tools were not adequate any more – just as the use of ten or twenty-year-old statistics was inadequate for understanding the present situation of unemployment (ibid.: 477). This was a typical Keynesian argument in form and content, but this time it was to be used against Keynes himself. Although Frisch recognised that economic theory had benefited from Malthus and Wicksell, and then Keynes and 'his followers', such as Hansen, he also argued that new conditions now condemned that theory to oblivion. The 'new facts' emerging after the war – the existence of direct forms of economic control in some countries, new patterns of social justice and the modification of the role of international commerce – called for new theories. In such a framework, Frisch argued, the Keynesian view of effective demand was inadequate (ibid.: 477). Planning was the solution.[34]

Planning was not, for a long period, just a Scandinavian idiosyncrasy: 'The "planning movement" was supported with enthusiasm by part of the European economic profession in the first two or three decades after the Second World War. . . . Ragnar Frisch clearly shared in that enthusiasm. Beginning in the late 1940s he turned almost all his research toward the issues raised by the operation of economic planning' and, in France, so did Malinvaud, who was 'sympathetic to most of the ideas prevailing in my country about it' (Malinvaud, 1998: 561). Others could say the same and when, in 1963, the Vatican seminar confronted the opinions of the laissez-faire supporters and the planners, the balance of forces, at least in Europe, was not settled.

For Frisch, planning was a central building block of democratic choice: 'My purpose is to make economic planning at a high aspiration level one of the pillars of a living democracy . . . a democracy that is living in the sense of actually engaging as many as possible of the citizens to take an active part in the affairs of the nation as a whole', he stated in his Nobel lecture (Frisch, 1970a: 27). He frequently argued that the clarification of the impact of alternative policies was a requirement for democratic choice. At a lecture to the Federation of Swedish Industries in 1970, he presented 'a plea for a new type of cooperation between politicians and econometricians. The new type of cooperation consists in formalizing the preference function which must underlie the very concept of an optimal economic policy' (ibid.: 41). Frisch was convinced that the presentation of detailed quantified alternatives was necessary for public debate:

> Such solutions will have a power of persuasion enormously superior to lengthy verbal arguments. It is therefore alternative optimal solutions and not alternative specific measures that should form the object of public debate. . . . this is what I would like to call liberty-planning. It is planning under liberty and at the same time it is planning for liberty.
>
> (Frisch, 1962a: 95)

Of course, such thinking was highly unpopular among post-war econometricians, as evidenced by a bitter discussion held at a Vatican conference that demonstrated what the more influential econometricians were thinking by the early 1960s about the role and destiny of their science. In 1963, the Pontifical Academy of Science brought together for a study week at the Vatican eighteen economists, including Frisch, Allais, Leontief, Theil, Dorfman, Koopmans, Mahalanobis, Wold and others. Some of them presented short papers, all being published within a couple of years, and a debate then followed. Frisch's paper was on 'Selection and Implementation – The Econometrics of the Future'.

It is obvious that the organisers' call for papers resonated with Frisch's preoccupations and repeated his old challenge against Mandeville's implicit wisdom of the Fable of the Bees. The call for the seminar adopted a Frischian version of econometrics:

> Modern economics are extremely complex and both theory and practice show that the free play of individual choice does not guarantee, as used to be thought, favourable results for the community. Once this is admitted it is obviously necessary to provide suitable informative and control instruments and fix the targets which the economy is aiming at. From these requirements was born econometrics, which uses the statistical and mathematical methods

> both in the theoretical study of economic phenomena and in the formulation of directives for economic policy and development planning.
>
> (1963a: 561)

Malinvaud, who was sympathetic to this view at that time, later wrote about this call that 'indeed, it would no longer be possible to write so now, but it was possible in the early 1960s, particularly in Western Europe', given the 'the errors in judgement that were part of the post-war European intellectual climate and that many, including Frisch, found so painful to recognize' (Malinvaud, 1998: 562).[35]

Just as had been the case in the 1930s, Frisch proved to be unrepentant. He thought that everything boiled down to the deficient structure of the free market: the free play of individual choices does not guarantee the community's needs and private vices do not ensure public virtues. As a consequence, the 'liberalistic' tradition was part of the problem and not part of its solution, as he had emphasised again and again in 1934 and 1935 and thereafter. 'What I am going to present to you today is in all humility a frontal attack on a ghost that has been haunting all of us for the last generations. . . . The ghost is human nature itself' (Frisch, 1963a: 1197–8). The consequence was the definition of the task of economics as just one of the means, and by no means the least of these, for arriving at the solution of that fundamental human problem:

> Therefore, the social challenge, facing us as economists and social engineers, is to help the politicians work out an economic system built upon a set of incentives, under the impact of which the economic activity will be satisfactory from the viewpoint of the economy as a whole, even if the behaviour of many individuals is essentially selfish. We must find a means of circumventing the human obstacle to human progress.
>
> (ibid.)

This required a philosophical and political discussion about the preferences of the scientist himself as to the definition of his own science. In such a framework, Frisch was in no way moving against capitalism: he favoured a market system, despite believing that it should necessarily be corrected in order to avoid its worst implications – the dominance of egoism and consequently of war:

> I think it is fair to say that the free market system has two advantages: (1) its simplicity and (2) its effort releasing effect. But it has one fundamental shortcoming: it does not assure the realization of *specific* preferences, such as a high rate of economic growth, a distribution of income and wealth based on social justice, aid to special social groups, economic development of lagging regions within the country, development of special agricultural and industrial sectors (for defence, health or humanitarian reasons) etc. The purpose of wise planning is to realize many such special goals, while retaining as many as possible of the advantages of the competitive system.
>
> (ibid.: 1198)

Next, Frisch discussed three available directions for the betterment of the economic system. The first was the steering monetary and fiscal policy, which he called the 'Samuelson-Solow menu', based on the Philips curve. 'The mild form of steering about which I am now speaking might perhaps be described by saying that it is a timid attempt to introduce a small amount of enlightenment into that which I have called, on several previous occasions, unenlightened financialism' (ibid.: 1199). 'Unenlightened financialism' was the term Frisch had coined in his campaign against the first attempt by the government of Norway to join the EEC – he had strongly opposed that move, and thought it was motivated by the egoistic preferences of the financiers. But, as indicated, this mild description of the first alternative amounts to a definite opposition: Frisch considered the Keynesian steering mechanisms inadequate to the task. The second policy he discussed was direct state intervention on the definition of quantities of goods and services to be produced, forming a mixed economy that would kill initiative and develop inefficiencies, and the third was that of the centrally guided economies of the East. Frisch had no sympathy for any of these.

Consequently, a new strategy was proposed, although it was wisely presented as a limited contribution, not an 'open sesame' in his own words. 'Rather, it is a suggestion as to a way of thinking which I believe is a condition *sine qua non* for real progress in our search for a solution.' This strategy was based on a distinction between two phases in the steering work – (a) selection and (b) implementation. The selection process would consist of choosing the technology and 'preferences regarding the results to be obtained in the nation as a whole, or in the world', but the ways in which to proceed would be ignored. A quantitative model would be built in order to take decisions on the selection of objectives and, once the target had been fixed, the econometrician would be in a position to suggest practical alternatives. 'This will, I believe, be a distinctive feature of the econometric planning work of the future, since our main concern will be research work on how the economy can best be steered' (ibid.: 2000). Finally, the 'implementation' process would consist of defining the national or international institutions needed to operate the model and apply the preferences as defined.

Maurice Allais reacted violently to this proposal, writing a response that was longer than the original contribution and certainly much more polemical.[36] He did not challenge the technical possibility of planning in the way Frisch had suggested, but pointed out that its use required a political stance: 'From a technical point of view, I completely agree in general with his [Frisch's] position, but his paper also expresses many views which rest on value judgements and have evidently many political implications' (Allais, 1963: 1205). The main difference was therefore that 'I think econometrics must remain neutral, i.e. we must avoid introducing political views into our discussion' (ibid.: 1206). Allais did not hide his own political preference, and presented himself as a neo-liberal. But what he could not bear was the idea that science should have a normative inclination: his argument was, as Friedman and so many others had argued before, that economics should be objective and positive, irrespective of the political and social

implications of the policy choices. This discussion on the role of econometrics was frankly assumed to be related to the core of the divergence:

> I do not accept at all that Prof. Frisch's paper can be regarded in any way as specifying the main lines of the future of econometrics. Econometrics is a very powerful tool of analysis but nothing more in itself, it cannot determine what economic policy should be, but only analyze observations and derive, in a rigorous way, the consequences of specific hypotheses. From an objective point of view, it is absolutely impossible to define the econometrics of the future by reference to Professor Frisch's paper. In Frisch's sense there are in reality at least two, three or maybe ten econometrics of the future: the Stone future, the Wold future, the Allais future, and so on. Thus, in my opinion, it is not desirable to connect econometrics with a social philosophy of any kind, however respectable it may be. Econometrics must remain limited to the discussion of technical questions.
>
> (ibid.: 1212)

Curiously enough, the case rested on the argument that social preferences would always be very difficult to define, even if not uncontroversial (ibid.: 1207), that the very notion of the interest of the community was elusive (ibid.: 1208), that social justice was too abstract and that the scientist could not, therefore, consider one single preference function (ibid.: 1209). In a way, this was a self-defeating argument, since what it challenged was the micro foundations of macroeconomics, and that was one of the building blocks of liberalism. In his rejoinder, it was precisely this point that Frisch attacked:

> You know that for centuries there has been a tendency to define neutrality in economics by saying that any analysis which takes the free market system as an axiom, is 'neutral', but any analysis that has the audacity of questioning the free market system is not 'neutral', but 'political' and should therefore not be allowed to enter into the ivory tower of the scientist.
>
> (Frisch, 1963a: 1220)

In any case, Allais felt (and he was the first to react in this way) that Frisch was indeed asking econometrics to change course and to become part of planning. He violently rejected that alternative approach and the episodes discussed in the next section show that his colleagues feared the same outcome and increasingly supported this criticism of Frisch's extreme vision of the duties of the econometrician.

Yet most of the other participants hesitated to enter the discussion at that time. Dorfman indicated that he did not want to enter the 'heated discussion' between Allais and Frisch, but argued that the method of directly interviewing decision makers in order to construct the social preference function, as Frisch had suggested, was not practical, and that econometricians should prefer the examination of past behaviour, i.e. statistical methods. Koopmans argued that it was very difficult to distinguish between structure and objectives in interviews with politicians, and therefore that the definition of the social preferences and alternatives was not objectively based. Mahalanobis, Wold, Theil and Leontief produced short but unsubstantial contributions towards this debate.

Frisch's final argument was an even bolder declaration against the state of economics, and a plea for a politically oriented and practically useful form of econometrics, looking for a certain 'Santa Claus form of the preference function', in order to allow for a choice:

> I even think that they [the alternative economic policies] will constitute the main object of our discussion in the future. If we are discussing the econometrics of the future we have to recognise this, and I will state a personal belief that 100 years from now our grandchildren will devote practically all their efforts to the study of those models that deviate from the free market system. They will only use an infinitesimal amount of their energy discussing such things as, say, the stability of the equilibrium in a free market system. This is my conception of the econometrics of the future.
>
> (ibid.: 1221)

A new, but short, discussion with Allais followed – and the seminar concluded with both camps battling fiercely. Two years later, they had a fresh occasion to bombard each other when Frisch spoke at the First World Congress of the Econometric Society (Rome, 1965). He created embarrassment among his colleagues, who wanted to devote something more than infinitesimal parts of their energy to discussing the stability of the equilibrium in the free market system. Some afterthoughts in relation to this Congress lecture were included in one of Frisch's last contributions (Frisch, 1970c), a chapter in Harrod's *Festschrift*. Having already received his Nobel Prize and nearing the end of his career, Frisch indulged in a general reflection on the state of econometrics. That is why he returned to his 1965 argument, and repeated it without any regret or attempt at reconciliation: 'at that juncture of econometric development, I believed I could render a better service to the econometric fraternity by being critical and outspoken than by sugar-coating the pill. I still hold that view today' (Frisch, 1970b: 159).

Critical and outspoken he most certainly was, and his colleagues were not used to it. But, for Frisch, this was in line with the editorial of the first issue of *Econometrica*: 'The policy of *Econometrica* will be as heartily to denounce futile playing with mathematical symbols in economics as to encourage their constructive use.' Almost forty years later, he continued to think that this was not a *formule de politesse*, and that the constructive – i.e. social – use of mathematics was still 'a social and scientific responsibility of higher order in the world of today'. The example he gave was an attempt to address the question of the feasibility and usefulness of this research into alternative preference functions and models to simulate their implication: the trade union employers' negotiations in Norway supported by econometric models were presented as a practical way of avoiding confrontation and taking wise decisions.

This echoed Frisch's criticism of *unenlightened financialism*, damaging for society for at least two reasons: for the egoistic behaviour and imposition of the interests of *financiers*, but also and not least for the *unenlightened* nature of the choice they imposed. On the contrary, Frisch favoured public discussion and the confrontation of the choices of social agents, through formal models that he expected to be objective enough to be accepted by all parties as a standard check. Once they had all the available information and could compare the economic and social effects of the programmes of the political parties, the electorate would be in position to choose – that was the perspective of 'enlightenment'.

This was also the reason for his rejecting and indeed despising the over-simplifications of standard mainstream theory. Such thinkers could not understand and act on reality, since real economic complexities were much closer to the landscape of a curved river-bed with steep banks, through which a raindrop would find the way to the ocean, than to the pale image of the geometrically arranged Euclidean objects of mainstream fantasy. Moreover, reality could be more appropriately described as a map of several river-beds or as a diffuse river-bed: 'Economic life and technical possibility are – just as the pattern of river-beds and bank steepnesses we find in the concrete shape of a country – too diversified to be classified according to some rule derived from oversimplified assumptions' (ibid.).

As the argument goes, over-simplification for the sake of mathematical expertise unadjusted to the solution of relevant problems, that was the danger for economics. And we may imagine the reaction of the World Congress to these declarations, even if their author could not be suspected of anti-mathematical obscurantism:

> And in particular we are absolutely certain of getting irrelevant results if such epsilontic exercises are made under the assumption of constant technology. 'In the long run we are all dead'. These words by Keynes ought to be engraved in marble and put on the desk of all epsilontologists, in growth theory under an infinite horizon. . . . And he [the policy maker] is not interested in knowing whether an actual development path in his country will come closer to or be far away from some intrinsic path that has been defined by piling up queer assumptions.
>
> (ibid.: 162)

Old age and fame afford certain privileges. Frisch used them all in this speech, and the chapter illustrates the points he made well enough. But the nub of the argument is still, I believe, the war and the dramatic effects that it had for a whole generation. He did not want to lose time: social responsibility should determine the choice of the subject and the strategy of the scientists. 'Too many of us used too much of our time and energy on the study of the keyholes in Northern Ireland in the first half of the thirteenth century', he bitterly complained (ibid.: 162–3). Consequently,

> observations get a meaning only if they are interpreted by an underlying *theory*. Therefore, theory, and sometimes very abstract theory, there must be. . . . But at the same time I have insisted that econometrics must have relevance for concrete realities – otherwise it degenerates into something which is not worthy of the name econometrics, but ought rather to be called *playometrics*.
>
> (ibid.)

Tinbergen reacted in the very same way:

> I'm afraid [technique drives economics away from human needs], yes. But, of course, that always has two sides. One of them is my insufficient interest in and knowledge of difficult mathematics. This means that automatically you tend to neglect the more subtle mathematical/statistical issues involved and that means also a considerable portion of econometrics. So, my interest is typically purely economic and sometimes I feel that so much refinement of methods of testing is perhaps not necessary. But I am not quite sure, so I say it tentatively. I simply cannot read the larger part of *Econometrica* anymore.
>
> (Tinbergen, 1987: 136)

New wars on the horizon

Were the younger members of the audience at those seminars and congresses too far removed from the preoccupations of the men and women who had lived the war or suffered its consequences? The fact is that they were apparently not impressed by Frisch's vigorous defence of his version of econometrics both for the future and on behalf of that past. Not only had his approach to planning become a purely academic and Scandinavian affair, with almost no attention being paid to his contributions on that matter – with the exception, for a while, of the government of Egypt, but surely without any support from his own government – but the econometric milieu also reacted to his proposals much as Allais had done. This change of mood is detectable in a couple of episodes that illustrate Frisch's by then difficult relations with the managers of the Society he had founded, related to non-scientific implications.

The first World Congress, which finally met in Rome in September 1965, had initially been called for Jerusalem. Being aware of that project, Frisch reacted immediately and sent a letter directly to the Fellows and to the Council of the Society on 20 March 1963. His argument was that the congress should not be held in Jerusalem:

> In view of the tense political situation in the Middle East, and in particular in view of the deplorable conflict between Israel and the surrounding Arab countries, the fact that the first World Congress of the Econometric Society is held in Jerusalem would – however 'purely scientific' the Congress be organized – appears as a political demonstration.

Instead, he suggested Lausanne, where the first Econometric Society meeting had been held in the 1930s.

A number of Fellows participated in the discussion that followed. Tobin wrote a careful letter on 31 May 1963, supporting Kenneth Arrow and Jacob Wolfowitz's argument that the only grounds for disqualifying a country as a venue for the congress would be security and free access for the delegates. Given the 'strong political feelings' aroused by the proposal, Arthur Smithies suggested a new venue that would minimise the costs of transportation – a very economic argument (3 June 1963). In the same sense, Harold Kuhn and William Baumol argued on 14 June that the only criteria to be considered were access, resources for the meeting and sponsorship, and that no 'extra-scientific political considerations' should be considered. But the tone had been set: although many of the Fellows suspected the political implications, and furthermore the very principle of taking political considerations into account, they also feared the sensitivity of the choice and their possible involvement in the Israeli–Palestinian crisis. Roy was the only one supporting the original choice, arguing that Jerusalem was convenient for the travel of the European members and that no political implications were involved.[37] At the time, Malinvaud was the president and Solow the vice-president of the Society: they called for a vote at the Council and the majority decided against Jerusalem. On 18 September 1963, Rome was chosen instead.

Shortly afterwards, a second episode proved that the venue for conferences was a difficult matter indeed. The European Meeting of the Econometric Society was scheduled for Barcelona in September 1971, and this generated a lot of criticism since the country was then ruled by a ferocious dictatorship, with some econometricians, including Frisch, opposing convening the conference in Spain.[38]

That was the last contact he had with the Society he had helped to create forty years earlier, which he had directed and decisively influenced for such a long period, and which had now moved towards *playometrics*, or so he thought.

The radical years

The experience of the war was never explicitly at the centre of the economic analysis either of Frisch or of any of his close collaborators in the econometric endeavour. Yet it dominated his thinking as the historical background warning of permanent danger, and motivated his quest for 'a means of circumventing the human obstacle to human progress'. War provided the ultimate measure of the challenge to science and the duty of the scientist. Liberalism, free trade and international competition were all seen in this framework as possible causes rather than as solutions to this general problem of defining the best way to develop egalitarian societies and to avoid greed, confrontation and militarism.

Through the evolution of the Cowles Commission in its first decade, this policy orientation remained at the core of the research: when it moved to Chicago in 1939 and adopted an explicit probabilistic model and a simultaneous equations approach, policy making was still the desired outcome of the exercise. This is why these creators of the movement feared that econometrics had subsequently evolved away from business-cycle analysis and considered any abandonment of the search for viable economic policies a definite betrayal. They felt that the notion of social responsibility was lost, and in some cases replaced by sheer playometrics, whose irrelevant beauty conquered many souls but did not help to solve any concrete problem – and that was why, in the late 1960s and in his last contributions, Frisch did not hesitate to challenge the prevailing attitude of many of his colleagues. He did not endorse their magnificent technical improvements nor praise their ability, just noticing their appetite for abstruse reasoning.

