6 MATHEMATICAL FORMALISM AND ECONOMIC EXPLANATION

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It seems to us nowadays a very simple thing to assign dimensions to magnitudes, so simple that we are apt to forget the extremely important implication of the assertions. When we assert that a certain derived magnitude always has certain dimensions, we are in fact asserting the complete accuracy of the law which determines that derived magnitude under all possible conditions. If there is any doubt whatever about the universality of the law, then there is a corresponding doubt about the dimension of the derived magnitude. . . .

—Campbell, 1957, p. 416

Investigate whether mathematical propositions are not rules of expression, paradigms—propositions dependent on experience but made independent of

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Because I found in the course of researching this paper that there was a large literature on the role of mathematics in economics, but most authors seemed unaware of any but their own contributions, I have appended an extensive bibliography which touches upon the issues raised in this paper.

> *it. Ask whether mathematical propositions are not made paradigms or objects of comparison in this way. Paradigms and objects of comparison can only be called useful or useless, like the choice of the unit of measurement.*
>
> —Wittgenstein, 1976, p. 55

> *It is difficult to contemplate the evolution of economic science over the last hundred years without reaching the conclusion that its mathematization was rather a hurried job.*
>
> —Georgescu-Roegen, 1976, p. 271

§1. Is there really nothing useful or novel to be said about the relationship between the study of economic phenomena and the casting of economic inquiry in quantitative and mathematical format? Everyone is fully aware that the trend over the last century has been toward ever greater mathematical sophistication as part and parcel of the professionalization of the discipline of economics. Everyone is equally aware that this trend has provoked periodic controversies over the meaning and significance of this conjuncture. Where awareness, or perhaps self-consciousness, is deficient is in the areas of the historical determinants of mathematical conceptualization, and of recent developments in the history and philosophy of mathematics.

Economists seem singularly oblivious to the forces that have shaped their present mathematical practice. While this undoubtedly serves as a bulwark against seizures of metaphysical loss of nerve, it also invites outsiders to indulge in sarcasm. Suppose a working economist were confronted by a well-known physicist, addressing a respected body of philosophers of science, who proceeded to state:

> ... the traditional view in this country from the time of Newton has been that science is the study of the nature of reality and different branches of science merely study different aspects of the same reality: Mathematics being the branch which is concerned with the quantitative aspects of reality exemplified in its simplest form by various kinds of measurements. Towards the end of the last century as a result of the interaction between logic and mathematics changes began to take place and pure mathematics moved along a path which seemed to diverge from the path taken by the other sciences. While physicists and engineers seemed to show a distinct preference for the mathematics of the older kind, practitioners of social and economic science, which have come into their own fairly recently, found that the new mathematics was quite useful in making both

their qualitative and quantitative arguments more obscure for laymen. (Sharma, 1982, p. 276)

Surely this aperçu is ill-tempered, and a bit of a low blow, but it is actually to be found in the pages of a very reputable philosophy journal, which has been known to print serious discussions of economic method on occasion. In this respect, 1982 seemed a year particularly notable for the airing of dirty laundry, with a Nobel laureate making essentially the same accusation in the pages of the most respected generalist periodical in the natural sciences (Leontief, 1982). Now, one could always write these little incidents off as a bad run of luck; but as a matter of form, economists would probably rather tend to think that a commitment to rational discussion would require a measured rebuttal; or at the very least, a little effort to sort out the issues. Suppose we decide to look for an answer. What lines of defense are deployed in the existing literature of economics?

§2. There are two generic responses. The first (which we shall dub Defense_1) is most readily accessible in Paul Samuelson's Nobel Prize lecture (Samuelson, 1972, p. 2). Tracing the pedigree of the idea to Joseph Schumpeter, he states that the "subject matter presents itself in quantitative form": that is, economics is held to be "naturally quantitative". In this view, while discussions of economic phenomena do not require any particular mode of discourse, the mathematical mode is the most convenient, because it is concise and well suited to the subject matter (Samuelson, 1952, p. 63). Further, although this extension is rarely stated explicitly, the quantitative character of the subject matter not only justifies mathematical formalism in general, but also justifies any subset of that formalism, since mathematics is presumed to be a unified and consistent body of technique *par excellence*.

Far from being original with Schumpeter, Defense_1 dates back to the earliest progenitors of a particular style of political economy, that associated with neoclassical economic theory. It can be found in the programmatic statements of the founders of that school; for example, in William Stanley Jevons' *Theory of Political Economy*:

> It is clear that economics, if it is to be a science at all, must be a mathematical science ... simply because it deals in quantities.... The symbols in mathematical books are not different in nature from language.... They do not constitute the mode of reasoning they embody; they merely facilitate its exhibition and comprehension. (Jevons, 1970, p. 78)

§3. The second defense of the mathematical method is somewhat more modern. (Let us call it Defense_2.) In this view, mathematical formalism is merely the imposition of logical rigor upon the loose and imprecise common discussion of economic phenomena. The efficacy of this regimen derives from the discipline of axiomatization. A major statement of this position can be found in another Nobel Prize lecture (Debreu, 1984). The most influential expositor of this defense was Tjalling Koopmans; his advocacy is worth quoting in the original:

> The appropriateness of mathematical reasoning in economics is not dependent upon how firmly or shakily the premises are established. Let us assume for the sake of argument that the attempt to establish premises or at least to explore their implications is worthwhile, that is, economics itself is worthwhile. In that case the justification for mathematical economics depends merely on whether the logical link between the basic premises economists have been led to make and many of their observable and otherwise interesting implications are more efficiently established by mathematical or by verbal reasoning. (Koopmans, 1954, p. 378)

Since it is deemed impractical for those who have invested the time and energy in mastering the special techniques of axiomatization to also offer a verbal restatement of their activities, in contrast to the first defense, no claim is made that mathematics is "just another language," or even that it is incumbent upon mathematical economists to communicate with the uninitiated (Koopmans, 1954).[1] No intrinsic link between the essence of economic phenomena and the character of mathematical and analysis need be postulated, because, in this view, mathematical formalism *is* logic (Koopmans, 1957, p. 143).

In Defense$_2$ economics is portrayed as a workaday card file of axiomatized formal models: prudent practitioners are encouraged to be agnostic concerning the principles of the draw of cards from an otherwise orderly deck. Here one detects a certain distaste about enthusiasms over correspondence to reality.

§4. These two defenses (like so very much else) did not originate in economics, but rather replicate earlier controversies in mathematics and physics. Defense$_1$, the claim that the ontology of phenomena is in its very quiddity patterned along mathematical lines, is found in Greek antiquity; in the eighteenth century, the success of celestial mechanics fostered the more widespread belief that, "The true system of the world has been recognized, developed, and perfected." This ontological manifesto, cut free from its previous moorings in theology, is echoed by many in our own time. For example, Eugene Wigner, the 1960 Nobel prize recipient in physics, has asserted, "The statement that the laws of nature are written in the language of mathematics was properly made 300 years ago; it is now more true than ever before" (Wigner, 1967, p. 288).

Defense$_2$ developed out of some controversies among mathematicians around the turn of the century (Kline, 1980, ch. XI). Concern over the logical foundations of mathematics led David Hilbert to found the formalist school, the purpose of which was to establish once and for all the certitude and dependability of mathematical proof techniques and practices. Hilbert believed that mathematics should not be comprehended as factual knowledge of the book of nature, but rather as a formal symbolic structure: abstract, austere, and without explicit reference to meaning. He specifically limited proof techniques to the rigidly confined manipulation of symbols according to previously validated formulas or logical axioms. The consistency of arithmetic itself, and hence of all of the other branches of mathematics, was to be settled for all time. "To the formalist, then, mathematics proper is a collection of formal systems, each building its own logic along with its mathematics, each having its own concepts, its own axioms, its own rules for deducing theorems, and its own theorems" (Kline, 1980, p. 249). Closely allied with the formalists was the set-theoretic school of Zermelo and Fraenkel. This school developed and extended the basic axioms of sets, from which it was hoped that all other mathematics could be derived. Some modern exponents of this school, a group of mathematicians writing under the collective pseudonym of Nicholas Bourbaki, have been particularly influential in this respect; many of their techniques and their attitudes are present in the mathematical economics of the Arrow-Debreu variant (Debreu, 1959, p. x; Samuelson, 1983).