In contradistinction to these choices, I submit that, as for many in the early days of this generation, the reason for Frisch's lifelong devotion to the definition of the role of econometrics – passing through several minor changes, such as the abandonment of the macro-modelling of cycles and his early attempts to develop new methods of statistical analysis, for instance – was his experience of the unemployment, deprivation and war periods of the 1930s and the 1940s. This demanding attitude produced an immense effort that outlived their contribution as social scientists. After them, economics was changed for good and forever.

12 Conclusion

A brave new world

The 1930s were fascinating but frightful years. It was a period of shock and fear, a time of war and racism, deprivation and misery. It was also a period of intense intellectual change, in particular in the sciences, promising thrilling adventures. As I followed the story of this cohort of mathematicians, physicists and economists converging from different perspectives and backgrounds and sharing the perspective of reinventing economics, it became clear that the intense menace of that time was matched by their strenuous efforts to uncover the secrets of economic evolution, to tame cycles and to stave off depression. They were hastened in their work by the countdown that began after the great crisis of 1929. This was the motivation that imposed their selection of both the problems and the techniques that they used to represent, to simulate and to compute. Under this generation, economics became the laboratory of social sciences at a time when social responsibility had become a matter of civilisation.

They were, as Frisch's words went, 'not afraid of the impossible'. They wanted to measure the immeasurable – they were eager to change the paradigm in economics, matching the rigour of natural science and its ability to control, portraying society as a machine.

It was a sweeping movement and it was unique. Unlike the previous neoclassical revolution of the 1870s, this was a structured international process of change right from its very inception – it was coordinated and not only convergent, articulated and not only paralleled. It deliberately created a common language and not only echoed a common metaphor. Last but not least, it led to the construction of institutional structures powerful enough to provide the impulse for a new professionalisation of economics, which finally led to the American dominance of the discipline.

When Frisch, Schumpeter and Haberler met at the Colonial Club in Harvard, in February 1928, they certainly measured the burden of the task they were assuming. When less than two years afterwards, by the end of December 1930, sixteen of twenty-eight invitees showed up at the Statler Hotel in Cleveland, Ohio, the heterogeneous and yet qualified audience could have not felt the same, since many of them only incidentally followed the works of econometrics. But the pioneers were there, ready to conquer economics, as indeed they did.

Shackle aptly called these years 'the years of high theory': it was the period of a paradigmatic revolution, moving economics from the heights of purely abstract theory to the mundane world of firms and policies, expansion and depression, labour and capital, unemployment and technological change.

Pure economics was the world of stationary equilibrium, of markets with no movement, economies with no time and agents with no will. It was a magnificent construction, opening wide the door to the mathematisation of economics, at the cost of imposing severe restrictions on the concept of agents, predicated upon its original metaphor:

> The fatal defect of the older conception was its assumption that men possess adequate knowledge, that they can act in the light of reason fully supplied with its necessary data. It is the false analogy from celestial mechanics, the unconsciously wrong and misleading interpretation of the word 'equilibrium'.
> (Shackle, 1967: 136)

Consequently, some feared that econometrics was useless for practical analysis, since 'in its arresting beauty and completeness this theory seemed to need no corroborative evidence from observation' (ibid.: 5).

As the years passed and the *belle époque* came to an end, the hurricane of the technological revolution brought electrification, the first steps of Fordism and then the frightful years of the depression, calling for a fresh discussion on the foundations of economics. Uncertainty became a nodal concept for the critique of neoclassical economics and, after the first attacks by Frank Knight, from Chicago, and the Swedish school, others jumped aboard this bandwagon: such was the case of Keynes with the notion of expectations, and later Robinson and Sraffa with the notion of imperfect competition.

Consequently, as economics was divided into powerful contending schools, reality imposed its spell and Keynesianism was able to cross the available theoretical boundaries and impose a new agenda. Yet, the glamour and success of his *General Theory* could not obscure the fact that another movement was simultaneously emerging as a response to this very same intellectual challenge.

Indeed, the 1930s defied economists precisely because the obtained equilibria were so threatening. That was why the new econometric generation looked for an alternative, and they sought that alternative precisely by concentrating on the political economy of the cycle, sharing some of the views of Keynes but not his choice of technical or analytical procedures, since they wanted more effective and quicker processes in order to tame the economic fluctuations. Consequently, at least some of the major players in the econometric movement converged with the motivations of Keynes and yet both movements rapidly grew apart. This book has discussed the history of this surprising mismatch.

The turning point

From all points of view, 1933 was a year that changed the world. After the drama in Germany, Europe was soon to be immersed in a tragedy that spread

across the whole world. It was also a year of great innovation in economics and science. Kolmogorov established the axiomatic basis for stochastic inference; Neyman and Egon Pearson finalised their strategy for the testing of hypotheses; and Frisch published his rocking-horse model of cycles, a difficult but impressive system of equations embodying the concept of an equilibrating system driven by exogenous shocks that produced cycles.

Econometrics was born as a programme for the exhaustive mathematical representation of models as the legitimate form of theorising, and for computation to serve as the practical procedure for corroborating the model. Statistical inference was later added to this toolbox, but not without some disagreement between those concerned.

As a consequence, econometrics was originally conceived of as a universal language, adapted to all enquiries in economics, and not just being treated as the exclusive peculiarity of some economists. The careful institutional setting, attracting the likes of Frederick Mills and Wesley Mitchell from the NBER or John Maynard Keynes from Cambridge, as well as a diversity of Walrasian brands, were clear signs of this display of openness, admitting that a new synthesis could emerge out of these procedures. The episode of the discussion of Tinbergen's report to the League of Nations is yet another instance of this process of selection in the midst of variety, since it was designed to represent Haberler's comparison of all the different contending theories of cycles and to provide a bridge to a new phase of inclusive research. But it proved to be a failure from that point of view and when, after the Keynes–Tinbergen controversy and after the Cowles programme was stabilised and the Koopmans–Vining confrontation was held, econometrics and the Keynesian and institutionalist circles were too far distanced from one another.

It is less well known that, while Marschak, Lange and so many other econometricians precipitated to defend Tinbergen, other tenors of the movement dwindled into doubt. That was the case of Frisch, one of the protagonists of this story, since he strongly felt that causal correlation could not be obtainable out of statistical inference, simply because the fundamental equations could not be authoritatively established. Therefore, he favoured the painstaking method of interviews in order to define intentionality and structure in economics – and he was alone on that. He believed he had less but wanted more than the others: in spite of the unavailability of a trustable formal system to define legitimate statistical inference, Frisch asked for a definite conclusion for direct control of the economy.

Reinvention

During the years of high theory, a number of great debates reinvented economics. They were all centred upon the emergence of econometrics: the Leontief–Frisch pitfalls debate on the estimation of demand and supply schedules, the Keynes–Tinbergen debate, the 'Measurement Without Theory' debate between the Cowles group and the institutionalists, the implicit debates on the stochastic nature of economic data with Haavelmo, and the Socialist Calculation debate with Hayek, Lange and many others, all provide clear proof of how the world of economics focused much of its attention on econometrics during that period.

This book has dealt with these and other debates, pointing to some of the subtexts that have become buried in the past. One of these was the eugenic connection, a social engineering *avant la lettre*: some of the inventors of modern statistics, such as Karl Pearson and R.A. Fisher, chose to develop biometrics given the motivation provided by their eugenic inclinations. In spite of an ocean of divergences, John Maynard Keynes and Irving Fisher also shared the same eugenic allegiance, although they inferred opposite conclusions from it for their own scientific interests: whereas the former gave greater emphasis to organic concepts, the latter was fascinated by mechanical modelling. This resisted as an unresolved issue during the whole period under scrutiny in this book.

In any case, under the influence of these constructors of statistics and modern economics, the econometric generation struggled with the difficult choice of a guiding metaphor. The option proved to be mechanics, but this short history has highlighted the intense difficulties encountered in transposing the mechanical analogies into economics. They were indispensable but impossible: as a representative agent of these econometricians, Frisch wrote once that he could only understand a theory if it was represented by a mechanical model ('I, for one, never understand a complicated economic relationship until I have succeeded in translating it either into a graphical representation or into some mechanical analogy', quoted in Chapter 5), but he soon discovered how economics could become trapped by these analogies, as did so many of his collaborators and colleagues. Yet some persevered, while others adopted a more distant stance as the limits of the analogies were being explored.

Mechanical devices were not only formal concepts used for the introduction of mathematical representation: they were indeed used as models, which required drawing models, designing apparatus, thinking of their functioning and using them to illustrate arguments. The pendulum, the most successful of these models, had already fascinated Marx, Fisher and Pietri-Tonelli, as it attracted the attention of Yule, Hotelling and then Frisch, Schumpeter, Marschak and Tinbergen. The episode of the correspondence between Schumpeter and Frisch on pendula is a telling example of the use of a metaphor to seduce, although in this case with no convincing results.

At least to both Frisch and Tinbergen, the rationale for the pendulum attractiveness was based on their definition of completeness in economics as the presentation of a totally deterministic model, with as many variables as equations. The pendulum was one of such, and furthermore a well known mathematical entity, that could be played and displayed with.

But the pendulum provided for another innovation, and a fundamental one: a new type of mechanics the econometricians were requiring, as they looked for new tools of measurement that could be used for the analysis of change. In fact, this new generation was faithful to the original influence of mechanics, but required its extension into the field of statistical mechanics: statistics could

account for irregularities and describe real dynamic processes, they could measure and could also – as they soon discovered – infer as well as deduce. Statistical inference was constructed as the legitimate mode of research for this generation, as lawful science.

Yet, statistical inference had not emancipated from an ambiguous epistemological status. Its reference was originally astronomy, with its well defined populations ruled by unquestionable general Newtonian laws and the samples of observations necessarily confirming these laws. In that framework, statistical deviations were simply attributed to errors of observation or the inaccuracy of the system of measurement. The laboratory counterpart of this statistical conception established as well clear rules of inference from the observations obtained out of a device embodying the law of behaviour of a system. Yet the laboratory could conceive experiences as purposeful perturbations that could be added to the system – still, a strictly mechanical world with no stochastic shocks but certainly with induced variation. In fact, by the end of the nineteenth century, the only conceived world of real stochastic shocks as the natural source of variation was biological inheritance, but *hic sunt leones*, that was unchartered territory.

Consequently, the young econometricians chose a strategy avoiding the labyrinths of justification but allowing for the simultaneous representation of a well known mechanical device and some incorporation of shocks impinging on the stable structure for the sake of mimicking irregularities: that was provided by the heuristics of the pendulum. For Frisch, the shocks were added but ignored, present but unexplained.

It was up to Haavelmo to challenge this model and to suggest an intrinsically stochastic world as the basis for an inference strategy.

Probability

The limits of the mechanical analogy were highlighted by the unresolved issue of the introduction of probability concepts. As economists carefully ignored the Homeric confrontation between Neyman–Pearson and R.A. Fisher over the foundations of statistical inference – in spite of so many of them having lived through it, such as Hotelling, Schultz, Frisch, Haavelmo, Koopmans and others – they struggled with a confused entanglement of arguments on the nature of the economic laboratory.

The very lineages of probability and statistics were ensnared in economics. The ambitious positivism of Mach was developed by Karl Pearson, who stood by correlation of observations ignoring chance and considering errors as mere artefacts from human failures. Schultz, a disciple of Henry Moore, himself a disciple of Carl Menger, studied with statistics with Karl Pearson and his first attempts at probability were moulded in that framework – but he soon emancipated and looked elsewhere. Unlike Pearson's approach, for R.A. Fisher experimental data should be tested by measures of significance, and Hotelling was directly inspired by this view. Contradictorily, Neyman and Egon Pearson established a powerful technology to infer and test hypotheses under the assumption of sampling from presumptive populations in a stochastic framework. This was what Haavelmo adapted to economics.

It is a fact that the intriguing analogies of the pendulum had provided a bridge between the conception of a mechanical device and a general mathematical description with separate random elements, but this was challenged in mechanics itself by non-representable double and triple pendula, by imagined rooms full of rocking horses suspected of coupling, resonance and in general interference phenomena, or by Schumpeterian innovations endogenous to the economic system.

Some of the annoying doubts on mechanics, namely on the application of a kind of Newtonian law, were highlighted by several episodes discussed through this book, such as the Creedy episode, opposing Roos and Tinbergen to Frisch, Le Corbeiller and Hotelling.

In spite of all this paraphernalia of mechanical variations, one of these mechanic wonders, the pendulum, prevailed as a simple mathematical object and as a representation of the cycle: a dampening structure plus some exogenous energy constituted by small random shocks. Equilibrium and movement, this was all that was necessary.

Necessary but not sufficient, since the generalisation of probability required the reconsideration of the values of economic variables as extractions from imaginary worlds, as Haavelmo suggested – not only the small shocks adding to the equations, but the variables themselves. The very idea of parallel thought universes was popular at the time and strongly defended by Karl Pearson as well as by Ragnar Frisch, but the latter could not extend it to the acceptance of the essential probability concepts. Frisch suspected these concepts to the very end of his life and, when axiomatisation began to plough its own particular path, he feared that they could only lead to playometrics.

The primacy of the pendulum organising metaphor in cycle analysis had indeed a devastating consequence soon to be evident: it vitiated the assumptions of Haavelmo's probabilistic revolution, since it alienated the stochastic framework from the functioning of the system itself, deterministic as it was considered to be. Consequently, two theoretical directions were thereafter followed, one by Frisch maintaining the pendulum model and for all practical purposes ignoring stochasticity, and the other developing what became the programme of structural estimation, the core of the early Cowles Commission project.

The background for these digressions so pregnant of epistemological divergences was the reception of the heritage of the Vienna Circle, where Wittgenstein's influence crossed paths with the activism and inventiveness of Rudolph Carnap, Otto Neurath, Hans Hahn, but also Quine, Hempel, Tarski, A.J. Ayer and others. In fact, most of economists daring enough to formulate their methodology would share at that time the impulse of the logical positivist strategy. Some had been there, such as the then young student Karl Menger, later one of the founders of the Econometric Society who was present at the Statler Hotel meeting, and of course the most influential of them all among economists, John (then Johann) von Neumann.

The positivist approach favoured a high profile positivism abandoning all recourse to intuition in order to define the meaning of a proposition according to the means of its empirical verification, and establishing axiomatics as a 'truth purification' device based solely on axioms, rules of inference and deduced theorems (Goldstein, 2005: 126) – but it is less than the truth to propose that this was either understood or followed by economists. Frisch certainly endorsed what he called 'axiomatics', and even proposed it in 1927 as a reading of neoclassical economics, with the concept of 'force' as its core, being, as for Irving Fisher, the convenient translation of marginal utility. But his unfaithful axiomatics could not dispense with thought experiments and was tainted with intuition. It was not axiomatics altogether.

Axiomatics reappeared in economics as a last recourse after the failure of the business cycle research and of that of the structural estimation programme, and as a programme for an approach following Hilbert's view of mathematics as a general and complete deductive logic. That was the time for Marschak and Koopmans at the head of the Cowles Commission, when the canon was established.

Yet, its foundations were shaken even before they were laid. Kurt Godel, who attended the Vienna Circle meetings, had established when he was just twenty-three years old the theorems of incompleteness refusing the dogma of a closed and self-sufficient demonstrative logic in formal systems. Intuition and paradox were therefore impossible to eradicate.

It bears testimony to the grandness of von Neumann that he was the first, and for some time even the only, to understand the nature and the success of Godel's challenge to a project he shared, that of Hilbert, and to diffuse the devastating results of his colleague against his own previous Hilbertian allegiance (ibid.: 161, 195).

This presented two aggressive attacks on economic canons: first, on the general claim of the sufficiency of formal systems, second and not least on the mechanical metaphor itself, as the representation of a closed system.

Indeed, as econometrics gradually unfolded under the guidance of the probabilistic revolution, the mechanical metaphor was abandoned since it had lost both its original sense and its point of reference. Drawing samples of economic series from populations of imaginary variations could no longer qualify as a mechanical analogy or as a laboratory simile – it was sheer imagination. This became the requirement for the production of probabilistic inference and then of theory, when statistics failed to achieve economic corroboration.

It was as a consequence of this shift that the strategy of the Cowles Commission was stabilised under Koopmans and Marschak during the 1940s: the econometricians were producing more pure theory than applied research, more conceptual discourses embodied in formal models than statistical estimation and corroboration of those models, a bizarre turn of events considering the original motivations for the birth of the movement. Simultaneously, and predominantly under the seal of military secrecy, operations research, game theory and experiments were being played at Rand, sometimes by the very same scientists who had been around the Cowles seminars – looking for an alternative toolbox with which to dissect economic reality and to understand the generation of information, the behaviour of agents and their strategies. For some, such as von Neumann and Wiener, the crux of the matter was that these new tools were considered to be more adequate for prediction and control, the same aspiration that had eventually led to the previous econometric upsurge, with which they had by then ceased to be enamoured. And, last but not least, the econometricians were not even slightly interested in their endeavours: as Morgenstern's diary shows, in October 1950, when he approached Frisch and Tinbergen in an attempt to explain game theory to them, they 'wanted to know nothing of it [game theory] because it disturbs them' (quoted in Mirowski, 2002: 139).

Control

Prediction and control: that was the very definition of early classical economics. The need for control was a central theme of successive debates, namely the selection of the business cycle as the core of economic policy, and of the econometric programme, such as it was established in the dark days of the 1930s. But that was also the case of another subtext that has only been scantily investigated in the history of economics: that of the building blocks of the Socialist Calculation debate, in particular the attitude of those economists whose Walrasian inclination was motivated by their search for a general equilibrium as the representation of the maximisation of social welfare, an early strategy for some sort of 'market socialism'.

In this book, I followed one of the expressions of that tension, namely the enthusiasm and the disillusion of a number of founders of econometrics, who conceived of it as a radical programme for the creation of economic control. They thought this was urgently required: Frisch argued in a debate with Tinbergen and Koopmans one night during the 1935 Namur Conference of the Econometric Society that egoism leads to the devastation of crises, and consequently economic policy should constrain the agents to impose coordination, the institutional form of altruism and cooperation (Chapter 10). Yet, the prevalence of control challenged the notion of agency as it was established in neoclassical economics and tended to impose a new direction to econometrics.

In that, the founders were defeated, since their demanding definition of econometrics was superseded by the sophistication of the mathematical abstractions and the development of pure theorising, which distanced the kind of econometrics favoured by the Cowles Commission from their original instrumental concepts.

As a consequence, to the dismay of their colleagues, Frisch and Tinbergen ceased to contribute to econometrics after the Second World War. They continued to teach, to investigate, to publish, to travel, to recommend policies, to debate with the same energy, but not in the field of econometrics, or at least not in the area of what econometrics came to represent. Planning was their brand of econometrics and this they continued to pursue. As a consequence, they were revered but alienated from the movement they had founded.

This is the personal drama amidst all the glory: the winners of the first Nobel Prize in economics, rightly rewarded for their contribution to the creation of econometrics, were by that time no longer involved in that field, even though they considered that it was econometrics that had deserted them. Even Haavelmo, who was fundamental to the probabilistic shift and to the incorporation of the Neyman–Pearson approach into economics, almost ceased to publish in that field. It was either too late or too early: by that time, they had already impressed and motivated many cohorts of scientists, creating a shared language and concepts that reinvented economics, but could not convince their colleagues on what type of econometrics was required.

Although it is certainly not possible to reduce the generation of the founders of econometrics merely to a listing of their accomplishments, it is fair to state that they have left us with a splendid legacy of concerns, problems, quarrels, brilliant insights, intense dedication and an insatiable desire to fight against the difficulties and to understand and explain. After all, this is what science is all about.

The immense success of the econometric generation was its reinvention of economics, which opened the path for the reinvention of econometrics itself, as it became the brave new world of the twentieth century.

Notes

1 'Not afraid of the impossible': Ragnar Frisch (1895–1973)

1 Although, up to this moment, there is no authoritative biography of Frisch, Bjerkholt (1995) and Andvig (1995a) provide fairly complete and competently written overviews of his life and career.
2 'Around 1630 King Christian IV of Denmark-Norway asked the Electoral Prince of Saxony to send him a team of mining specialists from Freiberg in Saxony (that had a Mining Academy) to the newly-discovered silver deposits from Kongsberg, Norway. We can trace our ancestry fairly exactly back to that time' (Frisch, 1970d: 211).
3 Original emphasis. Unless otherwise stated, italics are shown as the author indicated, throughout this book.
4 Their only child, Marie Ragna Antoinette, was born in 1938. Ragnar's first wife died in 1952 and he married Astrid Johannesen in 1953.
5 His work developed in many different directions. For instance, in 1923, Frisch submitted his 'Projet de Développement du Nombre Classificateur Décimal' to the Institut International de Bibliographie, in Brussels.
6 The main influences on Frisch's economics were Marshall, Wicksell and Fisher. Wicksell influenced his early career and was highly praised by Frisch: 'It must be said that there was just one Scandinavian economist of first order: Knut Wicksell, former professor of political economy at Lund University (Sweden), disappeared last year. I consider him a first order thinker, penetrating and original' (Frisch to Lutfalla, 26 January 1927). With Fisher, however, Ragnar developed a close personal and scientific relationship within the Econometric Society (see Chapter 2).
7 Frisch used to write in one of four languages: Norwegian (mostly unpublished works and classroom lectures), French (early papers), German (rarely) and English (most of his international communications and published papers).
8 Under the title 'Sur les Semi-invariants et Moments Employés dans l'Etude des Distributions Statistiques' (Frisch, 1926b). The 'semi-invariant' is a variety of a moment generating function, the modern term being 'cumulant'. There are slight differences between the submitted manuscript of the thesis and the later printed version of the book.
9 In the printed version, the formulation is slightly different and certainly clearer:

> In the empirical part of mathematical statistics, which we could equally well call the theory of the inversion problem, we consider not a given scheme but a given result. From one observation or a series of empirical observations, we try do determine firstly the most likely scheme to generate the data. This is the quantitative problem [sic; qualitative?]. Then we try to determine the 'presumptive' value of the parameters defining the scheme. This is the quantitative problem.
> (ibid.: 6)

The 'inversion problem' – how to establish the laws of motion from the real data itself – was to become a crucial issue throughout Frisch's scientific career.
10 This government decision was taken in order to prevent Frisch from accepting the offer of a position at Yale.
11 It must be added that Frisch, in turn, suspected the 'soundness' of Austrian verbalism (Andvig, 1986: 27) and did not hide his opinion: 'It is no doubt true that the original formulation by the Austrians of these subjective elements proved, on closer inspection, to be untenable and required modifications' (Frisch, 1932A: 3).
12 In spite of this challenging tone, Frisch's contribution to time series analysis did not convince many of his colleagues. According to some, his method was not so original as the one developed by Persons (Morgan, 1990: 89). Frisch only produced a couple of papers on the subject, in 1928 and 1930, and then returned to the topic at the end of the decade, with no publishable results.
13 Mills, a NBER researcher, was later involved in editorial work for *Econometrica*, during its initial year.
14 Mills to Frisch, 21 February 1928. This letter can be found in Frisch's Archive (Oslo University). The Schumpeter Archive (Harvard University) and the Tinbergen Archive (Rotterdam University) were also investigated.
15 Alvin Hansen confirmed Fisher's invitation by letter, dated 3 April 1930 (the stay was originally scheduled to be from January 1929 to January 1930). Fisher paid $7,000 for Frisch's wage at Yale from his own pocket (Fisher to Frisch, 27 June 1929). Frisch departed as soon as he got confirmation and produced an immense body of work while at Yale, amounting to almost 500 pages of typewritten lectures.
16 As stated by Edvardsen, who was one of Frisch's assistants in Egypt, Frisch was particularly pleased with his work in that country, since he had been able to involve many economists, develop new models and teach about them, unlike his work in India, where he was only able to report to the Statistical Institute (interview with Kore Edvardsen, 1996). Marcel Boumans indicated that Tinbergen also returned from India deeply moved and that the experience of his travel influenced his move to development economics (private correspondence).
17 Frisch wrote more than 150 papers, lectures and books. For instance, for his lectures and research from 1947 to 1964, Frisch prepared more than 250 unpublished memoranda with thousands of pages, equivalent in size to a large number of books.
18 Frisch also led a full life: he was not only an economist and mathematician, but also a specialist in apiculture, which he described as his 'obsession', and kept a farm seventy-five kilometres from Oslo, where he developed his amateuristic genetic research. He was also a keen mountaineer – this is how he broke his leg, preventing him from attending the Nobel ceremony at the appointed time – and wrote precise instructions for other mountaineers based on his own experience (for instance, in 1930, he prepared a thirteen-page typewritten report on 'Hikes in the Environs of Moraine Lake Camp, Rocky Mountain of Canada', available for those wanting to climb those mountains).
19 Frisch to Cowles, 20 February 1933.
20 Cowles to Frisch, 9 January 1936.
21 'Would you mind answering the following question? What type of calculation machine do you regard as being most suited for econometric work, especially for the ordinary work involved in correlations, etc?' (Marschak to Frisch, 19 November 1935).
22 Frisch also sought to collaborate with de Wolff on an ambitious project, the preparation of a book on nonlinear dynamics, which was never developed beyond the original idea. More will be said about this later on.

2 The emergence of social physics: the econometric people are assembled

1 Divisia to Frisch, 1 June 1926. All the correspondence between Frisch and Divisia was written in French; as in the previous cases of French texts, I am responsible for the translation.

2 'Nevertheless, I do not hide the fact that I am rather sceptical about the very principle of the method consisting of firstly analysing the elementary phenomenon in order thereafter to deduce the global phenomenon, I mean to study how the individual behaves (which is peculiar to choice theory).' Divisia further insisted that 'in the treatment of the observation material, I believe the usual methods of statistical science to be often either too vague or tainted with arbitrariness' (ibid.).

3 The contacts between the two men were intense, dealing in particular with the publication of Slutsky's paper on cycles emerging from random shocks. However, during the period of their correspondence, both the Society and the journal were still mere projects. The reasons why Slutsky did not become a member of the Econometric Society are unknown. He was both a friend and a regular correspondent with Frisch, and his 1927 paper (later published in *Econometrica*, 1937, under the auspices of Frisch) was widely circulated and attracted much attention. But there is no indication in their correspondence about the reason for Slutsky's failure to participate in the Society, although one may speculate that the evolution of the USSR in the late 1920s and his fear of the political consequences of being associated with a foreign institution eventually decided the issue.

4 Also referred to in the letter from Schumpeter to Frisch, 19 September 1930.

5 Lutfalla to Frisch, 10 November 1928.

6 Divisia to Frisch, 23 July 1930.

7 Pigou, Robertson, Cassel, Slutsky and John Bates Clark never joined the Society.

> Professors Pigou and Robertson of England have refused membership of the Society. Slutsky's name was removed from the list at the direction of Professor Fisher. I assume, but do not know, that Slutsky refused membership. Kondratiev, according to a rumour reported by Roos, is dead – executed by the Soviets. Hicks of England, Porri of Italy and Lange of Poland have never been formally proposed by anyone for membership of the Society.
>
> (Cowles to Frisch, 4 November 1932)

Frisch added that Pigou would not accept the invitation unless he was elected Fellow, since he was 'afraid of getting into the same class as all the ordinary members'. Frisch was convinced Slutsky would accept if re-asked and should also be proposed as a Fellow (Frisch to Cowles, 24 November 1932). Kondratiev was still alive, although imprisoned.

8 Frisch was responsible for the wording of the draft of the Constitution, after consultation with several founders of the Society (Frisch to Bowley, 27 July 1936).

9 Fisher to Edwin B. Wilson, 28 November 1931, Harvard Archive.

10 Cowles to Frisch, 4 November 1932.

11 The election was held according to a laborious method: the members voted, but corrections were made to the result of the vote in order to take account of country distribution and other criteria (consequently, Darmois was replaced by Colson, although he had received two more votes; Schneider and Kondratiev were elected, although they received less votes than Persons and Leontief, who were excluded).

12 Frisch to Schumpeter, 5 November 1931. Schumpeter accepted Marschak (Schumpeter to Frisch, 11 December 1931), although later on he went back on his recommendation. More will be said about this subject later on.

13 Divisia to Frisch, 15 August 1931.

14 Fisher to Schumpeter, 16 June 1931.

15 Schumpeter to Frisch, 28 October 1931.

16 Frisch to Fisher, 3 July 1931.

17

> For my own part there is only one man whom I should now like to propose as fellow, namely Dr. Marschak. As you will remember, I was a little hesitant about Marschak in the first round, my hesitation being caused by Divisia's remark that probably Marschak did not know what a partial derivative was. That was the only thing that held me back from recommending Marschak.
>
> (Frisch to Fisher, 6 December 1933)

In previously considering Divisia's insinuation, Frisch had for some time been convinced of Marschak's virtual errors and, when he recommended one of his papers to be translated for *Econometrica*, he indicated to his managing editor that 'here Marschak uses partial differentiation, and from my contact with him I would not be surprised if he had made a slip in considering one quantity as constant while it ought to be variable, or something of that sort' (Frisch to Nelson, 13 March 1933). An end was soon brought to this injustice and Frisch moved quickly to correct his own error of judgement.

18 Sraffa, unhappy at not being elected a Fellow, withdrew from the Econometric Society.

19 Day to Mills, 11 January 1932, Mills to Day, 15 January 1932. Mills strongly emphasises that the founders of econometrics – and he named Frisch, Roos and Evans – considered 'mathematical economics [as] a discipline quite different from traditional economics of a nonmathematical type', although

> I am personally agnostic concerning the possibilities of substantial accomplishment in this field by technicians who are not closely conversant with the actual economic process. Probably more important is the danger that mathematicians who are interested in economics may cultivate their art in isolation and that an esoteric, unrealistic discipline may develop.

20 Schumpeter was supposed to deliver the opening address plus a lecture on 'The place of innovation in business cycle theory', according to the programme proposed by Frisch (Frisch to Schumpeter, 24 July 1931). Considering the discussion they were having on this very topic (see Chapter 6), this lecture was certainly very important. But Schumpeter was finally unable to attend.

21 Frisch did not attend the first US conference: 'As the father of this promising offspring you will be sorely missed' (Mills to Frisch, 5 October 1931). Indeed, after 1931, he concentrated on his European work.

22 Frisch to Schumpeter, 11 January 1933.

23 Frisch to Fisher, 1 December 1932.

24 Frisch to Fisher, 6 September 1933.

25 In one particular instance, Frisch rejected Divisia's suggestion to meet the econometric conference as part of a congress of mathematicians:

> Then there is the argument that there is some danger in making the Econometric Society too exclusively mathematical. We must not forget that our first object is economic theory. Statistics and mathematics are only used as a means of furthering the main object.
>
> (Frisch to Divisia, 31 March 1932)

Frisch certainly considered Divisia's previous recognition of his own lack of preparation in mathematics.

26 The plot was to have Mitchell (president) and Amoroso (vice-president) in 1937; in 1938, Amoroso and Hotelling; in 1939, Colson and Wilson; in 1940, Wilson and Keynes; in 1941, Keynes and Roos; in 1942, Schumpeter and Zeuthen; in 1943, Zeuthen and Evans; in 1944, Evans and Frisch; in 1945, Frisch and Hotelling (Roos to Frisch, 21 May 1935). Divisia was not supposed to play any role. Cowles was another econometrician trying to prevent Divisia's election, besides Roos and Frisch.

27 Frisch to Schumpeter, 13 March 1933.
28 Frisch to Schumpeter, 25 October 1933.
29 Frisch to Schumpeter, 15 October 1935 and 22 November 1935.
30 Letters written in 1927, undated May–June and 4 July; letter from Frisch to Divisia, 22 May 1927. By 1928, the name *Econometrica* had become stabilised. But some references were still made to the 'econommetric' circles.
31 Roos to Schumpeter, 25 November 1935.
32 Alfred Cowles III was an investment counsellor in Colorado and had developed statistical research into stock market forecasting after the Great Depression. His main interest in econometrics arose from the bad record of stock predictors. His 1932 paper entitled 'Can Stock Market Forecasters Forecast?' examined three years of records kept by twenty-four leading financial services, including banks and investment companies, and he concluded that 'as a group, these supposedly shrewd investors would have accomplished a comparable result through a purely random selection of stocks' (Cowles to Frisch, 10 March 1932). The paper was discussed with Frisch and presented to the meeting of the American Statistical Association (Cincinnati, December, 1932).
33 Fisher to Frisch, 18 October 1931. Roos was also enthusiastic. The term 'angel' was certainly widespread in econometric circles: Mills refers to an 'angel' Fisher had found in Denver (Mills to Day, 15 January 1932).
34 Frisch to Schumpeter, 11 January 1933.
35 Frisch to Leavens, 17 January 1940.
36 The National Library of Oslo contains a collection of Frisch's correspondence that highlights his efforts to guide the development of the econometric movement and the journal: 175 letters from Alfred Cowles, seventy-eight from Divisia, 111 from Fisher, 295 from Leavens, forty-two from Marshack, thirty from Hotelling, 131 from Nelson, twenty-seven from Roos, thirty-three from Schultz, twenty-five from Hansen, sixteen from Gini, thirteen from Haavelmo, thirty-four from Khan, twelve from Keynes, fifteen from Kalecki, twelve from Koopmans, seventeen from Morgenstern, thirty-six from Schumpeter, fifty-one from Tinbergen, as well as letters to and from many other protagonists in the history of economics in the twentieth century.
37 When the managing editor, Leavens, left his job in 1948 after eleven years, he was bitterly critical of Frisch's editorship (Leavens to Frisch, 24 August 1948).
38 Sometimes, Frisch notified the authors of his decision to add his own footnote to their papers. For instance, Frisch told Roos he would add a footnote to his paper on 'Theoretical Studies of Demand', mentioning his own work and Marschak's: 'This seems to be exactly in line with the other things you speak of, but if you would rather not refer to it in this connection, cross it out when you get the galley proofs' (Frisch to Roos, 30 June 1933).
39 Hotelling to Frisch, 26 May 1938.
40 Neyman to Frisch, 4 March 1938.
41 Frisch to Neyman, 7 March 1938.
42 Frisch to Schumpeter, 28 March 1938.
43 Roos to Frisch, 19 February 1935.
44 This is the reason for Roos's reference to his own hostility to equilibrium models; from that point of view, he was closer to Frisch's approach than most of the other commentators on the paper.
45 Frisch to Roos, 21 March 1935.
46 Roos to Frisch, 26 April 1935.
47 Cowles to Schumpeter, 20 February 1940.
48 Schumpeter to Cowles, 27 February 1940.
49 The editorial policy of *EJ* was clearly stated in 1954: 'We suggest that authors should aim at avoiding the use of advanced mathematics, except where it is necessary for supplying a rigorous proof, or where the nature of the subject inevitably requires it', in order to avoid the creation of 'language barriers' (Editor's note, *Economic Journal*, March 1954, 64 (253): 2). From 1900 through to 1960, the percentage of mathematical papers published in the *EJ* was very low (5 per cent) and in the period 1887–1924, 39 per cent of such papers were produced by Edgeworth, the editor (Mirowski, 1991: 150).

50 Frisch to Nelson, 27 January 1934.
51 Frisch to Bousquet, 23 March 1934.
52 Schumpeter accused Snyder, a statistician at Federal Reserve Bank of New York, of not 'knowing an integral from a ratio'. Mitchell was certainly hostile to most of the econometric programme.
53 The Commission decided to publish monographs on econometrics, beginning with Frisch's 'Changing Harmonics Studied from the Point of View of Linear Operators and Erratic Shocks' (Frisch to Fisher, 4 January 1934).
54 For instance, in 1937, Frisch suggested a research project on 'economic control' – or planning – investigating the behaviour of a simultaneous equations system and using for that purpose either experiments or inquiries (Frisch to Cowles, 11 July 1937). More will be said on this subject later on (Chapter 11).
55 Marschak asked for $8,000 a year, Cowles paid only 6,000.
56 Cowles to Frisch, 6 May 1938.
57 Frisch to Bresciani-Turroni, 21 May 1938.
58 Frisch to Tinbergen, 17 December 1936.
59 Schumpeter's attitude was remarked by the young Kenneth Arrow, since he 'treated the whole matter with the benevolent condescension of a lord among well-meaning and deserving but necessarily limited peasants' (Arrow, 1978: 71).

3 The years of high theory

1 Erhenfest to Schumpeter, 2 May 1918, quoted by Jolink (2003: 27).
2 The history of the eugenic movement is outside the scope of this book. It is sufficient here to indicate that, in the first years of the twentieth century, the movement was divided between the dominant British views and what later came to be Nazism, although the contours of such differentiation were not quite as sharp by then as they came to be later on when the Nazi policy of genocide was at work.
3 Ernst Mach (1838–1916) was an Austrian physicist and philosopher who, apart from his work on optics and mechanics, propounded a theory according to which knowledge is merely a conceptual organisation of sensory impressions. As Porter noticed, there was an intense discussion about this theory and a curious feature of its incidence in Russia was the objection raised by Vladimir Ulyanov, alias Lenin, polemicising against Mach and challenging Pearson's endorsement of his idealism (Lenin, 1908: 50–1). Pearson was also a follower of Maxwell in relation to analytical methods, and of the Norwegian Carl Anton Bjerknes (1825–1903), a mining engineer, who had studied mathematics in Paris and concentrated his research in the field of hydrodynamics.
4 A couple of years before his confrontation with Pearson, Keynes wrote that

> I regard Pearson as primarily a statistician and in that he is eminent; but his kind of statistics has to rest on some basis of probability, and my complaint against him is that he can give no clear account of the logical part and indeed knows very little about it. His mathematics is excellent and doubtless proves something, but whether he proves quite what he thinks it proves I rather doubt.
>
> (Keynes to W.H. Macaulay, 30 August 1907)

5 All through their lives, the confrontation between both men was noticeable. But still many years after the disappearance of his opponent, Fisher still wrote that Karl Pearson's mathematics was 'clumsy' and that he was 'unwilling to correct his numerous errors or to appreciate the work of others' (Fisher, 1951: 35).

6 Karl Pearson pulled whatever strings he could to prevent the appointment of his rival. He was supported by friends: Raymond Pearl wrote to Pearson saying that R.A. Fisher, that 'lousy scoundrel', could not be allowed to succeed him (Porter, 2004: 313). Instead, Fisher commented on the outcome to his close friend Harold Hotelling: 'The situation will be rather a comic one, and I am really sorry it has been such a blow to my predecessor, who seems to have worked with desperate anxiety to avoid what has happened' (Fisher to Hotelling, 14 August 1933).