Both of these justifications of the employment and efficacy of mathematics are inadequate and have been vulnerable to rational criticism. For our purposes, their deficiencies can be explored upon two levels: that of recent developments in the history and philosophy of mathematics, and that of the history of economic theory. Because it is more recent and, among economists, more commonplace, let us examine Defense$_2$ first.

§5. The formalist program in mathematics has been subject to paroxysms of doubt and dissension since the 1930s, although the tremors have yet to trouble any economists. The first crisis of self-confidence, which has now even been popularized in certain best-sellers (Hofstadter, 1979), is a set of propositions derived from the work of Kurt Gödel. The full import of Gödel's theorem is still a wellspring of philosophical controversy; thus prudence dictates that we rely only upon its least contentious interpretation (Nagel and Newman, 1958).

Gödel showed that a consistency proof of a system meeting Hilbert's requirements and simultaneously strong enough to formalize arithmetic could not be given within that same system, assuming, in fact, that the system was consistent. This followed from Gödel's Incompleteness Theorem, which stated that, given any set of axioms for a system strong enough to express arithmetic, there exist sentences true in arithmetic which are formally underivable within that system. If any of these sentences are incorporated into the system as axioms, they then become trivially derivable, but then there exist other unprovable but true sentences. Thus, "mathematical proof" does not inevitably coincide with the use of a formalized axiomatic method. The demonstration that, "there are limitations on what can be achieved by axiomatization contrasts sharply with the late 19th century view that mathematics is coextensive with the collection of axiomatized branches.... As Paul Bernays has said, it is less wise today to recommend axiomatics than to warn against an overvaluation of it" (Kline, 1980, p. 263). The existence of such "formally undecidable propositions" to a certain extent undermines the "law of the excluded middle", and with it, much of the faith that mathematics is less fuzzy than the conventional vernacular.

A second attack on the formalist program came, not from within mathematics, but from philosophers associated with the ideas of the later Wittgenstein (especially, Wittgenstein, 1978). Although Wittgenstein's aphoristic writings prevent even the enthusiast from claiming his philosophy prosecutes a unified thesis, the aspect of it relevant to the formalist program is his critical exploration of the belief that mathematical practices could be unambiguously codified in axiomatic systems. One aspect of mathematical practice that he subjected to scrutiny was the role of ostensive definition and the rational persistence of unintended interpretations in an axiomatic system.[2] A second aspect, taken loosely, is that any collection of rules, such as a system of axioms, is incapable of definitively enforcing itself. To imagine otherwise is to descend into an infinite regress of the sequential postulation of rules whose purpose is to enforce certain interpretations of higher-level rules, and so on, *ad nauseam* (Levinson, 1978; Mirowski, this volume, ch. 7). Again, it is not the internal research project of mathematics that is deflated by these events, but rather the widespread confidence that mathematics embodies obvious superiority in the areas of consistency, clarity, and limpid communication.

§6. It would seem a worthwhile project to try to bring the recent history of metamathematics to the attention of economists, if only because one so frequently hears that mathematical methods constitute a neutral language, which only conveys what is consciously put into it. This belief is then further confounded with some auxiliary notions of the inherently value-free character of a class of theories. Again quoting Koopmans:

> ... that the mathematical method when correctly applied forces the investigator to give a complete statement of assuredly noncontradictory assumptions has generally been conceded as far as the relations of the assumptions to the reasoning is concerned. To this may be added that the absence of any natural meaning of mathematical symbols, other than the meaning given to them by postulate or by definition, prevents the associations clinging to words from intruding upon the reasoning process. (Koopmans, 1957, pp. 172–173)

Gödel's theorem alerts us to the fact that completeness and consistency are by no means as straightforward and effortless as here supposed. Again, let us try to be as clear as possible about what Gödel's theorem does and does not mean in this instance. It does not mean that there is anything illegitimate about the use of any particular branch of mathematics in order to make or illustrate a thesis in economics. It does mean, however, that the simple fact of the employment of mathematics in economic arguments cannot guarantee that the exhibition of assumptions is somehow more complete or less disingenuous than in the conventional vernacular. It does mean there is no certainty that the list of assumptions will not need augmentation in the future. It does mean that the Leibnizian dream of a universal algorithm has been severely tarnished in the twentieth century. These facts directly contradict Koopmans' assertions quoted above.

Further, the philosophical work of Wittgenstein cautions us to pause and wonder if those mathematical symbols really are so very free of clinging associations. The austere and asocial nature of mathematics sounds a little odd, coming out of the mouth of a social scientist. Koopmans' advocacy of the mathematical method makes it sound too much like snake oil, a universal panacea for all fuzzy thought. In therapeutic contrast, the metamathematical tradition sounds the tocsin that the axiomatic formalist method can be *potentially* strewn with pitfalls. These rather abstract arguments can be brought down to earth by means of a detour through the history of economic thought; it can provide the concrete counterexamples to Koopmans' assertions.

Interestingly enough, we can pick up the scent of the trail in Koopmans' own work. Almost in passing, he comments that, "A utility function of a consumer looks quite similar to a potential function in the theory of gravitation ...". (Koopmans, 1957, p. 176). Although he opts not to elaborate on that statement, let us explore it further. Suppose we are describing a mass-point moving in a three-dimensional Euclidean space from point A to point B, as in figure 6–1. The conventional method of describing this motion, developed in the early- to mid-nineteenth century, would postulate a 'force' decomposed into its orthogonal components, multiplied through by the spatial displacement of the mass-point, also suitably decomposed. In order

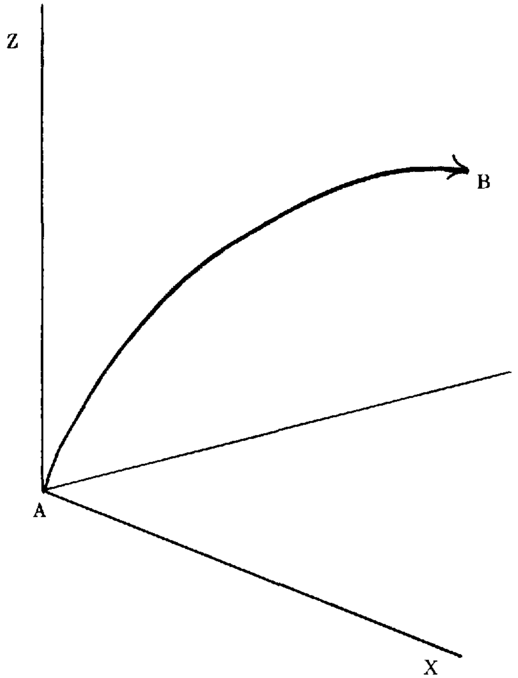

Figure 6–1

to incorporate cases of nonlinear displacement and acceleration, the "work" done in the course of the motion from A to B was defined as the summation of the infinitesimal forces multiplied times their displacements:

$$T = \int_A^B (F_x\,dx + F_y\,dy + F_z\,dz) = (\tfrac{1}{2})mv^2 \Big|_A^B$$

The writings of Lagrange, and more importantly of Hamilton, argued that the total energy of this system also depended in a critical way upon the *position* of the mass-point. This was subsequently clarified in the following manner: suppose that the expression $(F_x\,dx + F_y\,dy + F_z\,dz)$ was an exact differential, which implies that there exists a function $U(x,y,z)$ such that

$$F_x = (\delta U/\delta x); \qquad F_y = (\delta U/\delta y); \qquad F_z = (\delta U/\delta z)$$

The function $U(x,y,z)$ defines a gravitational field, which later was identified as "potential energy". The sum of potential and kinetic energies, $T + U$, was then understood as being conserved within the confines of a closed system. This conservation law, in turn, clarified and encouraged the employment of constrained maximization techniques (such as the Principle of Least Action, Lagrangean multipliers, and the Hamiltonian calculus of variations) in the description of the path of the mass-point under the influence of the impressed forces.

As Koopmans indicates, the similarity between this model and conventional neoclassical price theory is quite striking. In fact, if one merely redefines the "forces" to be prices, the displacements to be infinitesimal changes in the quantities of individual goods x,y,z "kinetic energy" to be expenditure; and relabels the gravitational potential field to be "utility", then one arrives at the standard model of neoclassical price theory.[3] Constrained maximization (or minimization) of an imponderable quantity leads directly to a conservative field, which in turn fixes the permissible configurations of forces/prices.