In any case, the previous conflict with Karl Pearson was reproduced with his son Egon, who apparently had proposed a restriction so that Fisher could not teach his own theory of estimation, which he rejected (Fisher to Egon Pearson, 2 June 1933). Fisher had proposed a gentlemen's agreement so that they try to avoid the 'impression of antagonism between the two departments' (Fisher to Egon Pearson, 27 May 1933). It was worthless: when both teams were at work, the 'impression of antagonism' was simply avoided by the fact that, not talking to each other, they occupied separate floors in the same building and had tea at 4pm (Fisher) and at 4.30 (Egon Pearson), as Neyman witnessed (Reid, 1998: 113–14). Yet, Egon Pearson respected R.A. Fisher and even considered that he was right in some of the disputes against his father.

7 In a letter to Henry Schultz, Fisher explained his divergence with Karl Pearson, who did not consider the importance of the Gaussian tradition and consequently attacked the method of least squares, unlike himself (Fisher to Schultz, 19 April 1937). Pearson had already died the previous year.

8 R.A. Fisher to Fréchet, 14 March 1934.

9 R.A. Fisher to Schultz, 30 March 1936.

10 Frisch visited R.A. Fisher in 1934. Koopmans sought Fisher's approval of the book he produced in 1937 on probability, which he was immediately given (R.A. Fisher to Koopmans, 26 October 1937).

11 Egon Pearson began his research career in Cambridge studying solar physics and the theory of errors, first with Eddington and then with Yule.

12 Hereafter, I follow the convention of referring to Keynes's works simply as *TP* (*Treatise on Probability*), *GT* (*General Theory*), etc., and identify the volumes of the collected works edited by D. Moggridge (1971–89) by their Roman numerals.

13

> Mathematical economists often exercise an excessive fascination and influence. . . . [They] introduce the student, on a small scale, to the delights of perceiving constructions of pure form, and place toy bricks in his hand so that he can manipulate for himself, which gives a new thrill to those who have had no glimpse of the sky-scraping architecture and minutely embellished monuments of modern mathematics.
>
> (Keynes, X: 186n.)

And in *GT* he wrote, in a very similar style to the one that Marshall had once used: 'I do not myself attach much value to manipulation of this kind [formal models]. . . . I doubt if they carry us any further than ordinary discourse can' (*GT*: 305).

14

> The orthodox equilibrium theory of economics has assumed . . . that there are natural forces tending to bring the volume of the community's output, and hence its real income, back to the optimum level whenever temporary forces have led it to depart from this level. But we have seen . . . that the equilibrium level, towards which output tends to return after temporary disturbances is not necessarily the optimum level, but depends on the forces in the community which tend towards savings.
>
> (Keynes, XIII: 406)

15 Joan Robinson defined the nature of the Keynesian way of incorporating history into economics:

> The *GT* broke through the unnatural barrier and brought history and theory together again. But for theorists the descent into time has not been easy. After twenty years, the awakened Princess is still dazed and groggy. Keynes himself was not quite steady on his feet. His remark about the timeless multiplier is highly suspicious. And the hard core of the analysis . . . is based upon comparisons of static short term equilibrium positions each with a given state of investment going on, though it purports to trace the effect of a change in the rate of investment taking place at a moment in time.
>
> (Robinson, 1962a: 78)

16 Keynes to Frisch, 10 February 1932. Frisch had submitted the paper on 26 January 1932 – a rather rapid response. One week later, Frisch agreed to prepare a new version of the paper, but later on announced it was to be published elsewhere (Frisch to Keynes, 3 March 1932).

17 Keynes to Frisch, 13 February 1935.

18 Frisch sent the paper on 15 October 1935, and was informed of its rejection on 28 November.

19 Frisch to Keynes, 14 December 1935; Keynes to Frisch, 30 December 1935; and Frisch's reply, 4 January 1936.

20 Marshall fought against this conception, in a letter to Clark:

> What I take to be the Static state is . . . a position of rest due to the equivalence of opposing forces which tend to produce motion. I cannot conceive of any such Static state, which resembles the real world closely enough to form a subject of profitable study, and in which the notion of change is set aside even for an instant.
>
> (letter written in 1902, in Marshall, 1925: 415)

Later on, Marshall again criticised Clark's definitions, since 'an exclusive study of purely statical conditions must be unsatisfactory' and Clark's attempt to isolate static forces was doomed to failure (1907 preface to Marshall, 1890: 51–2). In a note to the argument, Marshall added that the separation between static and dynamic forces could only be accepted for short-term analysis and then only for 'illustrative purposes' (ibid.).

J.M. Clark wrote in 1927 an essay on his father's contributions and argued – very institutionalistically – that the solution of departing from static conditions and later on adding some dynamic premises was incoherent, and that a whole new theory was needed for the qualitative or 'chemical' change implied by dynamics, since society should be defined as an 'organic whole' (J.M. Clark, 1927: 46–7, 68–9). J.B. Clark had argued since 1899 that the economy is like an 'organism' (Clark, 1899: 196), but was inconsequential in relation to the development of that concept.

21 See, for instance, Blaug (1986: 51).

22 Many years later, in *History of Economic Analysis* (HEA), Schumpeter defended Clark against the accusation that he was an apologist of capitalism, saying that the marginalist theory did not imply any social philosophy whatsoever (*HEA*: 869–70). This is openly contradictory of Schumpeter's 1906 review. In her study on Clark, Mary Morgan argues that he was a 'Christian of socialist leanings' (Morgan, 1994: 231), openly opposed to laissez-faire capitalism (ibid.: 236), who considered that the moral problem of distribution could be solved by marginalist economics (ibid.: 237–8).

23 In any case, by that time, Fisher's prestige was already on the decline, in part due to his failure to anticipate the 1929 Crash and his own personal misfortune in stock market operations. In a letter to Frisch, Fisher invited him to buy some of his stock in Sonotone Corporation, arguing that he had just sold \$20,000 worth of stock to Cowles Sr and \$2,000 worth to his son. The deal was presented as a compensation for

Frisch's previous acceptance of another of his friend's recommendations (Fisher to Frisch, 27 May 1933). There is no indication about what finally happened in this case, but Frisch carefully kept a cutting from the *New York Times* (10 February 1937), involving Fisher, a director of Automatic Signals, in the trial of two former Yale students, accused of a $1,000,000 fraud.

24 Alfred Cowles sent a copy of his paper to Frisch (31 July 1933).

25 I have already discussed elsewhere the role of metaphors in science (Louçã, 1997: 49f.).

26

> It is not by chance that these engineers have been attracted to economics, because both engineering and economics must deal with the problem of how to combine limited resources to achieve a given end, and must consequently make use of the principle of economy.
>
> (ibid.)

27 Mitchell to Frisch, 12 August 1930.

28 This gave him a very peculiar position in Austrian economics. His 1908 book had emphasised the importance of Pareto and Walras and Schumpeter's distance in relation to Austrian economics (Witt, 1993: xiii). Witt explained these features through his early wish to obtain a specific standing: 'It is no secret, of course, that Schumpeter wanted to achieve a standing of his own and thus tended to distance himself from standard Austrian positions from the very beginning' (Witt, 1995: 84). Nevertheless, he was clearly on the 'theoretical' or marginalist side: in 1906, Schumpeter published his first two papers along those lines. One was a paper on the role of mathematics ('pure theory') in economics, in which he approvingly quoted Jevons: 'If Economics is to be a science at all, it must be a mathematical one' (in Allen, 1991, I: 56).

29 Hereafter, Schumpeter's books are indicated, for the sake of simplicity, as *DW* (1908, *Das Wesen und der Hauptinthalt der Theoretischen Nationalokonomik*), *TED* (1911, *Theory of Economic Development*, using the revised edition of 1926), *EDM* (1914, *Economic Doctrine and Method: An Historical Sketch*), *BC* (1939, *Business Cycles*), *CSD* (1942, *Capitalism, Socialism and Democracy*), *HEA* (1954, *History of Economic Analysis*, posthumous) and *TGE* (1990, *Ten Great Economists: From Marx to Keynes*, posthumous reprint of essays).

30 The criticism did not challenge the importance of the physical analogies, but rather their general implications, which Schumpeter feared could launch a new and useless *Methodenstreit*: 'on those few and well-timed occasions when he is looking for formal analogy to the procedure of physical science, he seems to overstate the importance of the experimental, and to understate the importance of the theoretical side of their work' (Schumpeter, 1930: 152).

31 Hayek fought against the 'imitation of physics' leading to 'outright error' and transforming sciences into 'cooking recipes', arguing that quantification arbitrarily restricted the domain of causation and ignored some decisive features of economies:

> This brings me to the crucial issue. Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which we can get quantitative data are necessarily limited and may not include the important ones.
>
> (Hayek, 1978: 23, 30, 24)

32

> As regards the question of principle, there cannot be the slightest doubt that Hayek is right . . . in holding that the borrowing by economists of any method on the sole ground that it has been successful somewhere else is inadmissible. . . . Unfortunately this is not the real question. We have to ask what constitutes 'borrowing' before we can proceed to ask what constitutes illegitimate borrowing.
>
> [. . .] Similarly, the concepts and procedures of 'higher' mathematics have indeed been first developed in connection with the physicist's problems, but this does not mean that there is anything specifically 'physicalist' about this particular kind of language. But it also holds for some of the general concepts of physics, such as equilibrium potential or oscillator, or statics and dynamics, which turn up of themselves in economic analysis just as do systems of equations: what we borrow when we use, for example, the concept of an 'oscillator' is a word and nothing else.
>
> (*HEA*: 17–18)

33 In another place in *HEA*, Schumpeter again argued:

> Finally, the reader should also observe that the conceptual devices sketched have nothing to do with any similar ones that may be in use in the physical sciences.
>
> [. . .] Since physics and mechanics in particular were so much ahead of economics in matters of technique, these conceptual devices were consciously defined by physicists before they were by economists so that the average educated person knows them from mechanics before he makes the acquaintance in economics, and hence is apt to suspect that they were illegitimately borrowed from mechanics. Second, such devices being unfamiliar in a field where a looser conceptualisation prevailed, some economists, I. Fisher in particular, thought it a good idea to convey their meaning to the untutored mind by way of the mechanical analogy. But this is all.
>
> (*HEA*: 965)

Is this in fact all?

34 In a letter written to Oskar Lange in February 1937, Schumpeter bitterly complained against this turn of events, since Keynes was said to have missed everything in economics from the 1830s:

> The book could have been written a hundred years ago and skirts all real problems. It is the reverse of progressive . . . it is the dying voice of the bourgeoisie calling out in the wilderness for prophets it does not dare fight for and shifts its ego to the real problems it does not face.
>
> (quoted in Allen, 1991, II: 26)

In her introduction to *TGE*, Schumpeter's widow, Elizabeth, wrote that she could not understand the professional and personal distance between the two scientists (*TGE*: 15). So, no explanation was given for this outstanding fact. Smithies gave no interpretation (Smithies, 1951), and Heilbroner argued that they had different cultural backgrounds and scientific interests (Heilbroner, 1986). Allen writes that Schumpeter was always extremely generous to all his colleagues when criticising their work but that the obvious and extraordinary exception was his review of *GT*, the 'strongest he ever wrote' (Allen, 1991, I: 58, 1991, II: 24).

35 Kondratiev's ideas had a greater impact since his papers were soon (partially) translated and frequently discussed in broader scientific circles. But the Russian debate on his work was almost completely ignored. For a long time, Garvy's 1943 paper has been the most precise and complete source of reference for this debate, but it is a somewhat biased summary of the arguments.

36 Kondratiev criticised Schumpeter's confusion, in his 1911 book, as being between the static mode of analysis and the claim that the processes he described were static by nature (1924: 11). He anticipated Frisch's critique, which was later accepted by Schumpeter himself. Apparently, Schumpeter never read this argument by Kondratiev on this topic, nor did Frisch. Incidentally, the 1924 paper proves that Kondratiev had an impressive knowledge of the literature on macroeconomic cycles: Jevons, Walras, Pareto, Clark, Marshall, Wicksell, Juglar, Tugan-Baranowsky, Spiethoff, Lescure, Aftalion, Mitchell and Schumpeter were all quoted.

37 This he represented with his curve-fitting methods, generally using nine-year moving-

averages and exponential or other curves. But Kondratiev was not happy with his methods and, when he was imprisoned by the Stalinist regime, he prepared and posted to his wife the plan for a five-volume work that would include discussions on statics and dynamics, as well as on methods for the study of the social sciences, Long Waves and other matters. He could not finish most of these papers, since he was assassinated after a show trial and eight years in prison. Some of these papers by Kondratiev were only published in Russian in the 1990s.

38 The text included references to and quotations from Clark, Bowley, Babson, Jevons, Tugan-Baranowsky, Beveridge, Schmoller, Cournot, List, Marshall, Mill, Marx, Pareto, Persons, Durkheim, Mach, Poincaré, Meyerson, Comte, Simmel, Laplace, Boltzmann and Plank.

39 Schumpeter took pains to convince Frederick Mills, a NBER investigator (Mills to Schumpeter, 12 April 1940). In 1942, Frickey published a book that included an important argument against trend decomposition and argued that the secular trend should be assessed as 'a problem in historical description' and not as 'a problem in mathematical curve fitting'. He demonstrated that the fit of different functions could imply arbitrarily created cycles and therefore spurious results. His conclusions from American data were presented as compatible with Kondratiev's hypothesis (Frickey, 1942: 8, 231fn., 232, 340).

In the 1940s, another researcher taught the Kondratiev thesis at the LSE: W.W. Rostow (1948: 9, 29, 45). Others, such as the very young Richard Goodwin, learnt it from Schumpeter and later divulged it to the others. In 1949, Fellner prepared a manuscript, which was discussed with Schumpeter, entitled 'On the Waves of Different Lengths with Particular Reference to the Long Waves' (Fellner to Schumpeter, 26 March 1949), arguing that innovations accounting for the next Kondratiev waves were predominantly generated by military investment. Later on, in his 1956 book, Fellner took up the issue again, presenting Kondratiev's statistical methods, and inspecting a certain number of empirical series: his conclusions indicated the acceptance of long rhythms, but as irregular features of development. As a consequence, 'we prefer not to assert the existence of long cycles of fifty years', since 'the so-called long cycles in general economic activity are merely alternations between intermediate trends of greater and of lesser steepness' (Fellner, 1956: 38, 40–1, 42, 49).

40 Frisch to Slutsky, undated letter written in 1927.

41 Frisch to Schultz, 22 February 1932.

42 Schultz to Frisch, 9 March 1932.

43 Frisch to Schultz, 31 March 1932. By December 1934, Frisch had confirmed this to Slutsky (Frisch to Slutsky, 14 December 1934).

44 Schultz to Frisch, 19 April 1932; 7 January 1933; and 4 March 1935. The costly section added in 1935 was indeed a new paper published in Russian in 1929 on the standard error of correlation coefficients applied to a random series (Barnett, 2006: 2).

45 Frisch to Slutsky, 20 June 1936.

46 Frisch to Nelson, 22 August 1932.

4 What counts is what can be counted

1 Yet there were later afterthoughts on this extreme positivism: Einstein argued instead that 'not everything that counts can be counted and not everything that can be counted counts' (I owe this reference to the epigraph of the interesting Jolink's 2003 book on Tinbergen).

2 Apparently, Frisch was not very certain of this measurability of psychological motivations and actions, since he added that this was only possible given empirical regularity – but this passage was crossed out in the manuscript. Later on, he frequently insisted on the importance of psychological and social information to be obtained from interviews, in order to calibrate the models.

3 Fisher sent Frisch a paper to *Econometrica*, 'The Debt-Deflation Theory of the Great Depression' (7 May 1933, published as Fisher 1933b) stating that cycle theory was concerned only with disequilibrium:

> Only in imagination can all these variables remain constant and be kept in equilibrium by the balanced forces of human desires, as manifested through 'supply and demand'. Economic theory includes a study both of (a) such imaginary, or ideal, equilibrium – which may be stable or unstable, and (b) disequilibrium. The former is economic statics; the latter, economic dynamics. So-called cycle theory is merely one branch of the study of economic disequilibrium.

This attitude is even more revealing if one remembers that Fisher was the father of US neoclassical economics and his revered Ph.D. dissertation had established the map for the translation of the concepts of physics into economics in order to impose the analogy with the First Law of Thermodynamics.

4 One example is given by an early paper that Frisch prepared with Waugh, which was published in 1933. The authors castigated by the then common error of distinguishing between two methods for correlation analysis, the estimation of deviations from trend and the regression with time as a variable, since both were methodologically equivalent and produced similar results (Frisch and Waugh, 1933: 387–8). The paper simply provides technical advice to fellow econometricians but, even in this rather trivial case, Frisch considered possible complications, such as the fact that the variable under consideration contains a linear trend plus random irregularities plus an independent nonlinear component (ibid.: 391). He chose not to discuss further such a possibility in the paper, since he could not deal with the effect of the nonlinear component, but nonetheless indicated the difficulty – Frisch strove throughout his life to produce another mathematics capable of incorporating nonlinearities both for estimation and for simulation.

5 Marschak's attitude was not clear at all. In a letter to Frisch, he considered that a high correlation between prices and quantities could prove to be not a stable demand–supply structure but a correlation between shifts in both curves, due to cycles and trends (Marschak to Frisch, 3 February 1935). In any case, Frisch understood this and other attitudes as endorsing his own point of view, considering what Schultz, Working, Keynes and Marschak had written (Frisch to Schumpeter, 19 November 1934).

6 Other economists underestimated this debate. This was the case with Keynes, who essentially mistrusted statistical methods. He rejected the 'Pitfalls' paper for the *Economic Journal*, doubting its statistical conclusions, and offered to publish a simpler paper: 'I should like, if I could, to act as a midwife between your ideas and the average economist' (Keynes to Frisch, 10 February 1932). Frisch was never able to publish anything in the *EJ*.

7 Schumpeter to Frisch, 2 November 1934.

8 This friendly relationship would continue throughout their collaboration: 'I have never met a person with your ability to and eagerness to understand the other fellow's point of view and to do him justice' (Frisch to Schumpeter, 13 October 1939).

9 Frisch to Schumpeter, 19 November 1934.

10 Schumpeter to Frisch, 14 January 1935.

11 Frisch to Lundberg, 15 August 1970.

12 Divisia to Frisch, 7 August 1931.

13 Amoroso to Frisch, 16 October 1931.

14 Frisch to Amoroso, 19 October 1931.

15 Frisch to Amoroso, 4 January 1934.

16 There are some severe shortcomings in this story, since D'Avenel's series is merely an average of eclectic local observations and the meaning and coherence of the series itself is at best doubtful. But this did not prevent Frisch's acceptance and profound belief in this interpretation.

17 The correlation between business cycles and wars that Frisch found, must, I believe, either have been a statistical artefact produced by Frisch's special statistical decomposition techniques, or perhaps explained by wars giving rise to high wheat-prices.

> [. . .] Frisch's work in this area was, however, too far-fetched to be worthy of any serious consideration. It only confirms one's impression that the statistical decomposition ideas of Frisch were not fruitful. The profession has probably lost little by putting them aside.
>
> (Andvig, 1995a: 297 fn.)

5 Particles or humans? Paradoxes of mechanics

1 The interpretation of the Laws of Thermodynamics is not unambiguously legislated, as one author rightly emphasised:

> I offer here a crisp summation by the chemist P.W. Atkins, just to provide a sense of them: 'There are four Laws. The third of them, the Second Law, was recognized first; the first, the Zeroth Law, was formulated last; the First Law was second; the Third Law might not even be a law in the same sense of the others.' In briefest terms, the second law states that a little energy is always wasted. You can't have a perpetual motion device because no matter how efficient, it will always lose energy and eventually run down. The first law says that you can't create energy and the third that you can't reduce temperatures to absolute zero; there will always be some residual warmth. As Dennis Overbye notes, the three principal laws are sometimes expressed jocularly as (1) you can't win, (2) you can't break even, and (3) you can't get out of the game.
>
> (Bryson, 2004: 107fn.)

2 In 1929, Frisch suggested, to no avail, a new conceptual division: statics, dynamics and 'kinematics' for national accounting (Andvig, 1986: 35). Later on, he sought to reject the statics/dynamics antinomy as a description of the universe, since it was defined in the domain of methodology; instead, reality is 'stationary' or 'evolutionary'. These antinomies were accepted by Schumpeter.

3 To be more precise, statics itself was defined by Frisch as a counter-factual: 'The variations addressed by the static law are by definition not real variations in time, but formal variations which occur when we compare certain well defined situations which we imagine are realised alternatively' (Frisch, 1929: 391).

4 Frisch to Fréchet, 6 December 1933.

5 The same letter goes on:

> Therefore if no errors were present any magnitude of the correlation coefficient different from zero would be significant, but if errors are present the magnitude of the correlation coefficient that must exist in order to indicate a significant difference in phase will of course depend essentially on the intensity of the errors. In the case of time series some information about this intensity may be obtained simply from the plot of the curves, and thus some notion can be obtained about the size of the correlation coefficient that is necessary in order to indicate a significant difference in phase, that is to say a significant deviation from linearity. Of course, quite similar considerations may be applied to the case where the deviation from linearity is not due to a difference in phase between cyclical curves, but say, to the fact that the regression between the two variables is a curved line (for instance a parabola).
>
> (ibid.)

Frisch struggled with the nature of errors and was unable either to accept the prevailing interpretation or to provide an alternative. Schumpeter considered this problem to be common both to physics and to economics:

> It is indeed with us exactly as it is in Physics. We must construct theories as physicists construct them (compare, for instance, Bohr's model atom) which can hardly ever be fully verified or refuted in a material exposed to too many chance disturbances.
>
> (Schumpeter to Snyder, 16 December 1932)

More will be said about this later on (Chapter 8).

6 Frisch to Mitchell, 8 December 1930.
7 Frisch to Schumpeter, 5 July 1931.
8 Frisch to Hotelling, 1 April 1934.
9 Frisch to Tinbergen, 24 October 1934.
10 Frisch to Creedy, 30 October 1934.
11 Frisch to Roos, 16 December 1934.
12 Frisch to Creedy, 27 June 1935.
13 Tinbergen to Frisch, 24 December 1934.
14 Frisch to Tinbergen, 11 January 1935.

15 Davis was a mathematician who was very influential during the first period of the Econometric Society and Cowles Commission. He fully endorsed and used the physical analogies: for instance, he prepared a paper on 'The Perturbation Problem for Economic Series', presented to the Econometric Conference in June 1932, arguing that the 'physical analogy which first occasioned the present point of view' was the method to find the 'molecular spectrum of methane CH4 by means of the theory of elastic perturbations of the atoms'. Frisch surveyed this paper, but did not challenge the analogy.

16 Marschak wrote to Schumpeter: 'I am anxious to know what you think of my suggestion that someone should tell us of so-called statistical physics or astronomy, and its analogies in economics' (Marschak to Schumpeter, 5 August 1946).

17 On Tinbergen's 'limited transfer' from physics to economics, see Boumans (1992). On Roos, see the Creedy example. On other relevant differences between the members of this group, see Chapter 10.

18 Other members of the econometric circles shared the same antagonism in relation to mechanical analogies. For instance, Joseph Mayer sent Frisch the plan for a book entitled 'Pseudo-scientific Economic Thought', including a chapter on 'Pseudo Analogies: Mechanistic and Organismic Fallacies' (Mayer to Frisch, 4 April 1933).

19 Nicholas Bourbaki (1816–97) was an obscure French general, whose name was used as the alias for a group of young mathematicians engaged in promoting the prevalence of the axiomatic method and the reconfiguration of mathematics. The Bourbakist movement had an immense influence on the development of mathematics and namely on its applications to economic theory. Gerard Debreu, for one, went to the US under this spell and became one of its most successful proselytisers.

6 Intriguing pendula: delights and dangers of econometric conversation

1 Frisch's letters were clearly typewritten, whereas Schumpeter's were handwritten and are in a very poor condition (some of the words are quite difficult to decipher, and whenever this was not possible they are marked as [.]). The complete collection is obtained from the compilation of the correspondence available at Harvard University Library and the Archives of the National Library and Oslo University. Some of the letters from this period are apparently lost, since they could not be found either at Harvard or in the Oslo Collections. It is known that Schumpeter used to tear apart many of the letters he received, using the pieces of paper for notes.

2 References to Frisch's 1933 paper can be found in Schumpeter's *Business Cycles* (171fn., 181fn., 189) and in 'History of Economic Analysis' (1162fn.). Schumpeter never made any direct criticism of the paper in public, although he discussed its major features in private letters, as we shall see.

3 Louçã, 1999.
4 This crucial meeting for the formation of the Econometric Society took place on 29 February 1928, at the Colonial Club at Harvard (see Chapter 2).

5

> By a changing harmonic I understand a curve that is moving more or less regularly in cycles, the length of the period and also the amplitude being to some extent variable, these variations taking place, however, within such limits that it is reasonable to speak of an *average* period and an *average* amplitude.
>
> (Frisch, 1933a: 202)

6 Frisch to Mitchell, 8 December 1930. Interference phenomena were the main concern of Frisch at this time:

> The technique which is now most in vogue does not seem powerful enough to deal with the more complicated situations which arise when the time series studied represents an interference phenomenon between several components: short cycles, long cycles, different orders of trends, etc., and when, furthermore, the cyclical or progressive characteristics of these various components are changing.
>
> (Frisch, 1930: 73)

7 This is the first letter dispensing with formal treatment between Schumpeter and Frisch. It was sent from San Francisco.
8 To the best of my knowledge, this is the only letter quoted here that has already been partly reproduced elsewhere (Stolper, 1994: 70f.).
9 Fernão de Magalhães (1480–1521) was a Portuguese navigator who led the first expedition circumnavigating the world, although he was killed before it returned home. In his reply, Schumpeter spells the name as in the original language.
10 At that meeting, Tinbergen presented several models of endogenous and regular cycles. Frisch was, of course, much closer to the subsequent solution that would come to be accepted as the pattern of cycle models, which Tinbergen also later adopted. But both had long shared the same fascination for Aftalion's explanation of the cycle as the result of lags in the production of capital goods.
11 This difference in conceptualisation already suggests their alternative approaches: invention could eventually be considered as exogenous and as part of the scientific system, whereas innovation was precisely described as the result of the market selection process of invention, i.e. of the specific economic system. Innovation could never be described as a purely exogenous variable in the Schumpeterian model.
12 Samuelson, who knew Frisch and was a student and later a young colleague of Schumpeter's, has left us with an interesting testimony to their relationship and to Schumpeter's inability to deal with mathematics:

> I remember how my old teacher Schumpeter, perhaps Frisch's most fervent admirer, marvelled at the miracle that imaginary numbers could drive 'real' alternating current and 'actual' business cycles. I don't think he ever really understood that the complex number system is merely a convenient way of handling the algebra of the real sinusoidal solutions.
>
> (Samuelson, 1974: 8–9)

13 Frisch indicated in his fourth lecture that, at that time, an assistant in Oslo was concluding the computations for PPIP.
14 Frisch had obtained a copy of a letter from van der Pol to a colleague, Arnold Rostad, sending a paper on the heartbeat as a relaxation oscillation. Van der Pol wondered about the possible application of this concept to 'the periodical return of economical crises' (Van der Pol to Rostad, 25 October 1928). A copy of the letter was forwarded to Frisch.

15 In the same lecture, Frisch explained to the audience how the secular trend could be included, just as he had suggested the same method to Schumpeter:

> We may simply imagine that the pivot which suspends the device is not fixed, but that it can be moved through a fissure in the wall. The fissure is going up to the right. Consequently, the whole device will be moved by jumps, each one leading it to a higher place in the fissure. It is sufficient to feed the device by a current of water, then the impulse of the descent of that water is the cause for impulsion and it explains as well the elevation of the device itself. There will be an intense attraction between irreversible evolution and oscillations.
>
> (1933j: 45)

16 A note by Schumpeter emphasised the evolutionary character of economic data and therefore strengthened his critique:

> The simile limps, of course, like all similes. Cycles run their course in the *historical* evolution of the capitalist economy. Even neglecting all the economic sociology that must therefore inevitably enter into their explanation, we cannot help recognizing that their theory or, to avoid this word, their analysis must be largely bound up with the theory or analysis of evolution rather than with dynamics, which is the theory or analysis of sequences that do not carry any *historical* dates. No doubt there are certain mechanisms that played as great a part in 1857 as in 1929. And these must be taken account of in any observed cycle by more or less generally applicable macrodynamic schemata, just as must, on a lower level of technique, the ordinary theory of supply and demand. But they are only tools and do not in themselves suffice, even if supplied with all conceivable time series, to reconstruct the phenomenon as a whole and, of course, still less its long-run outcomes.
>
> (ibid.: 1167fn.)

17 Again, Schumpeter's second footnote to the same text is very revealing:

> Three of these may serve as illustration. They will at the same time show why the respective objections do not tell against the models themselves but only against the claim alluded to. (1) Macrodynamic models, presented with that claim, involve the proposition that the 'causes' of the business cycles must be found in the interaction between the social aggregates themselves, whereas it can be proved that business cycles arise from sectional disturbances. (2) With the same proviso, macrodynamic models carry the implication that the structural changes that transform economics historically have nothing to do with business cycles, whereas *it can be proved that cycles are the form that structural changes take*. (3) Constructors of macrodynamic models, almost always, aim at explaining all the phases of the cycle (and the turning points) by a single 'final' equation. This is indeed not impossible. But it spells error to assume that it must be possible and to bend analysis to that requirement.
>
> (ibid.: 1168fn.; my italics)

18 Frisch preferred a mixed system, such as that used in 1933. The use of difference equations by Tinbergen in his later work for the League of Nations became the subject of an important debate between the two friends and colleagues (Boumans, 1995; the texts of the polemics were reproduced in Hendry and Morgan, 1995: 407–23).
19 Slutsky sent the paper to Yule (Slutsky to Yule, 1 July 1927) and then to Frisch (Slutsky to Frisch, 4 July 1927), whom he had met before in Oslo (Bjerkholt, 1995: 7). Slutsky explained to Frisch:

> There are three distinct ideas in my paper. The first is that every series of intercorrelated quantities must show the quasi-periodical fluctuations. The second is

that such series can be produced by the summation of random causes; the third that serial correlations thus originated are approximations to the ordinates of the Gauss curve.

In 1932, the translation was available to Frisch (Frisch to Slutsky, 31 March 1932).

20 'Indeed, the theoretical scheme we consider is no other than the scheme of a linear resonator in acoustics or in the theory of electrical waves' (ibid.: 7).

21 Another interesting topic of this lecture was the critique of spectral or harmonic analysis. Frisch's main argument was that spectral analysis required applying a large window in time, an altogether unacceptable procedure: 'It is obvious that in social questions, this is an implausible procedure', and local methods were preferable (ibid.: 9–10). This is, of course, consistent with Keynes's later critique of Tinbergen's estimation (Chapter 7).

22 Commenting on Tintner's paper for the *Quarterly Journal of Economics* (November 1938), presenting PPIP as an unstable system with shocks, Frisch argued it was instead a stable (damped) system with shocks, like a pendulum. 'This idea is fundamentally the same as Slutsky's or Yule's', traceable to Wicksell, who 'was, I think, the first to suggest that shock-accumulations may initiate business cycles' (Frisch, 1939: 639).

23 The paper did indeed represent the culmination of Frisch's research into a suitable mathematical model for cycles. The working of the simulations took a great deal of time and the very long paper was sent very late to the editor of the volume (Frisch to Karin Koch, 21 June 1933). Proud of his work, Frisch considered ordering 300 to 500 separate copies of the paper for distribution.

24

> I am sorry that the manuscript of my paper for the October issue has not yet been completed. I have been enlarging it and introducing new results which I have recently obtained. I believe that the whole paper will be a sort of systematic exposition of the main problems of maintenance swings by erratic shocks primarily as supplied to economic problems. I am now endeavouring to build a bridge from the strictly determinate dynamic analysis which gives rise to the well known damped cycles to the point of view where erratic shocks are introduced. I have a feeling that this paper is going to arouse interest.
>
> (Frisch to Nelson, 22 June 1933)

25 The system ignores any repercussions from investment upon consumption and is definitely a non-Keynesian model.

26 The Cassel paper wrongly indicates the origin of this reference. Wicksell's metaphor appeared in his 1918 review of a paper by Petander, 'Karl Petander: Goda och darliga tider', in *Ekonomisk Tidskrift* 19, 66–73, in a footnote to page 71: 'if you hit a rocking horse with a stick, the movement of the horse will be very different from that of the stick. The hits are the cause of the movement, but the system's own equilibrium laws condition the form of the movement' (quoted in Thalberg, 1992: 115n.; also Velupillai, 1992: 70n.). Frisch gave 1907 as the date for the original formulation of this metaphor. It represented the single most important starting point for the econometric analysis of the cycle, and the metaphor explicitly or implicitly dominated the research programme for a very long time (Louçã, 1997: 117f.). Wicksell frequently repeated this rocking-horse analogy but also considered sporadic inventions and technical progress as the source of economic fluctuations (Thalberg, 1998: 463), in what would become a Schumpeterian formulation.

27

> It is in particular exceedingly interesting to see how come some of these cycles are intrinsic, that is to say, how they correspond to solutions of the characteristic solutions of the system, while others are pure *cumulative cycles* and coming in only through the fact studied by the 'Changing Harmonics' technique. For instance, in one of these set-ups I obtained, by inserting values of the numerical

> parameters which I thought would be plausible, first the cycle of 8.5 years, which, as you know, corresponds nearly exactly to the one we know from the statistical data. Second another cycle of 3.5 years which we know corresponds nearly exactly to the business cycle known empirically. There was no characteristic solution of the system that indicated a longer period, but such a longer period *came in through the erratic cumulations*, and it turned out to be about 55 years!, which, as you know, is about the length of the longer swings.
>
> (Frisch to Cowles, 3 July 1934)

This letter indicates a new interpretation of the long waves, as a process differing from the 'inner cycles'.

28

> The point really is that nothing whatsoever in the economics of Frisch's model warrants any justification for the 'propagation and impulse' dichotomy. That it was part of a long line of distinguished tradition of economic theorizing is quite another matter. Frisch, by ignoring the implications of nonlinearities implicit in economic and mathematical models, was forcing a methodological principle as the only possible – or, at least, the only desirable – way of approaching the problem of modelling economic fluctuations.
>
> (ibid.: 68)

29 Thalberg rightly emphasises the divergence between Frisch's and Schumpeter's points of view on cycles:

> Given Frisch's enthusiasm for Wicksell and the rocking-horse analogy, his strong belief in the stability or dampening axiom, and the idea that erratic shocks supply the needed energy to keep cycles alive, one might have expected that Frisch would have gone into some sort of polemics with Schumpeter. After all, Schumpeter's basic theory differed fundamentally from that of Frisch. Schumpeter's explanation of the 'normal' cycle did not involve disturbances or the stability assumption. In the view of Frisch, a model of the propagation mechanism explaining the periodicity of the cycle was missing. In Schumpeter's theory the length of the cycle was stochastically determined. However, Frisch's statement that his own theory did not contain the whole explanation, that there was also present another source of energy, may be interpreted to indicate that he felt that Schumpeter's theory could supplement his own theory. Frisch assumed serially uncorrelated shocks, whereas Schumpeter's own theory implied some sort of autocorrelated shocks.
>
> (1998: 471–2)

30 Frisch had only got to know Cowles recently, but they were in close touch given the preparation of *Econometrica* and other matters of the Society (see Chapter 2). In his work on the decomposition of historical time series, Frisch had already asked for Cowles's cooperation:

> I do not feel as yet that economic theory is in a position to offer means of definite forecasts. But nevertheless I should try very much to make a few attempts in this direction. I would like to make these attempts on a confidential basis without publishing anything about them, and still having to check and [get the] advice of somebody who has really been in the field. I am therefore asking if you would be willing to cooperate on this scheme.
>
> (Frisch to Cowles, 15 September 1932)

31 '[Series 5 is] a curve representing a series of erratic shocks with damped oscillations, these shocks being represented by a wide range of intensities and the resultant oscillations being superimposed on each other at irregular intervals' (Cowles to Frisch, 6 September 1933).

32 The final proofs of PPIP had already been sent to the editor the previous June, but the paper was then presented to the Leiden conference of the Econometric Society. Frisch possibly had in mind future corrections or additions. In spite of this, he made no reference to Cowles's experiment.
33 Cowles to Frisch, 13 November 1933.
34

> You have understood correctly our proposed procedure in the case of the roomful of rocking-chairs. The ordinate of the curve at the point of time t would be the sum of a greater number of damped sine curves started at erratic intervals with erratically varying velocities.
>
> However we have temporarily abandoned this experiment owing to the laborious nature of the calculations and have worked another representation of a stream of erratic shocks. In this we used a galvanometer, adjusted so that one cycle was completed in 17 seconds with a damping effect which, after one shock, would return the galvanometer to equilibrium in about 12 cycles. The galvanometer was operated through a switch, the force of the current going through the switch having been varied by setting a rheostat, and the direction of the current varied by means of a rotating device. We thus delivered on the galvanometer a series of kicks through momentarily closing the switch, and the intensities of the kicks were varied by fresh settings of the rheostat, both at erratic intervals dictated by card-drawings, both the erratic series referred to conforming to a normal distribution. The time, the force, and the direction of the shocks were all erratic. A beam of light from the galvanometer fell on a scale and we took motion pictures of the play of the beam on the scale. Thus we propose to compile our readings and chart them. We think we will have a true representation of the effect of a stream of erratic shocks on a pendulum. Our record will include 1,200 items, 30 cycles of 40 items each (by 30 cycles I mean what *would be* 30 cycles in the absence of erratic shocks).
>
> We may afterwards construct another curve representing the motion of a pendulum in the case where the length of the pendulum and the strength of the gravitational field are being varied at erratically chosen points of time in erratically chosen degrees. These reduce to changes in the amplitude and duration of the cycles, the changes being introduced erratically.
>
> (Cowles to Frisch, 13 November 1933)

35 Frisch had studied in France and he knew, and had bought, some of the major works by Poincaré. These books were part of Frisch's library, but the handwritten notes in the margin tend to show that he was mostly interested in Poincaré's concepts of science and world-views. In 1933, when Frisch gave some lectures at the Poincaré Institute in Paris (Chapter 2), he did not mention Poincaré's mathematical intuitions about nonlinear resonance, of which he was apparently not aware.
36 The letter goes on:

> We have thus two classes of fluctuations which are simultaneously present and to these must be added a third class: if any factor whatsoever so acts as to produce an 'up' and 'down', for instance government expenditure financing a war and government deflation after that war, the system practically always adapts itself in a fluctuating way so that 'waves' of a third kind arise which are simply due to the properties of the adaptive mechanism of a capitalist economic life. This third kind is what Tinbergen calls the endogenous fluctuations.
>
> [. . .] What I am primarily interested in is the second group of fluctuations which I believe owe their existence to a process I can fully explain and roughly trace through the whole stretch of economic history that lies within the framework of the capitalist society. Of course that process also induces fluctuations of the third kind.
>
> (ibid.)