Is this remarkable similarity merely an accident? Koopmans is silent on this issue, but examination of the origins of neoclassical theory reveals that its progenitors consciously and willfully appropriated the physical metaphor and imported it into discussion of economic theory in order to make economics a mathematical science (Mirowski, 1984b). The most curious aspect of this program to make economics more rigorous and more scientific is that *not one* neoclassical economist in over one hundred years has seen fit to discuss the appropriateness or inappropriateness of the adoption of the mathematical metaphor of energy in a prerelativistic gravitational field in order to discuss the preferences and price formation of transactors in the marketplace. This lacuna raises two immediate questions: do neoclassical economists have any inkling whence their favored mathematical techniques are taken? And, if one accepts the account of the matter that says they were directly appropriated from physics, has this really made any significant difference either for the subsequent evolution of research or for the communication of the results? Meticulous exploration of these questions should serve to reveal the lack of foundations of the allegation that the mathematical method encourages "a complete statement of assuredly noncontradictory assumptions" and "prevents associations clinging to words from intruding."

§7. In answer to the first question, neoclassical economists generally have no coherent conception of the genesis of their mathematical techniques, nor, indeed, of the extent of the similarities of their practices to those in physics. This is evident from the singular deterioration of the level of discourse whenever the role of mathematics in the development of economics is broached: on these occasions the preacher buried within the economist's husk comes bursting forth with homilies about elegance and progress and science and truth and efficiency. The actual context of use and meaning is the first casualty in the rush to testify the faith; the second casualty is any curiosity about what contemporary mathematicians and physicists are saying about how they use mathematics. Examples of these shortcomings can be found in assertions that the definition of economy as the maximization of an objective function under constraint was an "obvious" inference after the invention of the calculus (conveniently ignoring the two-century lag), and in assertions by some eminent neoclassical economists that economics has no analogue to the conservation of energy, nor, indeed, to any aspects of physics at all (see Mirowski, 1984c).

There is an irony at the heart of this stubborn oblivion on the part of the partisans of Defense$_2$. Their method of axiomatic formalism, which they tout as being utterly transparent and logical, seems to elude all of their attempts to discuss it transparently and logically. The method which reveals the clinging associations of words is incapable of revealing its own clinging associations: its own employment cannot be justified on its chosen grounds. Those familiar with the philosophy of mathematics will recognize this as another example of a general phenomenon of the twentieth century, examples of which include the Russellian paradoxes of set theory, as well as Gödel's theorem: indirectly self-referential systems are the bane of the formalist program.

§8. And now, to the second question: the fact of the appropriation of mathematical techniques from the physics of the nineteenth century has clearly influenced both the content and the mode of research in economics. These clinging associations are the residue of the projection of a metaphor from physics onto the sphere of the economy, where aspects of economic experience were then subject to reinterpretation. An enumeration of the myriad ways in which the mathematical model smuggled a hidden agenda into political economy would be a very substantial undertaking in the intellectual history of the discipline. (See Mirowski, forthcoming.) Our pretensions are more modest in the present context, and therefore, we shall have to rest content with approaching this problem in stages, weaving it together with a parallel evaluation of the defenses of mathematics in economics. To this end, in this section we shall provide a preliminary assay of the physical residue in economic theory, only to delve more deeply into issues of mathematical structure in sections §10 and §18–20 below. The following is a list of suggestions as to the multifarious influences of the physical metaphor:

A. There is nothing obvious about the definition of human rationality as the maximization of an objective function over a stationary field (Mirowski, this volume, ch. 7). This elevation of the significance of extrema did not arise first in social theory, but rather in physics, as the principle of least action. The physics of constrained extrema was interpreted as evidence supporting the existence of a God who had constructed the world in the most efficacious and coherent manner. This minimization or maximization was global in the most comprehensive sense, and encouraged the attitude that "efficiency" could be defined within some absolute framework. In its evolution from Maupertuis to Euler to Hamilton, the principle of least (or varying) action shed its theological skin, but the notion of the efficiency of extrema persisted, and it was this connotation which was recruited to tame the multiform and unruly phenomenon of rationality. The predisposition of the modern economist to "optimize" over someone's "objective function" is neither an empty tautology nor a harmless metaphor: it presumes an inordinately large amount of structure about the nature of desires and objectives, the role of time, the understanding of causality, the unimportance of process, the conservation of the domain of the objectives, the relative constructs of the world of the actor and its reconstruction by the social analyst, the separation of the phenomenon and the act of choice, and much, much more (see Bausor, this volume, ch. 4; and Mirowski, 1984c).

B. The metaphor of energy/utility which neoclassical economics appropriated was derived from the physics of a specific historical period: the years of the mid-nineteenth century just prior to the elaboration of the second law of thermodynamics. The mathematics of pre-entropic physics was the pinnacle of the development of static mechanism (Prigogine, 1980), where all physical phenomena are portrayed as being perfectly reversible in time, and no system exhibits hysteresis. Nineteenth-century physical laws were thought, by definition, to possess no history. The stubbornly ahistorical bias of neoclassical economics has been excoriated by critics such as Joan Robinson, and bemoaned by such partisans as Hicks (1979) and Shackle (1967). What the latter do not realize is that one cannot superimpose a history onto neoclassical processes without undermining the physical metaphor and the mathematical techniques that were the cause of its success.

C. In pre-entropic physics, all physical phenomena are variegated manifestations of a protean energy which is fully and reversibly transformed from one state to another. When this idea was transported into the context of economic theory, it dictated that all economic goods were fully and reversibly transformable into utility, and thus into all other goods through the intermediary of the act of trade. The introduction of money into neoclassical economic theory has always been tenuous and tentative, at best (see below, section §18; and Clower, 1967). The problem has been, strangely enough, metaphorical. In the mathematics, the analogue to money has not been the lubricant that reduces the friction in a mechanical system; it has been rather a superfluous intermediate crypto-energy which all other energies have been constrained to become in transit to their final state. The mathematics says one thing; the accompanying commentary another.

D. As a prerequisite for the application of constrained extrema techniques in physics, it has long been recognized that energy must be conserved as a mathematical rather than an empirical imperative (Theobald, 1966). A major theme in Western economic theory has been persistent controversy over what should be conceptualized as being conserved in the economy. Neoclassicals, in opting for the mathematics of energetics, have implicitly chosen the utility field to be conserved. Lack of self-consciousness concerning this choice has resulted in no end of muddle in neoclassical discussion of such issues as knowledge and uncertainty (Bausor, this volume ch. 4).

E. There was a flurry of activity in the 1940s and 1950s which portended the liberation of neoclassical value theory from any dependence upon the utility concept. The motivations behind this self-denying ordinance were never seriously aired, although a rationally reconstructed history (Wong, 1978) can easily be clarified by making a list of the various ways our understanding of the folk-psychology of utility renders it dissimilar to energy. (Parenthetically, it also can explain why economists cannot be bothered to take twentieth-century psychology seriously.) The failure of this abortive research program can be gauged by the extent to which the axioms of revealed preference are isomorphic to those of a gravitational field.[4]

F. Problems with the energetics metaphor sometimes assumed a very prosaic cast. For example, the components of forces can take on negative values without disrupting the physical intuition; but negative prices seemed to be pushing the analogy a bit too far. We shall return to this issue in section §10 below.

The more one is willing to become embroiled in the history of physics and mathematics, the more one could expand this list. For our present purposes, I presume it offers sufficient evidence to counter the claim that it makes no appreciable difference where mathematical analogies and techniques come from, because once appropriated, they are freely amended to express only what is transparently intended. At least in this respect, mathematics is not a colorless and secure coat into which the analyst can slip in order to shield himself from the vagaries of human discourse.

§9. There is another respect in which mathematics cannot be a neutral vehicle for abstract thought. Mathematics not only influences what is to be said; it also influences to whom you can speak. In retrospect, it seems clear that the physico-mathematical origins of neoclassical economics substantially shaped the structure of the nascent economics profession, thus determining what sort of person would be sanctioned to think about the economy. The defenders of mathematical neoclassical economics have always treated this fact with disarming ingenuousness. Alfred Marshall, the force behind the propagation of economic studies at Cambridge, wrote in the preface to the eighth edition of his *Principles*:

The new analysis is endeavoring gradually and tentatively to bring over into economics, as far as the widely different nature of the material will allow, those methods of the science of small increments (commonly called the differential calculus) to which man owes directly or indirectly the greater part of the control that he has obtained in recent times over physical nature. It is still in its infancy; it has no dogmas, no standard of orthodoxy . . . there is a remarkable harmony and agreement on essentials among those who are working constructively by the new method; and especially among such of them as have served an apprenticeship in the simpler and more definite, and therefore more advanced, problems of physics. (Marshall, 1920, pp. xvi–xvii)

But of course there was a dogma and a standard of orthodoxy: that was why agreement had been achieved so rapidly and so painlessly by the early neoclassicals: the standards, the metaphors, and the very gestalt of a specific mode of theorizing had been imbibed during an apprenticeship in physics or engineering (Mirowski, 1984b). While those on the wrong end of the bayonet in the marginalist revolution regarded that cadre as if they had dropped from another planet with their symbolic quantification of qualities, their abstract optimization, and their haughty wielding of the saber of science, the revolutionaries themselves immediately recognized each other as comrades in arms. Some, with Marshall as the premier example, tried to justify the revolution to the larger populace; but by then it was already entering its first phase of consolidation.

Perhaps the most important aspect of the mathematization of any intellectual project is its sociological impact upon the membership of the discipline. It is only fairly recently that the issue could be seriously discussed (Bloor, 1973, 1978, 1983; Colvin, 1977). One can only speculate as to the sublimations and fears that acted to place mathematics beyond the pale in any discussion of the social influences on science (Restivo, 1983), since evidence of the social functions of mathematics may be found as far back as the birth of modern physics. Few who revere Isaac Newton as the towering genius of Western science are aware that he originally composed his *Principia* in the popular vernacular so as to encourage its wide dissemination; but subsequent disputes with other natural philosophers prompted him to recast it into its now familiar mathematical format. Newton himself tells us in the *Principia*:

. . . considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore, to prevent the disputes which might be raised on such accounts, I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which could be read by those only who had first made themselves masters of the principles established by the preceding Books." (quoted in Westfall, 1980, p. 459)

Years later, Newton recited a similar story to his friend William Derham. He abhorred contentions, he said. "And for this reason, namely to avoid being bated by little Smatterers in Mathematicks, he told me, he designedly made his Principia abstruse." (Westfall, 1980, p. 459)

Such revealing admissions concerning the sociological role and function of mathematics are common enough in the historiographic record, once one ventures beyond the hagiography of science.

Mathematics is a primary tool in the creation of a well-behaved audience for a particular discourse, the establishment of an orthodoxy which automatically serves to exclude dissension. It is a prosaic but nonetheless accurate observation that the time spent in mastering the mathematics and the translation of those symbols into the orthodox statements of a discipline is a regimen sufficient to discourage the skeptical and reinforce the self-esteem of the willing recruit.

How can a system of obscure symbols be responsible for the maintenance of orthodoxy? First, it is a restricted language, and like any such language (say, the Latin at a Roman Catholic Mass, or the jargon of Freudian analysis), it possesses a certain ritual efficacy over and above its content. Such a language expresses social relations by its very use, and independent of any conscious intent.

Secondly, mathematics is a singular sphere of human discourse where the assertion of the discreteness of intellectual constructs is pushed to its extreme, resulting in the most rigidly inflexible claims that the manipulation of concepts is either unambiguously correct or unambiguously incorrect. This construction of knowledge is particularly serviceable in the context of the classroom, where discipline and the hierarchical status of teacher and student are projected into the realm of knowledge itself. Once internalized, mathematics seems to police itself, sanctioning the correct application of its own rules. This undoubtedly accounts for the fact that most mathematicians would rather adopt Platonism than be confronted with the idea that they themselves participate in the construction of mathematics (Penrose, 1982). (Unfortunately, most mathematics classes are not conducted along the lines of Socratic dialogue as in the ideal world of Lakatos (1976).)

Third, the illusion of self-policing rules are reproduced in the social theories which depend heavily upon mathematical formalization. Many of the "constraints" binding the actors in mathematical social theories partake of the character of "natural" limitations because mathematics is incapable of encompassing the process of interpretation and the freedom and exhilaration of the redefinition of a problem context which is the prerogative of human rationality (Mirowski, this volume, ch. 7). This hemmed-in conception of the human dilemma would appear as more plausib'e to a mathematician than, say, a rhetorician, because it is precisely that freedom of interpretation which they were taught to curb in their schooling. To put this bluntly, mathematics fosters the impression that the actors who are the subject of analysis are determined by alien extraneous forces.

Fourth, the discrete character of mathematics encourages what Colvin and Bhaskar call "the norm of closure," which, briefly, signifies the creation of a system restricted in time and in space, in which a constant conjunction of events is maintained in the isolated ideal, and upon which is imposed a tendency to atomism and a prohibition of novelty by combination.[5] Colvin (1977) suggests that the norm of closure comes to transform the social structure of the discipline that embraces it. For example, research itself in such a discipline becomes more fragmented, and the issues themselves come to be seen as more and more discrete and isolated. The conviction gains currency that rigor is identical with the most extreme ontological individuation. The industrialization of research becomes more feasible and desirable, and the responsibility for the success or failure of the research program becomes diffused over large numbers of workers oblivious to the "big picture". A certain anomie sets in, with mathematical workers appealing ultimately to epistemologically vague "elegance" or "simplicity" as the prime justification for their endeavors. Legitimation in the field comes to be confused with the norm of closure, so there arises a very low threshold of toleration for debate which does not seem to be headed toward closure. Pronounced changes in the field also seem ominous.

Fifth, the penetration of mathematics induces a particular form of hierarchy within a discipline, where mathematical theorists become separated from a lower class of researchers whose task it is to connect the theoretical terms to empirical data and to reprocess the "highbrow" theory into "lowbrow" expositions and contexts. As Colvin (1977, p. 116) writes, "In this type of work differentiation, it is the theorist, rather than the experimentalist, who projects the capacities of arithmomorphism to the hilt, in that he is able to slide fairly casually over the domain of the grounds of reality, where the experimentalist might ordinarily hold sway, in order to reinforce the theoretical position of the arithmomorphic norm." Thus mathematics frees the theorist from having to create a context of justification. Hence it is notable that in the history of physics it has not been the literary natural philosophers but rather the mathematicians who have proposed some of the most outrageous analogies in the course of their endeavors: heat flows like a liquid; electricity and light undulate like waves on a pond; electrical inductance behaves like a gravitational mass; and so on (Olson, 1958). While these analogies often ride roughshod over the multiform particularities of the actual phenomena, their acceptance and adoption is encouraged by the separation of the discipline into mathematical theorists and applied practitioners. The mathematical theorists can disregard mundane phenomenological problems and choose descriptions they find agreeable because they resemble other existing mathematical constructs, however farfetched. The applied practitioners and experimentalists, aware of their second-class status (perhaps due to an earlier failure in pursuing the mathematical frontiers of their discipline), acquiesce in the analogies of the theorists and work to find some common ground between the phenomenon and the formalism. The relevance of this dynamic to the explanation of the appropriation of the physical metaphor of energy by neoclassical economists should be obvious.

§10. There is no more stark illustration of the difficulties of Defense$_2$ than the evolution of what is today widely considered the pinnacle of neoclassical economic theory, the Arrow-Debreu (AD) general equilibrium model. While it is the case that the AD model deploys some of the most sophisticated mathematics to be found in any branch of social theory, there has never been serious defense of the tenets that its axiomatic structure has improved the tenor and clarity of theoretical discussion or revealed exhaustively all of the necessary assumptions underlying the neoclassical world view, or even that the model adequately represents the issues that have vexed economists prior to its inception.

The history of the AD model is admirably summarized by Weintraub (1983). The progenitor of the model, Léon Walras, did innovate the inscription of utility functions and production functions, as well as invent the artifice of the market-clearing auctioneer, but his work contained many anachronistic features from classical economics, and his only attempt at providing a solution for the model consisted of counting the number of equations and unknowns. After an interval of neglect, a few mathematicians observed that the counting method did not guarantee the existence of an equilibrium solution consisting of a strictly positive set of prices and outputs (Baumol and Goldfeld, 1968, pp. 267–280). Abraham Wald in 1934 succeeded in providing the first proof of existence and uniqueness of a positive equilibrium price vector. The rather primitive techniques used in these papers—for example, the bald assumption of price as a monotone decreasing function of output, or the unjustified postulation of convergent sequences of Δ's (see Wald in Baumol and Goldfeld, 1968, pp. 281–287)—next attracted the attention of other mathematicians. John Von Neumann in 1944 shifted the premises of the inquiry by postulating global characterizations of objective functions and constraint sets, which in turn required solution concepts based upon the convexity of sets. In the early 1950s, there was a rush to apply further topological techniques, such as the Brouwer and Kakutani fixed-point theorems (see Takayama, 1974, pp. 260–265), in order to provide more "elegant" formalization of existence and uniqueness of competitive equilibrium. By the time of the contributions of Arrow and Debreu, there was no longer any pretense that the object was to find the solution to a set of equations or inequalities; there was, instead, the more modest goal of "proving that a number of maximizations of individual goals under independent restraints can be simultaneously carried out" (Koopmans, 1957, p. 60).