37 For instance, Samuelson:

> well, it [the acceptance of Frisch's point of view] slowed down our recognition of the importance of nonlinear models of Van der Pol-Raleigh type, with their characteristic amplitude features lacked by linear systems. And, in my case, it led to suppressing development of the Harrod-Domar exponential growth aspects that kept thrusting themselves on anyone who worked with accelerator-multiplier systems.
>
> (Samuelson, 1974: 10)

38 Examples abound of chaotic models that have been developed for such diverse topics as multiplier-accelerator dynamics (Gabisch, 1984), Cournot oligopoly (Puu, 1993), neoclassical growth (Boldrin and Montrucchio, 1986), R&D expenditure generating chaos (Baumol and Wolff, 1992), IS-LM economies (Day and Shaffer, 1985), cobweb models and inventory dynamics under rational expectations (Hommes, 1991), consumer behaviour (Benhabib and Day, 1981), Walrasian general equilibrium (Gandolfo, 1997), overlapping generations models (Grandmont, 1985), equity bond pricing under rational expectations (van der Ploeg, 1986), Lotka-Volterra populations (Gandolfo, 1997), spatial pattern formation and the Hotelling model for population dynamics (Puu, 1993) and so many others.
39 Indeed, nor is there equilibrium in the pure neoclassical sense. Frisch argued that the evolution of endogenous variables can be studied from

> the nature of the structural equation without introducing any notion of 'equilibrium' values of the variables. However, although *this notion may in point of principle not be necessary in a truly dynamic system*, it may, even here, in many cases, help towards a simpler and clearer systematisation of the various features of the movement.
>
> (Frisch, 1935–6: 101, my italics)

In the same paper, Frisch indicated a possible alternative strategy: equilibrium could be defined either as a mechanical concept or as a 'social concept' (ibid.: 101). Of course, the second concept was not operational for modelling, since the dominant strategy assumed that economies always tended to be equilibrated. And yet it is relevant to notice that Frisch was aware of the limits of his own methodology. Ignoring this social interpretation, Frisch's impulse-propagation scheme, the rocking horse, incorporated the physical and mechanical notions of movement and rest, and allowed for the use of the more sophisticated tools of econometric analysis.

7 Challenging Keynes: the econometric movement builds its trenches

1 Some anecdotal evidence is to be found, for instance the letter Schumpeter wrote to Keynes about his *TM*, where he tells how, during a trip, he met a professor of Greek who classified only two living people as first-class world scientists: Einstein and Keynes. 'It is true *vox populi*', and Schumpeter added: 'People are full of your book here, as well they might' (Schumpeter to Keynes, 22 October 1932). Knowing that Schumpeter so deeply envied Keynes's success and abandoned his own attempts to write a book on money after the publication of the *TM*, the duplicity of this remark is evident. Of course, Schumpeter was not so kind when, later on, the publication of the *GT* set the agenda for economics, and even influenced most of his disciples and colleagues at Harvard: he then wrote the bitterest review he could.
2 Nothing less than the work of the newly-created (1930) Econometric Society provides indicative examples of this conflicting *état d'esprit* among the mathematically inclined economists. For instance, in a letter to Schumpeter on 25 November 1935, the secretary of the Society, Charles Roos, complained about the difficulty in obtaining grants and in particular about the possible negative assessment of most of the eventual

referees if consulted by the finance institutions. According to Roos, the mathematicians would be hostile to the projects of the Econometric Society but, worse still, some of its own members might adopt an unpredictable attitude. In Roos's opinion, this would be the case with Snyder, who was 'quite unfavourable' to such projects, and with Wesley Mitchell, both founder members of the Society. Mitchell was one of the most famous and most respected of the original members. When, in February 1933, the members of the Society elected their Fellows for the first time, the candidate who received most votes was in fact Mitchell, who was not really an econometrician. And the second doubtful referee, Snyder, was appointed by the Econometric Society to serve on the Advisory Board of the Cowles Commission, in spite of the general mistrust displayed in Roos's letter. More details can be found on this subject in Chapter 2.

3 The Constitution of the Econometric Society opens with the following programmatic statements:

> The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics.
>
> [. . .] Its main object shall be to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences. Any activity which promises ultimately to further such unification of theoretical and factual studies in economics shall be within the sphere of interest of the Society.

4 Frisch argued that, precisely because communism was to be avoided in his view, planning and governmental economic activism were decisive: society should

> let the government manipulate certain links in the machinery, such as monetary and credit policies, trust policies, trade policies, and so on, with the conscious aim of indirectly steering the economy out of the existing chaos and into a situation guided by a definite social plan.
>
> (ibid.)

Hansen, who could read Norwegian and studied this text, answered that the crisis was not one of organisation but of adaptation to change, and that the planning alternative would damage the necessary adaptive ability of the economy (Hansen to Frisch, 12 December 1931; Frisch to Hansen, 20 February 1932).

5 In a new version (1947) of his 1932 radio lectures, Frisch added a paragraph politely accusing Keynes of a lack of originality:

> His [Keynes's] large influence is mostly due to his mode of presentation, convincing, incisive, penetrating, even if it was not always clear and precise. The thoughts he combined were not new, although (in part due to his lack of sufficient knowledge of other authors) he had a strong impression of his own novelty.
>
> (Frisch, [1932b] 1947: 56)

In particular, Frisch considered that Wicksell had anticipated most of the *GT*.

6 This meeting had occurred in 1934, when Frisch visited Cambridge. He was interested in approaching the Keynesian group: 'Besides seeing Keynes I want to discuss the matters of the Econometric Society with the younger group in Cambridge. Do you think that Kahn, Joan Robinson, Gifford, Colin Clark and others will be around these days?' (Frisch to Sraffa, 7 March 1934). Sraffa arranged for a meeting with Pigou and lunch with Keynes and, on 12 March also invited Frisch to an evening meeting of the Political Economy Club in Keynes's rooms, where Austin Robinson presented a paper on increasing returns (Sraffa to Frisch, 7 March 1934).

7 This was Frisch's first direct contact with Neyman's sampling theory, and he was quite impressed, asking Haavelmo to stay in London in order to study this new approach (see Chapter 8).

8 J. Hicks was

> an economist who did not belong to the core of the econometric group. Hicks had always been sceptical of the econometric approach towards empirical studies and his scientific relationship to the leading econometricians has been strained at times. Nevertheless, Hicks had established fairly close relations with the econometricians, his article was partly instigated by the group, and if Hicks had not succeeded, econometricians such as Lange or Leontief would have been ready to supply close substitutes. Frisch, too, considered to formulate Keynes' theories mathematically.
>
> (Andvig, 1995: 278–9)

9 Frisch also took a close interest in Marschak's career, beginning at the time he escaped Germany when Hitler came to power in 1933: he interceded with Bowley in order to provide help for Marschak when he got to England (Frisch to Bowley, 3 May 1933), and then again tried to get him a job in the US. From the early 1930s onwards, Marschak was one of the European economists most heavily involved in the workings of the Society, and he was the local organiser of the Oxford meeting.

10 Andvig argues that at that time Lange, Leontief and Frisch considered the possibility of formulating Keynes's theories mathematically (Andvig, 1995: 279).

11 Frisch to Hicks, 20 November 1936.

12 Frisch to Hicks, 20 November 1936. Two months later he insisted: 'I wish you would consider the suggestion I made in an earlier letter of adding a rather full footnote referring to the other people who have taken part in the colloquium discussions at Oxford on this topic' (15 January 1937). And still in February, Frisch justified his suggestion, which was so important *from the point of view of the Society*: 'With regard to the footnote I thought I remembered your speaking rather enthusiastically in Oxford about the discussion on Keynes. This was the only reason why I suggested something in the way of a footnote. You must of course express exactly what you feel in the matter' (8 February 1937). Hicks had rejected the idea from the beginning, on the initial grounds that there were so many people to acknowledge that it became impossible:

> It [the paper] has had a great deal of rehashing, first as the result of the discussion I have had here. The list of acknowledgements got so long that it had ultimately to be scrapped altogether – which I have no doubt is what all concerned would prefer!
>
> (Hicks to Frisch, 12 November 1936)

13

> I am frightfully afraid of the tendency, of which I see signs in you, to appear to accept my constructive part and to find some accommodation between this and deeply cherished views which would in fact be only possible if my constructive part had been partially misunderstood.
>
> (Keynes to Harrod, in Keynes, XIII: 548)

The enigma is then to discover why he was so 'frightfully afraid' of those versions but did not react to Hicks's. Of course, Keynes did not consider Hicks's work very highly, and this may explain his reaction to the 'emptiest platitudes paraded as generalisations of vast import' of the 'utterly empty' *Value and Capital* (see Moggridge, 1992: 553).

14 'With these two he could feel complete confidence that they understood what he was driving at' (Harrod, 1951: 451). It is well-known that Robinson argued that sometimes they needed to show Keynes the radical implications of his own thought – and that Samuelson argued the other way round, that 'until the appearance of the mathematical models of Meade, Lange, Hicks and Harrod, there is reason to believe that Keynes himself did not truly understand his own analysis' (Samuelson, 1947: 146). The *Quarterly Journal of Economics* paper refutes Samuelson's point: Keynes opposed a large part of what he understood.

15 It is worth noticing Hicks's afterthought on the IS-LM case, since Hicks himself later pointed out the two major modifications that the IS-LM scheme implied for the original Keynesian theory. First, it excluded time and uncertainty and therefore reduced the *GT* to a particular case of general equilibrium. Second, it introduced an alternative vision of causality, which is sequential and non-deterministic for Keynes and simultaneous and deterministic for the Hicksian scheme (Hicks, 1979: 74). Consequently: '[The IS-LM scheme] is now much less popular with me than I think it still is with many other people. It reduces the *GT* to equilibrium economics; it is not really *in* time. That, of course, is why it has done so well' (ibid.: 289–90). In that sense, he argued that general equilibrium is not realistic and therefore is not general, as Keynes did:

> I doubt if there is any concept of equilibrium usable in economics which is truly 'general', in the sense that there are no choices which might conceivably be open to the actors but which have not been, for the purpose of the model, deliberately closed [for example, collusion between agents]. The Walrasian equilibrium itself, which is commonly regarded as a pattern of general equilibrium, is not general in this unrestricted sense.
>
> (ibid.: 79n.)

16 Such implications eventually became clear later on. Pasinetti argued that

> the Hicks reinterpretation also helps to illustrate how the replacement of causality ordered relations with a system of simultaneous equations is not used only as a purely found device but as a medium to reintroduce a basically different interpretative model of economic reality.
>
> (Pasinetti, 1974: 47)

And so did Leijonhufvud: 'It also becomes apparent that IS-LM is a cumbersome, inappropriate frame for representing theories that make different assumptions about the knowledge possessed by transactors and, consequently, about the time-phasing of events' (Leijonhufvud, 1981: 148) and therefore 'the IS-LM model has not served as an adequate representation of Keynes' (ibid.: 206n.). Garretsen (1992: 45) and Mini (1974: 252) argue along similar lines.

17 In the introduction to the second volume, Tinbergen wrote that

> the problem of finding the best stabilizing policy would consist in finding such values for the coefficients as would damp down the movements as much as possible. *The outstanding importance of the numerical values of the coefficients may be clear from these few considerations.*
>
> (Tinbergen, 1939, II: 18)

18 The fact that Keynes did not follow many of the technical details of the books he was reviewing is quite obvious and was anticipated by many of those who knew him. Some impressive evidence can again be found in his correspondence with Frisch. In 1932, Keynes rejected a paper by Frisch for the *Economic Journal* – in fact, he was never able to publish a single paper under Keynes's editorship – on the grounds that it would only be read by 'half a dozen readers' (Keynes to Frisch, 10 February 1932). Two weeks later, he explained that he feared the limits of mathematical formalism, since he considered that intuition should lead the research and not be limited by the restricted set of assumptions of a formal model. Three years later, in acknowledging a book sent by Frisch, Keynes indicated his distance in relation to such techniques:

> It looks to be a very interesting piece of work but, alas, though once qualified to taste such things, I am afraid that I should now find myself out of depth if I were to try to embark on critical discussion of this difficult branch of a subject which I have long neglected.
>
> (Keynes to Frisch, 13 February 1935)

He emphatically repeated his distance in relation to the branch of mathematical economics, and underlined his mistrust about its performances and results, when subsequently rejecting another of Frisch's papers:

> But I am unfamiliar with the methods involved and it may be that my impression that nothing emerges at the end which has not been introduced expressly or tacitly at the beginning is quite wrong.
>
> [. . .] It seems to me essential in an article of this sort to put in the fullest and most explicit manner at the beginning the assumptions which are made and the methods by which the price indexes are derived; and then to state at the end what substantially novel conclusion has been arrived at.
>
> [. . .] I cannot persuade myself that this sort of treatment of economic theory has anything significant to contribute. I suspect it of being nothing better than a contraption proceeding from premises which are not stated with precision to conclusions which have no clear application.
>
> [. . .] [This creates] a mass of symbolism which covers up all kinds of unstated special assumptions.
>
> (Keynes to Frisch, 28 November 1935)

19 In another letter to Frisch, some weeks later, Keynes explained that

> I think it vitally important that econometricians should avoid using an elaborate symbolic language and pretentious mathematical formulae unless they do really bring something out at the other end. It has to be admitted, I think, that at the present time these methods are proving disappointing and in risk of falling into general discredit.
>
> (Keynes to Frisch, 30 December 1935)

As in the previous case of Hicks, Keynes proved again to be a bad judge of the main trends in economic theorising.

20 Keynes to Tyler, 23 August 1938, in Keynes, XIV: 285.

21 Harrod was very sympathetic to Tinbergen's approach. He criticised Keynes's critique: 'My main point, however, is that you have got the balance of emphasis quite wrong' (Harrod to Keynes, 7 September 1938). Just after the 'Business Cycle Conference' of 18–20 July he had written to Keynes: 'We had a little League of Nations meeting in Cambridge, last month, to discuss Tinbergen's work. Tinbergen himself seems very open-minded – and such a delightful man!' (3 August 1938, in Keynes, XIV: 301). This last remark is shared by many that knew Tinbergen. And Keynes was no exception: in his last letter to Cowles, on 23 July 1945, Keynes gave an account of a meeting with Tinbergen, who had just visited him, and clearly indicated his abiding affection for Tinbergen: 'I felt once more, as I had felt before, that there is no-one more gifted or delightful or for whose work one could be more anxious to give every possible scope and opportunity' (quoted in Stone, 1978: 15).

22 Keynes to Harrod, 16 July 1938, in Keynes, XIV: 299.

23 Keynes to Harrod, 11 August 1938, in Keynes, XIV: 302.

24 There is nevertheless a very curious note in Keynes's reply to Tinbergen in the *EJ*, in which he admits that, had he been engaged in statistical work, for practical reasons he would probably have accepted some of the points he was then criticising: 'I am afraid it may be true that if I moved in statistical circles, I should find trend terms a terribly convenient "catch-all"' (March 1940, in Keynes, XIV: 319).

25 Harrod commented that Keynes had never quite grasped the mathematical techniques: 'He had no specific genius for mathematics; he had to take pains with his work' (Harrod, 1951: 57). So did Stone, who wrote that in the 1930s, his mathematics was 'pretty rusty' (Stone, 1978: 12). But Patinkin goes even further, suggesting that the general criticism of mathematical models was due to the failure of the previous formal mathematical models devised by Keynes: 'In fact, it may have been Keynes's

lack of success with such formal model building in the *TM* that led him to the more critical attitude expressed in the passage from the *GT* just cited' (Patinkin, 1976: 1094). Such a suggestion is highly arbitrary, if one acknowledges the consistent philosophical discussion by Keynes about inductive strategies, which was the basis for his work on statistical inference. Yet, it is true that, as indicated, Keynes did not conceal his ignorance of the recent techniques and talked openly about it both in private letters and in public speeches.

26 In his very first letter on the subject, Keynes wrote: 'There is first of all the central question of methodology – the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non-homogeneous through time' (Keynes to Tyler, in Keynes, XIV: 285–6). This 'central question of methodology' was naturally related to that of defining economics as a 'typical natural science' or as a 'moral science' (letter to Harrod, ibid.: 296). Carabelli argues that this was also a 'family quarrel' for Keynes, who was opposed to his father's concept of economics as an empirical science (Carabelli, 1988: 175).

27 This last charge was not so negative for Keynes, since he was fascinated by alchemy (he bought Newton's precious manuscripts on alchemy, Moggridge, 1992: 492n.). His survey of Tinbergen finished with the phrase: 'Newton, Boyle and Locke all played with alchemy. So let him continue' (Keynes, XIV: 320). He did so.

28 Keynes to Lange, 10 April 1940.

29 Such is the argument of Skidelsky:

> Yet Tinbergen and others found it all very strange, because Keynes's own policy purposes required some attempt to quantify the consumption, investment and liquidity-preference functions, as well as to measure national income. He was also on the Council of the Econometric Society. Keynes, it may be said, objected not to econometrics as such, but to the method of econometrics. But the conditions he imposed on the fruitful application of the method were so strong as virtually to eliminate its usefulness.
>
> (Skidelsky, 1992: 620)

Keynes was president of the Econometric Society in 1944 and 1945.

30

> In the case of the history of gold prices, to which defenders of the Quantity Theory usually refer with confidence, factors other than gold production have changed and fluctuated so hugely and so notoriously that the use of any apparent close coincidence between the level of prices and gold production in support of the Quantity Theory is a gross example of the *post hoc*, *ergo propter hoc* argument. Since other factors have not remained constant, the theory would lead us to anticipate coincidence between prices and gold production if the other factors happened to balance one another; and one cannot easily prove this without assuming the theory itself.
>
> (Keynes, XII: 765)

31 'The review [Keynes on Tinbergen] is a model of testiness and perverseness. Apart from a number of misunderstandings on mathematical questions, Keynes seems positively to resent all attempts to overcome what he recognises as "the frightful inadequacies of most of the statistics employed"' (Stone, 1978: 11). The author claims to have been surprised by the 'violence of his [Keynes's] hostility to the use of mathematics in economics and his eagerness to belittle its difficulties and its potential usefulness' (ibid.: 8). Stone's argument that this attitude dominated the young Keynes and was replaced by a mature respect (ibid.: 54) is certainly flawed, since the Tinbergen debate occurred when Keynes was aged fifty-five to fifty-seven, just six years before his death. Tinbergen was convinced that Keynes did not understand either the method or the implications, and later he recounted one last meeting with Keynes in order to emphasise this point:

> I think Frisch was not entirely against me. Keynes was more doubtful of the whole thing than Frisch was. . . . Indeed I felt that, at least on certain points, he [Keynes] was badly informed. . . . It was a bit strange to me because he had written [the] Treatise on Probability, so I thought he was somewhat familiar with statistics. At first I was a bit disappointed, because I thought that he would be especially happy with my work, since we had very largely followed his main macro-theories. But all that seemed not to impress him very much.
>
> (Tinbergen, 1987: 129)

32 Marschak and Lange thought Keynes had just read the first volume and not the second (Marschak and Lange, 1940: 390). Morgan argued in 1990 that Keynes did not even bother to read Tinbergen's volumes with care and that he ignored the technical developments of statistics (Morgan, 1990: 121, 121fn.), and Hendry and Morgan added that 'Keynes might have been reading another book altogether' or even not have read it at all (Hendry and Morgan, 1995: 54). It is certain that Keynes did not take the time to study the details of Tinbergen's method, and completely ignored important technical topics, such as Tinbergen's concern about the stability of the sub-samples. But his critique was more general and still accurate on the main points: in 1980, Hendry accepted that many of these arguments still remain unopposed forty years afterwards, and argues that the distinction between statistical science and alchemy is still a narrow one (Hendry, 1980: 402–3).