Even a cursory examination of this history reveals that "the box of tools", the language purportedly free of "clinging associations", took on a life of its own. Walras imported his model from physics without understanding a critical flaw in the analogy: negative solutions for forces and displacements are quite common in physical problems, and cause no problems of interpretation, because they are seen as representing the relative orientation of the phenomenon in space; negative solutions for outputs, and more significantly, prices, posed a much more sticky problem of interpretation in economics. Instead of seeing this as a seriously damaging drawback of the physical metaphor, or deciding that the algebraic structure of nineteenth-century physics was inappropriate in the context of the economic sphere, twentieth-century *mathematicians* sought to augment the model with more assumptions in order to banish the anomaly. Initial auxiliary hypotheses concerning inequalities and the free disposal of superfluous goods gave way to changes in the mathematical solution technique. These changes in technique, and the movement toward global and topological considerations, in turn altered the content and the goals of the research program. Physicists do not generally hold proof of existence and uniqueness of a solution to a model high on the roster of their research accomplishments, because they aim for constructive proofs. The movement toward nonconstructive proof techniques in economics was a portent of a larger-scale tectonic shift in the conceptualization of what an economic system did, and what an economist could hope to say about it.

One of the fault lines of this tectonic drift has been charted by Garegnani (1983), albeit in another context. He argues that the transition from classical to neoclassical economics was accompanied by a lagged transition from the notion of equilibrium as the "center of gravity" of a limited set of forces to the notion of equilibrium as a sequence of temporary market-clearing prices. Garegnani sees this shift as motivated by internal failures in the neoclassical theory of capital, but we would instead suggest that it is symptomatic of the wholesale reinterpretation of the economic system caused by the adoption of advanced mathematical techniques. The classical theory of competition and equilibrium sought to ground the phenomenon of market price in prior and independent physical determinants, which would fully characterize a long-run equilibrium price of production independent of transitory variations in demand (Levine, 1980; and this volume, ch. 2). The importation from physics of the metaphor of constrained extrema implied that physical surroundings be reinterpreted as a domain of free choice, rather than a self-contained determinate environment. The invariant point of departure for analysis was shifted from the world to preferences/energy. Walras and the other early neoclassicals attempted to retain the classical conception of competition as a center of gravity, but subsequent generations of economists realized that the very conception of *order* in neoclassical theory must diverge from that characteristic of classical economics (Mirowski, forthcoming).

The Western tradition of economic theory is united in its search for order amidst the seeming anarchy of the market; but throughout the parade of individual theories, harmony has been a very plastic notion. The order of the classical economists was a social arrangement sanctioned by natural law (i.e., the laws of physics and the presumed constancy of a given class structure) which was used to explain the reproduction and growth of a national economy. The neoclassical conception of order as represented in the AD model is the potential consistency of individual mental constructions sanctioned by personal constrained optimization over noneconomic initial conditions (i.e., stylized preferences and rules for the transformation of commodity identities, as well as endowments). The stark contrast between the two conceptions of equilibrium is marked by the fact that Arrow-Debreu systems must impose ad hoc auxiliary conditions that ensure individuals can subsist on initially given endowments without engaging in exchange, and that minimum consumption requirements are covered in equilibrium (Takayama, 1974, p. 264). Classical economics spoke the language of *persistence*, whereas the mathematics of energetics and constrained extrema dealt in terms of *invariance*. It is flatly not the case that neoclassical economists first decided that it was better to think of the economy as an aggregate of invariant preferences rather than a system of persistent social relations; instead, economists baldly mimicked physics and its attendant mathematical formalism, and then only discovered gradually that their world picture had to be strategically stretched and shrunk to conform to the metaphor of the transformation and the conservation of energy (Mirowski, 1984b, 1984c).

The transformations in the ideas of order, competition and equilibrium are thus the direct result of the adoption by neoclassicism of its characteristic mathematical techniques. Classical economics postulated a preordained equilibrium of nature which the market generally, but not invariably, acted to ratify. There was no requirement that all generic commodities had to trade at the identical price; nor, indeed, was there any imperative that markets continuously clear. Arrow-Debreu economics, on the other hand, can only implement its mathematico-physical metaphor by imposing the law of one price for any generic commodity and by defining equilibrium prices as those that clear the market.[6] A moment's reflection should reveal that heightened mathematical concern with constrained optimization implies the law of one price as a lemma; whereas preoccupation with proofs of existence and uniqueness in the presence of exogenous stable preferences ordains that the function of price is to clear the market.[7] The fact that it was the mathematics that came first and the economics second is demonstrated by the curiousum that in neoclassical textbooks the motivation for the law of one price is disposed of in a paragraph or less, while any discussion of the identification of equilibrium with the clearing of the market is relegated to the literature of industrial organization or the endless quest for the "Keynesian synthesis". However incongruous, these two pillars of neoclassical theory are introduced *en passant* by the inscription of p_i or $\Sigma_i p_i(D_i - S_i) = 0$. This happens just as unselfconsciously as a literary economist inadvertently introduces an unintended or ill-considered idea in a rhetorical flourish.

Not only has the mathematical development of the AD model altered the content of the theory; it has also continued to allow the sort of errors of economic reasoning that have been deplored in premathematical economic theory. A single example, albeit a somewhat important one, should suffice to demonstrate this thesis. The attraction of mathematical formalism resides largely in its promise of consistency. In the preface to one of the canonical sources of the AD model (Arrow and Hahn, 1971), the authors state their purpose is not the description of an actual economy. However, on page 346 they write: "... we must conclude that the failure of the market mechanism to establish equilibrium—if such failures are in fact observable—must be due to the elements of the actual economy that the economy of section 13.4 neglects." Now, the statements in the preface and on page 346 are patently inconsistent. One nonconstructive proof in one particular model that contains one ideosyncratic conception of the preferences of its agents—one of many possible descriptions of a technology—one incompletely specified definition of private property (Ellerman, this volume, ch. 3), in conjunction with one algorithm governing the adjustment of prices can in no stretch of the imagination absolve those rather large classes of phenomena from any responsibility for failure of actual market coordination. This is not an insignificant or minor non sequitur, however much it is mitigated by the coy reference to observation, because it brings us back full circle to the question:

what is the significance of the formalist program in economics? If it is asserted to have no necessary significance outside of some semiotic practices of a closed fraternity, then that is one thing. Alternatively, if it is asserted that it is a superior method of illuminating questions about the nature of order and coordination in economic life which date back to John Maynard Keynes, or Adam Smith, then that is quite another thing. It makes one wonder if formalists take their own methodological pronouncements seriously.

§11. Defense$_2$ is untenable, which probably explains why its partisans prosecute it with such modest vigor and enthusiasm. As frequently as not, when persistently pressed about the merits of the axiomatic method, those same proponents retreat to the following position: "Let us live and let live. Just give us our teaching posts and our graduate students and our journals, and let us cease this tiresome discussion of method. Let history judge." On the face of it this is an admirable sentiment, the request of every scholar to be left in peace. Nevertheless, in this context it is the subtle extension of the formalist program: it redirects attention away from the ends and purposes of research and back toward more vague impressions of scientific method and means. It denies the possibility of any rational discussion of the impact of formalist practices upon the remainder of the economics profession, either through the alteration of analytical content, or through transformations in the sociological structure of the profession. It ignores the fact that formalists are at present in the ascendancy in the profession. And, finally, it is not even good neoclassical economics: if resources are scarce, then presumably it is desirable to foster competition among various research programs.

It is in this sense that the formalist program serves to hinder rational communication in economics.

§12. Now let us turn to Defense$_1$, which, in contrast to Defense$_2$, will provide us with much more substantial material upon which to ponder. To reiterate, Defense$_1$ insists that the subject matter of economics is "naturally quantitative"; and it is this fact which dictates that mathematical expression is more convenient, more concise, and admirably suited to its subject matter. Defense$_1$ is frequently yoked to another thesis to the effect that mathematics is just another language, so that comparisons of convenience and conciseness are thought to be carried out among languages freely translatable and of the same epistemic efficacy, at least beyond the sphere of the naturally quantitative.

Various considerations broached in previous sections (especially §8 and §9) provide us with the initial means to evaluate this defense. The whole question of "convenience" seems to dovetail with the neoclassical appeal to "efficiency", as if this referred to some unique and unambiguous criteria, independent of context or of the vantage point. We have already observed that the importation of mathematical techniques brought with it subtle and telling metamorphoses in the subject matter, as well as having profound impact upon the sociology of the discipline. If the convenience of mathematics were to be portrayed as some sort of global optimum, then that optimization would necessarily involve the comparison of the status quo with worlds embracing different economic theories, different conceptions of science, different social structures of research, etc. Since such comparisons are not even remotely possible, assertions of convenience reduce to Panglossian notions that what is, is right.

It might be objected that this misconstrues the meaning that partisans of Defense$_1$ intend in their use of the term "concise". Perhaps they wish to bracket the whole question of the evaluation of theories by presuming we are already in possession of the "correct" theory, and only then confronted with the prospect of choice between a mathematical formulation and one in English, perhaps also assuming our audience is fluent in both modes of discourse. Given that the economy as we know it operates within a regime of numbers, would it not be more concise to choose mathematical expression? This sentiment has been criticized by Ken Dennis in two articles on problems of translation in mathematical economics.

Dennis (1982) argues that for purposes of exposition, there is no hard and fast dichotomy between mathematical language and the vernacular; mathematical models constitute a subsystem of notations which, by necessity, remain embedded within a framework of conventional language. Samuelson (1952, p. 59) saw the fact that mathematics is taught using the vernacular as support for his thesis that mathematics is just another language; but it could equally well be interpreted as showing that mathematics and the vernacular are not completely separable and self-sufficient communication systems. If this point is granted, then the predisposition to view logic and systematic exposition as an intrinsic property of mathematical symbols becomes the source of much confusion. Rigor and concision derive as much from the precision with which the analyst is capable of performing the translation between the subsystem of the mathematical model and the English commentary, as it does from the appropriate handling of the rules of mathematical manipulation. Errors in transit from a differential to a "marginal cost" will render an analysis void of sense as readily as errors in differentiation or integration. To illustrate the importance of this fact, Dennis provides numerous examples of what he calls the "double standard of high mathematical rigor and low semantic comedy." The formalist may feel this vindicates his or her disdain for the vernacular, but this overlooks the fact that there will never be an escape from the conundrum of translation in economic theory, and thus the notion of an absolute ranking of convenience of either mathematics or the vernacular is an empty one. The proportion of symbols from the respective systems will differ, along with style and semantical conventions, but these are merely the reflections of the personality of the author, and not the imperative of some spectral Platonic mandate. Since no axiom system can be fully and finally specified, there will always be room for originality. In fact, since economists are so rarely first-class mathematicians, most of the contributions economists can reasonably aspire to make to their chosen discipline must come in that twilight zone of semantical interpretations of previously developed mathematical structures.

§13. There remains the issue of what it means for a phenomenon to be "naturally quantitative".

To an economist, the broaching of this question may seem to be the worst form of hair-splitting. "Prices are numbers. Everyone can see that." It is true that prices are quoted as numbers, but that does not settle their relationship to mathematical techniques, nor does it begin to explain why prices are quoted as numbers. The remainder of this paper will consider these two questions in some detail.

Those who insist that the naturally quantitative character of prices, commodities, etc., suffices to justify the employment of mathematics in economics run up against the problem that they would not like to extend a blanket justification to any deployment of mathematical symbols in economic theory. Clearly, any old math will not do. Consider Schumpeter's comment:

> But the use of figures—Ricardo made ample use of numerical illustrations—or of formulae—such as we find in Marx—or even the restatement in algebraic form of some result of nonmathematical reasoning does not constitute mathematical economics: a distinctive element enters only when the reasoning itself produces the result that is explicitly mathematical. (Schumpter, 1954, pp. 954–955)

The distinction between legitimate and illegitimate mathematical economics is somewhat vague. Is Marx quarantined because he employed algebra instead of the calculus? Did Ricardo fail because he did not cast his discourse in the format of theorem-proof lemma? Why is it that Jevons and Walras disparaged the mathematical models of William Whewell as illegitimate? And why is it that Cournot is widely considered to be the first mathematical economist, even though there were many who preceded him (Theocharis, 1961)?

There is a tangle of issues here to be sorted through. Initially, one might suggest that the illegitimate use of mathematical symbols in economics might be defined as ineptitude or errors in the manipulation of mathematical symbols. Whewell got his sums of infinite sequences of fractions wrong; Marx botched the transformation problem; Ricardo was embarrassed by his 93-percent labor theory of value. The problem with this interpretation is that the "legitimate" mathematical economists are equally as guilty of these errors. Prudently restricting ourselves to the long deceased, we discover: Cournot (Cournot, 1897, pp. vi–vii); Walras (Walras, 1965, letter 1679, n. 3; letter 211, n. 4; letter 331; letter 1009); Pareto (Volterra in Chipman, et al., 1971, p. 368); (Wicksell, 1958, pp. 141–158); and Marshall (Marshall, 1975, pp. 4–5).[8] The curious predicament that besets those who maintain that mathematical exposition banishes error from discourse is the fact that historically, everyone makes errors of manipulation. Candid observers such as David Hume understood this long ago:

> There is no Algebraist nor Mathematician so expert in his science, as to place entire confidence in any truth immediately on his discovery of it, or regard it as anything more than a mere probability. Every time he runs over his proofs, his confidence encreases; but still more by the approbation of his friends; and is rais'd to its utmost perfection by the universal assent and applauses of the learned world. (Quoted in Kitcher, 1983, p. 41)

It cannot be simply errors of manipulation of mathematical rules which deem that a particular application of mathematical formalism to economic theory is inappropriate.

Perhaps, alternatively, the reason particular theorists are disqualified as mathematical economists is that they did not avail themselves of formalist proof techniques. But surely this criterion would be much too restrictive, since formalist proof techniques were only propagated in the late nineteenth and early twentieth century; and even today, many who would consider themselves mathematical economists only use the format intermittently, and often in a desultory manner. Further, formalist proof techniques are not coextensive with the entire project of mathematics, as we have indicated above in section §5 in our discussion of Gödel's theorem. Moreover, the structure of mathematical proof is neither independent of historical context, nor has it always conformed to a certain rigid format. This is due to the fact that every mathematical proof skips an indeterminate number of steps, as well as the fact that the standards of mathematical proof have changed drastically over time (Kitcher, 1983, pp. 42, 191; Kline, 1980). Thus, conformity to a particular proof format is not a valid passport to the pantheon of the mathematical economists.

Given that we have already claimed in section §12 that mathematical argumentation can never be entirely divorced from linguistic expression, the statement that there exists a particularly appropriate mathematical economics must mean that a specific subset of mathematical technique is ideally suited to prosecute discussions of the operation of the economy. It does not appear that any other coherent interpretation can be given to dicta represented by the above Schumpeter quote; and yet, I have not found a single economist willing to make this argument.[9] Instead, all partisans of Defense_1 treat all of mathematics as if it were a single unified body of knowledge, indiscriminately and equally fit for economic discussions. The conflict between credo and practice has not been reconciled within the bounds of Defense_1.

§14. It is possible, with the aid of section §6 above, to offer a very brief explanation of the observed fact that *neoclassical* economists do not lavish equal praise upon all competent examples of the employment of mathematical formalism in economic theory, however much their methodological statements suggest this should be the case. As previously observed, mathematics was integrated into economic theory simultaneously with the marginalist revolution, which appropriated a specific model from nineteenth-century physics and merely changed the names of the variables. An unintended consequence of this event was that a very narrow subset of mathematics came to be identified with neoclassical theory: that is, the mathematics developed specifically within the context of the physical theory of the late eighteenth and nineteenth centuries, the calculus of constrained extrema.