The reason is of course the very nature of economic processes:

> Econometricians conceptualise this [economic] system as a complex nonlinear, interdependent, multivariate, disequilibrium, dynamical process dependent on agents' expectations and their adjustment, subject to random shocks, and involving many phenomena that are unobservable; relevant time series data are inaccurate, exist only for short periods and for a few major variables; economic theories are highly simplified abstractions usually of a comparative static form invoking many ceteris paribus clauses (with yet another implicitly required), most of which are invalid in empirical applications – little wonder our macroeconometric representations are less than perfect.
>
> (Hendry, 1980: 399)

But isn't this merely an echo of Keynes's critique in modern parlance?

33 A very obvious example is provided by the *Handbooks of Mathematical Economics* edited by Arrow and Intriligator (1981–91): in the 2,264 pages of the twenty-seven chapters of the four volumes, only two chapters include a (single) reference to Keynes. For mainstream economics, Keynes had become a museum curiosity. And only when the problems mount up in the paradigm is any attention paid to his critique once more.

34 The Keynesian circle clearly underestimated the importance of the econometricians. There is not one line of reference to this debate in Harrod's biography of Keynes. And Marschak was presented to Keynes by Harrod as just a 'minor Tinbergen' (Harrod to Keynes, in Keynes, XIV: 298). Keynes wrote to Pigou that he had a 'very poor opinion of Marschak and only a moderately good one of Lange' (29 March 1940, quoted in O'Donnell 1997: 154–5). Considering Marschak's later role in the development of the Econometric Society and the Cowles Commission research programme, these were obvious and consequential understatements. Lange was convinced of the superiority of their treatment since it is 'much more superior and thoroughgoing' than Tinbergen's answer (Lange to Marschak, 3 July 1940).

35 Lange to Marschak, 3 July 1940.

36 Lange to Marschak, 5 February 1940.

37 Marschak to Lange, 11 February 1940.

38 In fact, this view was not completely shared by the authors and there is a *clause de style* in this formulation. In the paper, they join Keynes in supporting the use of

Lexis's method for investigating evidence of structural change (as it happens, the authors surprisingly ignored similar attempts made by Tinbergen in the very book in question), but Marschak wrote a note in the margin of his manuscript, stating that 'This is not new. I think this is too kind to Mr. Keynes.'

39 Lange prepared the first draft, but the second one was more critical towards Tinbergen. Lange told Marschak that 'in my opinion, Tinbergen is much more open to the reproach of having neglected the difficulty of the problem of correlation of time-series than you seem to admit'. In a hand-written note added to the letter, he further argued that

> I am afraid Tinbergen is in a really bad situation with regard to the serial correlation problem. The very point of a dynamical model implies the existence of serial correlation. Thus Tinbergen's treatment is really of doubtful statistical significance. But Keynes criticised him for the wrong reasons.
>
> (Lange to Marschak, 5 February 1940)

Marschak responded that the criticism levelled against Tinbergen should be that of 'not taking care of the errors' and time series correlation (Marschak to Lange, no date).

40 Frisch to Tinbergen, 5 September 1932.

41 Another reason for the scepticism, other than that which has been discussed here, was Frisch's distance in relation to the probabilistic approach that Koopmans and Haavelmo were beginning to adopt at that time, and which was to dominate econometrics from then on. Tinbergen explained this difference with Frisch quite explicitly in the introduction to his first volume (Tinbergen, 1939, I: 28–9).

42 Tinbergen to Frisch, 11 September 1938. Indeed, either Tinbergen or the editors of the book rejected the inclusion of Frisch's comments.

43 Tinbergen to Frisch, 8 April 1939. The personal relationship between Frisch and Tinbergen was not damaged by this difference. Tinbergen always considered Frisch's scientific abilities and capacities very highly, and frequently asked for his plans to publish books highlighting the 'shock theory' (Tinbergen to Frisch, 20 January 1940) or the 'disturbed dynamic systems' (22 September 1941). There was justice in the fact that both were awarded the first Nobel Prize for economics together.

44 Divisia to Frisch, 14 November 1938.

45 Divisia to Frisch, 20 May 1933.

46 Divisia to Frisch, 1 June 1933.

47

> Econometricians conceptualize this system as a complex nonlinear, interdependent, multivariate, disequilibrium dynamical process dependent on agents' expectations and their adjustments, subject to random shocks, and involving many phenomena that are non-observable; relevant time-series data are inaccurate, exist only for short periods and for a few major variables; economic theories are highly simplified abstractions usually of a comparative static form invoking many explicit *ceteris paribus* clauses (with yet others implicitly required), most of which are invalid in empirical applications – little wonder our macroeconometric representations are less than perfect.
>
> (Hendry, 1980: 399)

8 *Quod errat demonstrandum*: probability concepts puzzling the econometricians

1 Gauss claimed in 1809 that he had been using the OLS method since 1795. Galton generalised the procedure of regression in 1885 (Stigler, 1986: 2).

2 There was an important moment of discussion between Galton, the British promoter of positivism and a cousin of Darwin, and Wallace, the co-founder of modern biology. Galton argued that there is an immanent structure of order, that of the normal law:

> I know of scarcely anything so apt to impress the imagination as the wonderful form of cosmic order expressed by the 'law of frequency of errors'. The law . . . reigns with serenity and in complete self-effacement amidst the wildest confusion. The larger the mob, and the greater the apparent anarchy, the more perfect is its sway. It is the supreme law of Unreason. Whenever a large sample of chaotic elements are taken in hand and marshalled in the order of their magnitude, an unsuspected and most beautiful form of regularity proves to have been latent so long.
>
> (quoted in Peters, 1994: 14)

Wallace, on the other hand, argued that the attributed stable structure of order is suspect, given the creation of variation.

3 Marshall to Bowley, 21 February 1901.

4 For Frisch,

> a regression was considered properly specified if it was 'complete', meaning that it contained all the relevant variables and so did not contain an error term. This was the standard framework that explained residual variance in terms of measurement errors in the variables. Frisch's innovation was to extend the framework to encompass what he termed the 'complete system', i.e. the total of n equations that presumably were needed to determine the n variables appearing in the equation of interest. He took pains to avoid mentioning the market equilibrium problem in order to emphasize his notion of a system as a general feature of *any* econometric movement.
>
> (Epstein, 1987: 38)

In the same sense, Boumans emphasises that both Frisch and Tinbergen shared the ideal of a closed system with all endogenous variables (Boumans, 1992: 129).

5 Reiersol studied mathematics and, as he became involved in economics, he was part of a group led by Haavelmo. In 1946, he went to Cambridge to study with R.A. Fisher. The same year he moved to Columbia, New York, and then became part of the Cowles Commission in Chicago (Reiersol, 2000: 119).

6

> If there could be developed a practical method of tracing the roots of this equation as time series, very great progress would be made toward the further application of my time series method to the more complicated cases where more than two essential components are present. I hope that you will give this problem your closest consideration. With the apparatus and technique which you have at your disposal in your knowledge of the theory of matrices and the theory of approximation of the roots of algebraic equations, you should be in a position to obtain significant results along this line. I hope that this problem will interest you and that you let me know your reaction to it.
>
> (Frisch to Aitken, 26 September 1930)

7 Aitken to Frisch, 1 November 1930.

8 Frisch to Aitken, 20 November 1930. Other established scholars showed no interest in testing these methods – and it is also probable that some of these indicated by Frisch were not enthusiastic. But when Frisch approached Persons, he got a negative response: '[we, Persons and Crum] have reached the opinion that, despite the possibility that this method might prove immensely helpful in the kind of work we do, we shall have no early opportunity to subject it to numerical tests by the use of actual data' (Crum to Frisch, 30 June 1927).

9 Schultz to Frisch, 26 January 1931, my italics.

10 Frisch to Schultz, 3 June 1931.

11 This situation [the study of the law of variation of utility] has many analogies in problems of other sciences where the crucial thing to know is the law of variation, not the philological question of interpreting the unit of measurement. Take, for instance, the theory of gravitation. In that stage of development where physics were at the time of Newton the important thing to find out was the law of attraction saying that the attractive force between two bodies is proportional to the square of the distance between them. It was this discovery that constituted the crucial progress and which opened up the possibilities of a vast field of applications. In comparison to this the discussion of the philosophical interpretation of the 'dimension' of the absolute gravitational constant that occurs in the equation was a matter of decidedly secondary importance.

(Frisch to Schultz, 3 May 1932)

12 Schultz to Frisch, 19 April 1932.
13 Frisch to Schultz, 13 May 1933.
14 Waugh to Frisch, 20 March 1935.
15 Schultz to Frisch, 4 March 1935.
16 Frisch to Schultz, 21 March 1935.
17 Frisch to Schultz, 20 May 1935.
18 Schultz to Frisch, 5 June 1935.
19 Frisch did not forget the quarrel. Six months after this letter, he sent his colleague the notes of the lectures on sampling theory by Koopmans, then at Oslo, which Schultz considered a 'very neat job' (Schultz to Frisch, 14 January 1936). Frisch knew Schultz would understand and share Koopmans' points of view, although it was not exactly his own case.
20 Furthermore, Bjerkholt notes that Frisch developed a deep epistemological concern with the nature of human knowledge of the outer world, and that this notion was instrumental in deciding his preference for deterministic mechanical models instead of systems contaminated by unexplainable randomness.
21 In 1934, Frisch added shocks to the sine curves representing the trajectory of the solution of his model, and, in order to obtain irregularity, used the end-digits of the Norwegian lottery as shocks (Frisch, 1934a: 271). Yet, in other places, Frisch accepted another role for 'error': in his *Makrodynamikks* he states that random shocks create evolutionary forces (chapters 85 and 86).
22 Against the assumption of fixed technology, Frisch used the example of the policy maker in underdeveloped countries: 'he [the policy maker] is not interested in knowing whether an actual development path in his country will come closer to or be far away from some intrinsic path that has been defined by piling up queer assumptions' (ibid.: 162).
23 R.A. Fisher, for example, emphatically argued that statistical tests could disprove, but were unable to prove, a theory (Fisher, 1935a: 22).
24 Tinbergen to Frisch, 28 March 1935.
25 Koopmans to Frisch, 25 March 1935.
26 'On the request of Frisch, Koopmans then gave a series of lectures on R.A. Fisher's theory of estimation and Neyman–Pearson's theory of testing hypotheses' (Reiersol, 2000: 117). The lecture, 'On Modern Sampling Theory', fills thirty-six pages.
27 Hoel, of Norwegian origin, had just finished his Ph.D. Later he wrote a textbook on statistics that was used in Oslo after the Second World War, until it was replaced by the Frisch–Haavelmo memoranda.
28 At the time, the intricacies of the divergences between Neyman and Pearson, on the one hand, and R.A. Fisher on the other hand, were not clearly perceived by the neophytes: Koopmans wrote to Fisher stating that he could not understand his criticism of the Neyman–Pearson method (Koopmans to R.A. Fisher, 20 November 1935).
29 'By viewing exogeneity as a statistical property it is possible to introduce many

economic factors into a model that have the same interpretation as laboratory stimuli' and 'exogeneity was essential in the simultaneous equations methodology because it provided the conceptual basis for understanding economic data as the result of experiments' (Epstein, 1987: 171).
30 Koopmans emphasised the epistemological difficulties generated by this juxtaposition of different 'factors at work', and conceded Burns and Mitchell's argument against smoothing:

> In fact, one of the reasons why business cycle analysis is a difficult undertaking is that the economic system itself is such an effective smoothing agent of the random shocks to which it is exposed. The analytical problem is one of de-smoothing rather than smoothing.
>
> (ibid.: 171)

31 Hendry and Morgan argue that it is 'puzzling folklore' that Koopmans won the debate in this battle over funding (Hendry and Morgan, 1995: 69, 71).
32 One example is Haberler's misunderstanding:

> I admit I feel a little like a babe in the woods when confronted with the last intricacies of the Cowles Commission approach.
>
> [. . .] The econometric approach of the Cowles Commission seems to be petering out rapidly or not to be getting anywhere beyond extensive methodological discussions.
>
> (Haberler, 1949: 84)

33 Samuelson's interpretation for this event is that Schumpeter loved to take the 'unpopular side' in disputes (Samuelson, 1951: 49–50, 50fn.). Goodwin indicated that 'it was a great shock to me' (in Swedberg, 1991: 176), and so did Machlup (1951: 95)
34 The impact of the Cowles Commission research programme by then dominated the profession: the econometric revolution had won the day. Friedman, by then a researcher at the NBER, argued at the conference that a final synthesis should be reached between the NBER method and the Cowles approach (Friedman, 1951: 114). But Koopmans was so convinced of the victory of the econometric camp that he was able to recommend, in an internal memorandum to the Cowles Commission, 'Let's not fight too much' (Epstein, 1987: 111).

35 I had then the privilege of studying with the world famous statistician Jerzy Neyman in California for a couple of months. At that time, young and naive, I thought I knew something about econometrics. I exposed some of my thinking on the subject to Professor Neyman. Instead of entering into a discussion with me, he gave me two or three numerical exercises for me to work out. He said he would talk to me when I had done these exercises. When I met him for that second talk, I had lost most of my illusions regarding the understanding of how to do econometrics. But Professor Neyman also gave me hopes that there might be other more fruitful ways to approach the problem of econometric methods than those which had so far caused difficulties and disappointments.

(Haavelmo, 1989)

Notice how Haavelmo argues that only with Neyman and his method could he understand 'how to do econometrics'. Haavelmo's conversion to the Neyman–Pearson approach is well documented (Morgan, 1990: 242 fn.; Duo, 1993: 129 fn., Bjerkholt, 1995: xxviii).

36 But it played right over into Haavelmo's 1940 paper in *Econometrica* where he reconsidered the riddle and showed that a rocking horse, indeed, was *not* necessary for random shocks to create a cycle. Haavelmo tumbled with this idea since 1938, but did not resolve it until after arriving in USA. The opening of the paper

drew attention to a new focus: ‘The whole question is connected with the type of errors we have to introduce as a bridge between pure theory and actual observations’. Haavelmo showed that a macrodynamic model with coefficients that gave the propagation part a non-cyclic character, say damped exponentials, could still generate cycles when exposed to random shocks, invalidating the idea that had been promoted by Frisch that the deterministic part of the model had to have damped cycles for the models exposed to shocks to generate cycles.

(Bjerkholt, 2005: 529)

37 For this purpose, he reproduced Slutsky’s experiment, although with no reference to his predecessor: using a cumulative series of Danish lottery numbers, he obtained nonsensical explanations for the artificially generated long cycles (ibid.: 321).

38 A constant law is defined by Haavelmo as an approximation: if we have $f(x_i, \ldots, x_n; a_1, .., a_n) = s$ whatever the x_i, and for another experiment we get $f(.) = s'$ with s and s’ having the same properties, there is a constant law (ibid.: 23).

39 Or else:

> Since we do not know what this true distribution is, we have to consider a whole system of possible alternatives. We might consider each of these alternatives as a description of one possible ‘mechanism’ which might produce the 26 annual figures [the predicted values of national product and consumption for the following 13 years].
>
> (Haavelmo, 1943: 17)

40
> ‘The discussion above gives also, I think, a clearer interpretation of the general phrase: ‘Suppose the whole formal set-up of the theory is wrong, what is the use of testing significance of coefficients etc.?’. As a matter of fact, this question is, strictly speaking, always justified when we try to explain reality by means of a theoretical model. But if we follow this attitude to its bitter end, we shall never be able to accomplish anything in the way of explaining real phenomena. It holds – in the scientific research as well as in other matters of life – that ‘who risks nothing, he gains nothing either’.
>
> (ibid.: 94)

41 Haavelmo also argued about the technical possibility of this approach: it is not necessary to assume independence of observations following the same one-dimensional probability law,

> it is sufficient to assume that the whole set of, say n, observations may be considered as one observation of n variables (or a ‘sample point’) following an n-dimensional joint probability law, the existence of which may be purely hypothetical. Then, one can test hypotheses regarding this joint probability law, and draw inferences as to its possible form, by means of one sample point (in n dimensions).
>
> (Preface to 1944: ii)

42 Coherently, Haavelmo rejected the ‘illusion’ of considering shocks as errors of measurement and not as stochastic concepts:

> Real statistical problems arise if the equations in question contain certain stochastical elements (‘unexplained residuals’), in addition to the variables that are given or directly observable.
>
> [. . .] In other words, if we consider a set of related economic variables it is, in general, not possible to express any one of the variables as an exact function of the other variables only. There will be an ‘unexplained rest’, and, for statistical purposes, certain stochastical properties must be ascribed to this rest, *a priori*.

> Personally I think that economic theorists have, in general, paid too little attention to such stochastical formulation of economic theories. For the necessity of introducing ‘error terms’ in economic relations is not merely the result of statistical errors of measurement. It is as much a result of the very nature of economic behavior, its dependence upon an enormous number of factors, as compared with those which we can account for, explicitly, in our theories. We need a stochastical formulation to make simplified relations elastic enough for applications.
>
> (Haavelmo, 1944: 1)

43 Wilson, from Harvard, stood out against this trend accusing Haavelmo’s paper of being filled with metaphysical statements, written in difficult English and exhibiting unpractical methods: ‘There is a small group of econometricians who are well trained in mathematics and who apparently chose to write for one another rather than for economists (or even econometricians) in general’ (Wilson, 1946: 173).

44 In any case, the Cowles Commission was not successful in its project for the development of methods for testing hypotheses (Duo, 1993: 26, 32).

45
> These purely mathematical connections, that are called Central Limit Theorem in statistical mathematics, explain that in nature and society there are so many phenomena that follow the normal distributions. It is therefore not surprising that it has been a theoretical basic problem in statistical mathematics to arrange in exact criteria, as general as possible, so that the conditions applied to a sum of stochastic variables approximately obtain the normal distribution.
>
> (Frisch, 1951b: 3)

46
> Another way of changing the results is to say that in practice we are just interested in the *central part* of the distribution. If the approximation is good here, so it doesn’t matter for us that the approximation – e.g. the expression of the relative error – is inadequate for the other part of the curve. We can use this point of view because we again have Tchebycheff’s inequality. With its help we can, assuming a finite dispersion, verify that in the ‘tail’ where approximation is not good, there is just a significantly small part of mass.
>
> (Frisch, 1951b: 50–1)

47 ‘Questions of this kind can be raised within many different investigation areas: physics, chemistry, medicine, agronomy, sociology, economy, etc.’ (Frisch, 1952c: 2).

48 Poincaré argued that chance is not a measure of ignorance. And, if Frisch were able to read the precious insights – as his contemporaries were not able to do – he would have taken note that small causes may produce large effects, which is attributed to chance (Poincaré, 1908: 68), that all sciences have ‘unconscious applications of probability computation’ (ibid., 1906: 216–17) and that irreversibility and entropy exclude symmetry in time (ibid., 1911: 160–1).