When a Schumpeter says that Ricardo and Marx were not really mathematical economists, the only consistent interpretation of this statement is that they did not employ techniques of constrained maximization. When Cournot is cited as the first legitimate mathematical economist, it is not because he theorized in terms of utility (he did not), nor because he provided proofs to accompany his mathematical symbols (ditto), but because he applied optimization techniques to fixed revenue and demand functions. When Whewell is denied the status of a mathematical economist, it is because he simply found the solution to a set of algebraic equations. When an Edgeworth or a Pareto is remembered while a Bortkiewicz or a Palomba or a Mandelbrot is forgotten, it is preponderantly due to their respective attitudes toward and incorporation of constrained extrema in their work.

One of the greatest misperceptions in the history of the discipline of economics is that which credits neoclassical theory with the wideranging appreciation and appropriation of mathematical tools. On the contrary, the neoclassical "box of tools" is very small: a purse, or a pouch. Neoclassicism has become little more than constrained optimization in ever-more baroque guises.

§15. Disregard of the twentieth-century doctrine that there is no single unified body of techniques called "mathematics" (Kline, 1980, pp. 275–277) and the conventional belief that the economy is naturally quantitative are really two aspects of the same idea. One of the most profound intensifications of the abstract character of mathematical speculation occurred in the late nineteenth century, when geometry was divorced from the study of physical space; shortly thereafter, algebra came to be distinguished from the study of number (Wussing, 1984; O'Malley, 1971). Hilbert once said that although he spoke in terms of points, lines, and planes, the terms he employed could just as well have been mug, chair, and spoon. The rise of abstract algebra suggested that most existing mathematical theorems were merely different realizations of more general principles governing the relationships between abstract objects possessing a very few basic properties. Poincaré flippantly summarized this trend by defining mathematics as the art of giving the same name to different things. For Poincaré and others, there was a kernel of Platonism buried in this epigram, since mathematical formalism does tend to encourage the imposition of the aura of persistence and essence upon unruly and disparate phenomena (Meyerson, 1962). This imperative to uncover the one in the many would only make sense if, at some fundamental level, everything really partook of some abstract unity (Giedymin, 1982, p. 31).

It was the further elaboration of the implications of abstraction by Kurt Gödel, through the assignment of statements to their "Gödel numbers", where any string could simultaneously be interpreted as a metamathematical statement as well as some assertion in arithmetic, that ultimately undermined the confidence in this approach. This escalation of abstraction finally led to the realization that there is no single unique meta-structure embedded either in the elaboration of all mathematics, or in the symbolic expression of events. The history of abstraction surprizes us with proliferation as well as with unification (see Lakatos, 1976).

Understanding the historical timing of this realization is a prerequisite for the explanation of economists' impressions of the significance of mathematics. The physics model appropriated by the progenitors of neoclassicism was generated around the middle of the nineteenth century, just before the spread of the furore over the significance of the non-Euclidean geometries. The first generation of neoclassicals were contemporaries of Klein's Erlanger Program (Kline, 1972, p. 917), which became the group theoretic manifesto, but the economists remained unacquainted with it. The next major wave of neoclassical economists in the 1930s–1950s was also unaware of foundational issues in mathematics and their attendant controversies. These economists came either from physical science backgrounds (Harcourt, 1984, p. 500), which eschewed these ideas in the interests of pragmatism; or else they were heirs of the Bourbaki tradition, the major school of formalists who have shrugged off foundational challenges, and pursued the dream of a unified mathematics. (The backgrounds of economists in the post-Vietnam era are even more narrow.) Thus, perhaps it is not at all unusual that neoclassical economists are predisposed to believe that there is a unified corpus of mathematical technique, which then must be isomorphic to the patently obvious quantitative character of prices, outputs, money, and so forth.

§16. On a few rare occasions, prominent mainstream mathematical economists have seen fit to elaborate upon the idea that the economy is naturally quantitative, although inevitably these episodes take the form of remarks in passing or asides. After an intensive search, the few instances I could find in the entire postwar period are best represented by:

> The logical justification of the use of diagrams (in economic theory) lies in the *fact* [my italics–P.M.] that the postulates underlying the analytical description of space are identical with those used to represent the joining and separating of commodity bundles and the multiplication of such bundles by numbers. (Koopmans, 1957, p. 174)

> Having chosen a unit of measurement for each one of them (the commodities), and a sign convention to distinguish inputs from outputs, one can describe the action of an economic agent by a vector in the commodity space R^l. The *fact* [my italics–P.M.] that the commodity space has the structure of a real vector space is a basic reason for the success of the mathematicization of economic theory. (Debreu, 1984, pp. 267–268)

The agreement over the interval of nearly thirty years is impressive. The testimony that the economy is naturally quantitative does not consist of the observation that prices are expressed as numbers. More fundamentally, the proffered explanation of the efficacy of mathematical economics is that commodities naturally come in real or Euclidean sets. Curiously enough, this is not expressly included in the list of axioms, but couched in the language of *fact*, which presumably is intended to indicate that this is self-evident. To rephrase this in the somewhat arcane terminology of classical economics, exchange values are quantitative because they are merely a reflection of the "fact" that use values are physically quantitative.

This argument is yet another corollary of the neoclassical predilection to appeal to physics-style arguments. The apparently extrasocial aspect of the sequence one apple, two apples, three apples,... is to provide the natural starting point for price and value. Unfortunately, it is precisely this dichotomy between the "natural" sphere of use and the "social" sphere of value that we wish to isolate as the untenable bulwark of Defense_1. If we are forced to judge by the criteria of use, then it is not at all clear that apples and oranges span a Euclidean vector space. Before we appeal to mathematical and philosophical arguments, it may prove instructive to note that various neoclassical economists have already voiced this caveat, albeit in contexts other than an evaluation of mathematical methods in economics.

The first manifestation that something was amiss can be associated with the work of Lancaster (1966) on his revision of the theory of neoclassical consumer demand. Lancaster sensibly suggested that commodities are not generally desired because of their phenomenological identity, but rather for some bundle of characteristics they presumably embody. In effect, Lancaster proposed an intermediate mathematical device which would translate "apples" into appropriate indices of sweetness, crunchiness, redness, "fostering the image of promoting our own health"-ness, and so on. Then, after a *particular* apple is encoded into the terms of the variables with which we express our desire and longing, these variables are entered into the new model utility function.

In the intervening years, sporadic reference has been made to Lancaster's work in bibliographies of consumer theory (Green, 1971), but it has not attracted further research; possibly because it had touched an exposed nerve. On a superficial level, it would seem that sweetness, crunchiness, etc., are rather more difficult to quantify than the apples themselves; therefore, to interpose these less-mathematically accessible variables between unobservable utility and the apples as discrete units seemed to weaken rather than strengthen the existing theory. Nevertheless, this attempted revision was significant, because it gave voice to a hesitation that had occurred to many who had given serious consideration to the utility function: from a strict utilitarian point of view, there is no such thing as a generic commodity. To every individual *qua* individual, each apple is different: some bigger, some stunted, some mottled, some worm-ridden, some coated with stuff that will kill me slowly, some McIntosh, some engineered to look and taste like tomatoes.... Although the thrust of the insight remained latent in Lancaster's article, reconsideration of these issues raised the possibility that the self-identity of the commodity, which is the necessary prerequisite of its basis as a cardinal number, is not at all psychologically present. The Lancaster model remained in the background as an irritant precisely because the natural ground of cardinality, the very quiddity of the definition of the commodity, melted into air; and all that survived was the faintest suggestion that the rigid standardization requisite for cardinality was *imposed* by the development of the market with its arbitrary bundling of characteristics.[10] Perhaps number is not a natural attribute.

Another version of this reticence appeared in Oskar Morgenstern's popular book on the accuracy of economic observations. As is rather frequently the case among neoclassical economists, Morgenstern took the opportunity to excoriate the accounting profession for producing what he considered to be meaningless numbers. He complained:

> Both balance sheets and ... profit and loss accounts represent a mixture of figures that belong in widely separate categories. Yet these figures are treated conceptually and arithmetically as if they were completely homogeneous.... There simply cannot be a financial statement which is not ultimately the report of some physical event: money passing from one hand to another ... or a record made of some physical entities allegedly in the possession of the business. The record, however, may contain an additional element, namely that of *evaluation* of the physical activity. (Morgenstern, 1963a, p. 72)

As the reader may realize, this is the same problem in a different setting. Business accounts impose a type of homogeneity upon their assets and liabilities, and thus a certain algebra (see Ellerman, this volume, ch. 3), which is hardly obvious, and in certain circles, is quite an object of contention. Oddly enough, Morgenstern seemed to feel that this was the fault of the businessmen, who deviously and wrong-headedly resisted dividing the world up into "figures [which] can be viewed as direct statements about fairly easily ascertained *physical* things such as cash, currency and bank deposits" (Morgenstern, 1963a, p. 75) and valuations dependent upon some theory. Here once again is the physicalist bias, but in a distorted mirror-image: now it is *money* that is the physical touchstone, and it is physical commodities that require some dubious theory of imputation in order for them to be subject to the same format of algebraic accounts. And once again, the irony is close at hand: is it not incongruous to refer to *money* as if it provided the physical foundation for the quantification of business records? The "natural" basis of quantification slips further from our grasp.