49 Notice this effort by Schumpeter to save his allegiance to orthodoxy stating that whatever the impulses were they would be compatible with equilibrium:

> Now, what causes economic fluctuations may either be individual shocks which impinge on the system from outside, or a distinct process of change generated by the system itself, but in both cases the theory of equilibrium supplies us with the simplest code of rules according to which the system will respond. This is what we mean by saying that the theory of equilibrium is a description of an apparatus of response.
>
> (Schumpeter, BC: 68)

In this sense, equilibrium was the property of the rocking horse and this propagation device also accounted for the effect of its internally generated process of change.

50 Ekeland concludes that the existence and relevance of chance do not necessarily lead to a probabilistic framework for modern statistics:

> The great discovery of these last years, in fact, is that statistics can function perfectly well without chance. The spread of computer techniques in management has led to the accumulation of enormous masses of data in all areas, and their simple classification, not to mention their interpretation, poses considerable problems. Traditional statistical methods such as factorial analysis are available to do this, but new methods of automatic classification and of data analysis have been developed which still call themselves statistics but do not rely on probabilistic models.
>
> (Ekeland, 1993: 167)

51 Ecology is currently developing new methods, in particular, understanding dynamic processes of evolution with inherent stochasticity, considering the characteristics of populations and not just of samples, and describing these universes using non-parametric methods. This may provide insightful inspiration for social sciences, given the centrality of the same type of problems: evolution through time, and complexity emerging out of interactions between agents.

9 Chaos or randomness: the missing manuscript

1 Frisch had lived in France while studying and preparing his Ph.D., which was originally written in French. He was quite aware of the academic landscape of France, which had one of the best mathematical schools, and was acquainted with some of the most influential scholars.

Frisch thought the invitation to give the Poincaré lecture had been an initiative of Divisia's. He was wrong about that: Divisia thought the invitation had come from Fréchet or Darmois and advised him to accept (Divisia to Frisch, 26 November 1931). In fact, the invitation had come from Georges Darmois (Frisch to Darmois, 20 February 1932).

2 Von Neumann and Ulam published the first account of the tent map in the unit interval in the *Bulletin of the American Mathematical Society*, 1947, 53.

3 In spite of this, the manuscripts of the seven lectures were never published. This was not uncommon in Frisch's academic career: quite often, after finishing a piece of research, he would put it in the drawer rather than publish or even informally circulate it. In the case of his last lecture, it could not be found either at the Frisch Archive of the Oslo University, which is very well organised, or at the archives of the Poincaré Institute.

4 This last lecture was an addition to the previously defined syllabus. Indeed, eight lectures were suggested to Frisch on 15 November 1932, but as late as 1 March 1933, the title and content of the last lecture were still undefined (correspondence between Frisch and the IHP). Consequently, it was decided upon by Frisch only a few weeks beforehand.

5 This lecture was not available to anyone other than his students and is written in Norwegian: like the other lectures, it was never published.

6 The difference between Darwin and Lamarck as presented by economists is frequently a misrepresentation of their positions and leads to a telescoping of their respective scientific programmes (Louçã, 1997: 82–3). But here dominance of a natural process (selection or adaptation) is sufficient for the identification of their respective alternatives.

7 Slutsky himself mixed the notions of chaos and randomness, as did most scholars at that time: 'In our case we wish to consider the rise of regularity from series of chaotically-random elements because of certain connections imposed upon them' (Slutsky, 1937: 106).

8 An unconventional scientist, Stephen Jay Gould, puts it another way and argues that reducing understanding to determinism is an expression of our fear that randomness means the non-causality of our world:

> Perhaps randomness is not merely an adequate description for complex causes that we cannot specify. Perhaps the world really works this way, and many happenings are uncaused in any conventional sense of the word. Perhaps our gut feeling that it cannot be so reflects only our hopes and prejudices, our desperate striving to make sense of a complex and confusing world, and not the ways of nature.
>
> (Gould, 1981: 349)

9 It became famous not only for denying the self-equilibrating property of capitalism, therefore predicting the collapse of the free market, but also because, and more trivially, it is the longest paper ever published in *Econometrica*.

10 Is capitalism doomed? A Nobel discussion

1 Collapse here means the very large cyclical fluctuations, with enormous sales one moment and enormous losses in the next period. The criterion adopted for declaring collapse is the breakdown of the Excel computation. It is hypothesised that, under these circumstances, the agents would be expelled from business.

2 Frisch to de Wolff, 15 October 1935. De Wolff, in correspondence with the author (September and October 1998), indicated that Koopmans also told him about this episode. As De Wolff joined in 1936 the Central Bureau of Statistics, led by Tinbergen, he might have also been informed by Tinbergen.

3 Frisch to de Wolff, 14 December 1935.

4 Letter from de Wolff to the author, 13 October 1998.

11 Prometheus tired of war

1

> The climax culminated in Galton's preaching of Eugenics, and his foundation of the Eugenics Professorship. Did I say 'culmination'? No, that lies rather in the future, perhaps with Reichskanzler Hitler and his proposals to regenerate the German people. In Germany a vast experiment is in hand, and some of you may live to see the results. If it fails it will not be for want of enthusiasm, but rather because the Germans are only just starting the study of mathematical statistics in the modern sense.
>
> (quoted in Klein, 1997: 185)

Hitler was of course doing much more than adapting Germany to modern statistics.

2 'James Meade, a convinced Keynesian and a moderate socialist' (Harrod, 1951: 501). According to Arrow, Hotelling defended a 'mildly socialist ideology' (Mirowski, 2002: 298). As he was active in the anti-Nazi resistance, Reiersol fled to Sweden in 1943, remaining there until the end of the war, when he returned to join a research group led by Haavelmo (Reiersol, 2000: 114). Klein stated that he was mainly interested in formulating a system that would compare the Keynesian and Marxian equations (Klein, 1987: 414). When he joined the Cowles Commission in 1944, Klein was a member of the Communist Party (Mirowski, 2002: 246–7).

3 Andvig indicates that, later on, when a coalition of right-wing parties was elected in the summer of 1963, Frisch offered to provide economic advice (Andvig, 1986: 30). But this seems to have been an exception. As he opposed the 'enlightened plutocracy' that the Labour Party was complying with, Frisch wrote twelve articles in the paper of the Sosialistik Folkesparti (Socialist Left Party), formed as a result of a radical split from the Labour Party, in the 1960s; on 19 August 1969 he produced a statement supporting that party. 'He was one of those rare men who grow more politically radical with the years', writes Andvig (ibid.: 17).

4 Andvig explains this radicalisation by comparing Frisch and Wicksell:

> Both might be said to belong to the radical part of the bourgeoisie, both were preoccupied by problems they believed everybody had to be interested in solving (inflation and unemployment, respectively), problems created by almost invisible and anonymous forces within the circulation system, that among other things caused unnecessary strife between the different economic classes.
> (Andvig, 1988: 165–6)

One of these 'anonymous forces' was the 'ignorant monetary plutocracy'. Wicksell was the pessimist, believing that there was no institutional framework capable of responding to scarcity; Frisch was the optimist, considering that the problem was a matter of organisation, and therefore avoidable.

5 Frisch to Bernal, 17 June 1965.

6 As previously stated, Tinbergen shared with Frisch the first Nobel Prize in economics. His brother, Nikolaas Tinbergen, a zoologist and ethologist, shared the Nobel Prize for Physiology and Medicine four years later, in 1973, with Karl von Frisch and Konrad Lorenz.

7 Tinbergen to Wicksell, 22 June 1925 (quoted in Jolink, 2003: 16fn., 84).

8 Koopmans wrote to Tinbergen immediately after graduation:

> Two weeks ago I graduated from theoretical physics with Professor Kramers at Utrecht. However, I seem to be taking the same route as you have done in the past: although in principle I find physics a beautiful field, I am too concerned with the social problem to be able to devote myself completely to theoretical physics. I therefore consider the possibility to use the mathematical developments I possess in the study of economic and statistical problems. . . . Would you be so kind as to allow me to visit you to discuss these issues?
> (Koopmans to Tinbergen, 18 July 1933, quoted in Jolink, 2003: 77–8)

9 The Menshevik party was the former minority of the Russian social democratic party, the majority being the Bolshevik faction.

10 Frisch noted Schumpeter's opposition as being politically biased:

> I take it that your reference to Marschak being biased in his selection of Fellows, means that Marschak is a Socialist and that he therefore is trying to get Socialists into the picture. I knew that Marschak is a Socialist, but I have a very strong impression that in the matters of the Econometric Society he is guided uniquely by scientific motives.
> (Frisch to Schumpeter, 12 November 1932)

Schumpeter responded very soon afterwards:

> No. You do me an injustice: I am not so narrow as to object to anyone because he is a socialist or anything else in fact. If I did take political opinion into consideration I should be much in favour of including socialists in our list of Fellows. In fact, I should consider it good policy to do so. Nor am I or have I ever been an anti-Semite. The trouble with Marschak is that he is both a Jew and a socialist of a type which is probably unknown to you; his allegiance to people answering these two characteristics is so strong that he will work and vote for a whole tail of them and not feel satisfied until we have a majority of them, in which case he will disregard all other qualifications. This is in the nature of a difficulty. But personally I like him immensely and I think a lot of him.
> (Schumpeter to Frisch, 3 December 1932)

This was not satisfactory for Frisch: 'I must admit that I am very surprised about your views on Marschak' (Frisch to Schumpeter, 11 January 1933). After the first selection of Fellows, Frisch insisted on including Marschak in the next group to be elected.

11 Schumpeter to Cowles, 6 May 1937. As he was quarrelling with Frisch over the nomination of Marschak, Keynes asked Schumpeter's opinion about the suitability of Lederer for the job of correspondent of the *Economic Journal*. Schumpeter advised against him:

> Now you have asked about him I find it much easier to do so: he is a party man of a type which obeys orders without asking a question. And in all matters which can be brought into any relation at all with politics he is absolutely unable to see except through party glasses. I hope you will believe me if I say that it is not his belonging to the socialistic party which caused my qualms. I should have felt exactly the same difficulty about any other strong party man who reacted on the party type in this particular manner.
> (Schumpeter to Keynes, 3 December 1932)

12 It is fair to say that this was not the end of the story: later on, when Marschak could not find a suitable job, Schumpeter wrote letters of recommendation to Columbia (12 March 1939) and Berkeley (6 April 1939).

13 Frisch to Bowley, 3 May 1933.

14 Schumpeter to Mitchell, 19 April 1933.

15 Schumpeter to Day, 2 May 1933. In spite of his anti-Semitism, Schumpeter rejected the discrimination against Jews: for instance he supported the appointment of Samuelson against the opposition of an anti-Semitic head of department, but his private diary included several anti-Jewish and very racist remarks. His anti-Semitism was, by the way, mildly shared by Keynes (Moggridge, 1992: 609), in spite of his irreproachable friendship with Kahn, Sraffa or Leonard Woolf.

16 Schumpeter to Frisch, 25 February 1933.

17 Schumpeter to Haberler, 20 March 1933.

18 Schumpeter to Day, 2 May 1933.

19 It is fair to say that none of his students rallied the Nazis and his former secretary and mistress in Germany, Maria Stockel Bicanski, joined the underground and was shot by the Nazis.

20 Schumpeter's last years were marked by long periods of depression, probably motivated by the turn of world events, namely the Second World War which was destroying Europe, and mainly by the dramatic loss of his second wife and child in 1926 (Allen, 1991, I: 236).

21 Frisch to Neyman, 7 March 1938.

22 Hansen to Frisch, 12 December 1931.

23 Frisch to Hansen, 20 February 1932. The causes of the depression were, for Frisch, the unequal distribution among industrial sectors and social sectors, provoking underconsumption. This required an optimal distribution and therefore planning.

24 Frisch to Tinbergen, 26 March 1936.

25 Frisch formally addressed the lack of coordination not only in this paper (1934a), but also in a private debate with Tinbergen and Koopmans the following year, during the Namur conference of the Econometric Society, which was the main topic of the previous chapter. This example is particularly telling since it represented an attempt to represent the structure of the interactions of the different agents in a nonlinear framework. But this proved to be too much for the prevailing mathematical techniques.

26 Despite having contributed to the definition of a sort of Keynesian policy, with his 'oil brake principle' of anti-cyclical policy, Frisch suspected the viability of the steering control mechanisms, since he believed that no parametric stability existed and therefore that there was no basis for prediction. Much later, in 1958, Frisch again insisted that predictions were untrustworthy:

> I have personally always been skeptical of the possibility of making macroeconomic predictions about the development that will follow on the basis of given initial conditions. . . . I have believed that the analytical work will give higher

> yields – now and in the near future – if they become applied in macroeconomic decision models where the line of thought is the following: 'If this or that policy is made, and these conditions are met in the period under consideration, probably a tendency to go in this or that direction is created'.
>
> (quoted in Andvig, 1995a: 11)

27 In spite of this optimism, no precise guidelines were presented in order to make the proposal concrete. It appears to have implied a voluntary participation in a rationing scheme:

> To a certain extent the technique in the organization of such an exchange service will resemble that which was employed in the rationing during the war. There is, however, the difference that the exchange service should be based on voluntary participation, and must embrace many more kinds of goods and services. It will first obtain its full effectiveness when there is a comparatively large choice of goods and services.
>
> (ibid.: 323–4)

28 Roos to Frisch, 13 February 1935.

29 Frisch to Cowles, 11 July 1937. Andvig argues that this was the last effort by Frisch in macroeconometric research (Andvig, 1988: 496). After the attempt to get Cowles's support for the 1937 project, Frisch devoted his attention to decision models.

30 When the Econometric Society meeting was called for 10–17 September 1947, in Washington, the first draft of the programme (7 January 1947) included a session on 'econometrics as an aid to policy decisions', and Frisch, Tinbergen and Richard Stone's papers were scheduled for the discussion. At other sessions, a paper by Fréchet on 'the possibility and limitations of the application of probability theory in the field of social and economic phenomena (based on a questionnaire)' was to be discussed by Marschak (who would also present an argument on 'statistical inference from non-experimental observations'). But the second draft of the programme (20 March 1947) abandoned the idea of holding the session on 'aid to policy decisions' (letter written on 21 March by the Programme Committee – Koopmans, Leavens, and chaired by Marschak). Topics on similar themes eventually appeared at some of the next meetings of the Society, such as the one held on 10–12 September 1959, in Amsterdam (a session on the 'use of econometric methods for policy purposes'), with Tinbergen as chair and Frisch, de Wolff, Greniewski and Lesourne, but the focus of attention was already elsewhere.

31 This may be interpreted as an anticipation of Lucas's critique: if the governments act systematically, the agents will anticipate and therefore their action is inconsequential since it is not an autonomous function (Andvig, 1995a: 24, 50fn.).

32 The Ecocirc system was a model developed in 1942 and used in the autumn of 1943, since the Germans wanted to use it to justify future claims for war damage against the Allies. It was instrumentally used during the occupation by Gunnar Jahn, the director of the Statistical Bureau and a top member of the resistance, in order to justify opposed claims (Bjerve, 1998: 539).

33 In the preface to the Oslo Channel Model, Frisch gave an equivocal presentation of Soviet planning:

> It is my deepest conviction that if this situation continues, the West will be hopelessly lost in its competition with the East. The outcome will be the end of the Western kind of democracy. . . . It suffices [for the Soviets] to let the West continue in its stubborn planlessness. It will then rapidly be lagging behind economically and will in due time fall from the tree like an overripe pear.
>
> (Frisch, 1962a: 97)

It is obvious that his intention was not to emulate this sort of planning, but to argue in favour of his own model.

34 Frisch's proposals for this new orientation of economic policy were not published any more in *Econometrica*, the journal he had edited for two decades. The 'Oslo Refi Interflow Table' was published in the *Bulletin de l'Institut International de Statistique*, after it had been presented to a Paris meeting of that Institute in 1961. The same thing happened to other contributions of this type.

35 Malinvaud argued that Frisch did not participate in the discussion in Europe about 'enlightened planning', having ignored the contributions made by Hayek, Lange, Mises and Kantorovich (ibid.: 563). This is true. But the criticism also extended to Frisch's scholarship:

> Like his Norwegian students, I was fascinated by many of his ideas about our discipline, its relevance and its scientific methodology; but I could also be worried about what I perceived to be a lack of realism; in particular, I could be irritated when he began to discuss the technical details of mathematical programming, on which he spent so much of his time and effort. Great men have their weaknesses.
>
> (Malinvaud, 1998: 575)

36 Frisch and Allais had had a previous confrontation at a conference in Paris in 1955 on different nonlinear models, in which Allais, Kaldor, Goodwin and Hicks had read papers. In the discussion, Frisch presented his doubts on models that were unable to fulfil the criterion of predictability. He argued that there is a difference between meteorology and physics in terms of prediction, and that economics should seek to draw closer to the latter.

> He went on to say that the latter sort of prediction is what we should be trying to do in econometrics, because he felt that we were entering more and more into a planned economy. As ever, Frisch was pointing to studies of repercussion and thought that we should be concentrating on research efforts in that direction.
>
> (Klein, 1998: 494)

37 Roy to the Econometric Society, 1 August 1963.

38 On 18 December 1970, a large group of French researchers, mostly from Cepremap and including Grandmont, wrote a letter to the scientific community plainly calling for a boycott of the meeting. At that time, sixteen activists from the Basque country were on trial at Burgos and risked a death penalty (they were later effectively sentenced to death); the French economists asked for a campaign of letters to be sent to Debreu, then the president of the Society. Frisch (correspondence is from Frisch's Archive) was very quick to respond to the call and, on 8 January 1971, wrote a harsh letter to Debreu:

> I inform you that I will not attend the next European meeting of the Econometric Society if it takes place at Barcelona as scheduled. The international public opinion would interpret our presence in Barcelona as an implicit support of Franco's regime, which is responsible for the scandalous trial of Burgos. I therefore ask the Econometric Society to change the place of the meeting to another country.

The same day, he informed his French colleagues that they could add his name to a declaration saying that the condition for attendance was the liberation of the 16 imprisoned militants. The tone of the discussion – and the power of the parties involved in the argument – was rather different from the one that had taken place in relation to Jerusalem, and this was possibly why Frisch and his side chose to invoke the boycott from the very beginning.

Debreu was blunt. His speedy answer, sent on 25 January 1971, brought the discussion to an end: the ruling bodies of the Society stood united in relation to their previous decision and preferred to ignore the objections.

> On learning about the protest against the Burgos trial expressed by several members of the Society, Professor Drèze and I undertook to consult the Executive Committee and the Standing Committee on European Meetings of the Society comprising together fifteen persons. At this date I have still not heard from one of them. The fourteen persons who responded to our inquiry are unanimous in their belief that present circumstances do not warrant a change of location for the September 1971 meeting. The members of the Executive Committee and of the European Standing Committee were guided by the following principles. An international scientific society must not take an artificial stand on the political structure or the political acts of any country and it must be in a position to hold meetings in the largest possible set of countries. The choice of a particular location must naturally take into account the degree of success with which the society can be expected to achieve its scientific purpose in that location at a given date. On the basis of the information presently available to the two Committees, it is their judgement that the Barcelona meeting will successfully attain its scientific objectives. In reasserting the previously made decision about the location of the September European Meeting, I deeply regret that you will not attend it.

In the discussion following a paper anticipating the argument of this chapter, Roy Weintraub pointed out that at least one Econometric Society meeting, to be held in 1968 in Chicago, had been moved to another location for political reasons, following the anti-Vietnam War demonstrations that had surrounded the National Convention of the Democratic Party. Consequently, there were at least several precedents, and not only that of Jerusalem and Rome.

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