The level of subtlety of discussions surrounding this issue was raised incalculably by the appearance of Nicholas Georgescu-Roegen's *Entropy Law and the Economic Process*. In place of the excessive deference conventionally displayed when an economist invokes the name of physics, Georgescu-Roegen's familiarity with the subject prompted him to start from the premise that, "Physics, therefore, is not as free from metaphysics as current critical philosophy proclaims" (Georgescu-Roegen, 1971, p. 97). In practice, he agrees with the quote from Norman Campbell at the beginning of this chapter. He insists that the use of cardinal measure reflect a particular physical property of a category of objects. To quote his argument in detail:

> ... this simple pattern (of proportional laws in physics) is not a mere accident: on the contrary, in all these cases the proportional variation of the variables is an inevitable consequence of the fact that every one of these variables is free from any qualitative variation. In other words, they are all cardinal variables. The reason is simple: if two such variables are connected by a law, the connection being immediate in the sense that the law is not a relation obtained by telescoping a chain of other laws, then what is true for one pair of values must be true for all succeeding pairs. Otherwise, there would be some difference between the first and, say, the hundreth pair, which could only mean a qualitative difference. This characteristic property of cardinal laws ... constitutes the very basis on which Cantor established his famous distinction between ordinal and cardinal number. We arrive, Cantor says, at the notion of cardinal number by abstracting from the varying quality of the elements involved and from the order in which we have "counted" them. (Georgescu-Roegen, 1971, p. 102)

The elaboration of this conception of law-like structure can be seen, in retrospect, as the prime motivation behind most of Georgescu-Roegen's impressive *ouvre*. In the 1950s he argued that if commodities were cardinally measurable, then there would always be an uncaptured qualitative residual associated with any individual's esteem for them, and that this fact in itself, even in the absence of other psychological assumptions, would guarantee that indifference curves would always be convex. He later realized that it could only guarantee that indifference maps would be nonlinear; a much less interesting proposition (Georgescu-Roegen, 1971, p. 113). Nevertheless, this insight can serve to explain the failure of Lancaster's research program: the qualitative residual cannot be banished by appending any set of quantitative variables to existing neoclassical theory (Georgescu-Roegen, 1971, p. 76). To put it in a somewhat different manner: If utility really were measurable, then all units of one generic commodity could be made psychologically identical with any other commodity; all commodities could be reduced to other commodities; and we would be back to a classical theory of value which discovered value as embodied within the commodity. The question of the "natural" or "unnatural" quantification of economic phenomena is, properly interpreted, a metaphysical problem of identity. Hence Poincaré's remark that mathematicians give the same name to different things.

Although Georgescu-Roegen neglected to press the inquiry into the cardinal measurability of commodities in consumer theory, he did choose to do so in the theory of production (Georgescu-Roegen, 1976, pp. 72–73). His contributions in this area are decisive. First, he has observed that physics is not uniformly "quantified". There are many areas of study which have not been able to construct or discover proportional laws, presumably because they are more directly concerned with variations in qualities. Secondly, he points out that the technical role of an input in a production process *may* be specified in a physically quantitative relationship, but that quantification rarely has any direct relationship to the "cardinality" of the input in its incarnation as a commodity (Georgescu-Roegen, 1971, p. 218). In a simplistic example, oil is sold by the barrel, but its efficacy in one production process is measured by foot-pounds per BTU, and in another by sulfur content in milligrams per litre, and in a third process a measure of resistance relative to roughness (in terms of the diameter of sand particles that give the same effect at a high Reynolds number). One might retort that the fully appropriate measure of the commodity should be some such vector as (liquid volume, BTU rating, Reynolds number, ...); but this ignores the fact that if we extend the metric to encompass every possible aspect of every conceivable production process, we absurdly balloon the length of the list of generic "commodities" until cardinality is defined away, because there is no remaining identity of "oil". Third, he explains that algebraic operations upon the input units cannot be confused with algebraic operations intended to represent production processes. Production processes may be cojoined, or they be assigned membership to a set in the mind or on paper; however, they cannot strictly be added or multiplied (Georgescu-Roegen, 1971, chap. 9).

Georgescu-Roegen brings to bear all of these considerations to demonstrate that the neoclassical production function is a thoroughly slipshod construct which is incapable of any appeal to physicalist notions as justification of its mathematical structure. In fact, since a production process does not satisfy the first requirement of lawlike behavior—that is, inputs and outputs are not directly connected, in the sense outlined above in the lengthy quote from Georgescu-Roegen—it does not even qualify as an appropriately cardinal formalism. The devastating moral of this line of inquiry is that, "If we maintain that any scale is as good as any other, then such fundamental notions as decreasing marginal rate of substitution, constant returns, efficiency, etc., lose any meaning whatsoever" (Georgescu-Roegen, 1976, p. 274).

It would thus seem that by the 1970s most of the components of a powerful critique of the received doctrine that the economy is "naturally quantitative" could be harvested from the neoclassical theory literature; nonetheless, this critique never materialized. Although he hesitated to do so himself, Georgescu-Roegen's critique of production theory could easily have been extended to the theory of the neoclassical consumer. After all, consumption is also a process, and is treated in other respects by neoclassicals in a manner symmetrically to production.

It could have been pointed out that the common thrust of these varied writings is the overarching thesis that *there is no reason to believe that the algebras of economic quantities are isomorphic to the algebras used to characterize their physical manifestations.* An alternative interpretation would see metrics as constructed entities conditional upon the intended use, based upon the imposition of identity upon phenomenological diversity. Alas, this line of inquiry has lain dormant. We now return unerringly to that non-quantitative question, the motor of metaphysics: Why?

§17. There are at least two distinct answers to that question. The first derives from a certain tradition in anthropology and sociology, which claims that all cultures, preliterate and literate, are predisposed to base their explanations of their own social interactions upon their theories of the natural world and natural order (Barnes & Shapin, 1979; Douglas, 1966, 1970). As much as we might wish to feel superior to the Tiv or the Nuer or the Bushmen, the continuous invocation of and appropriation of physics by neoclassical economists documented in this essay reveals that we really all are brothers under the skin. One reason why the critique of "natural order" in the quantitative sphere has not been followed to its conclusions is that, as we have observed, this inquiry would reveal the social and conventional bases of quantification, and it would therefore undermine the direct lineage of economic magnitudes' descent from physical magnitude. Many unexplored programs of research remain that way because the abyss seems to yawn just inside of their perimeters.

The second answer to the question may be more palatable to those who find such functionalist explanations distasteful. Another major reason that the critique of a direct isomorphism between physical and economic algebras has languished in an undeveloped state is that the most perceptive and insightful critics have not marshalled one of the major mathematical devices of the twentieth century to their cause. That body of technique is a subset of the discipline of abstract algebra called group theory.

Group theory evolved out of work done on the theory of equations in the early nineteenth century (Wussing, 1984). It began as the documentation of certain patterns in the solutions of equations when various key parameters underwent permutation. After 1870 a more abstract view of groups gained ascendancy. Around the turn of the century it was recognized that the structure of groups could be employed to describe any arbitrary operation, not necessarily those restricted to the theory of equations or geometry, which conformed to a few simple rules. Groups provided the language for a discussion of very abstract patterns which, when interpreted, promised to uncover connections between many disparate areas of mathematics.

An abstract group is defined as:

I. A set of elements (a, b, c, d, ...) which can be of finite or infinite order. The number of elements in the set is called the order of the group.
II. Any operation between any two elements, which we shall read from left to right. For example: $a \times b$
This operation must obey the following rules:
i) *Closure*. If a and b are elements of the set, so is the result of $a \times b$.
ii) *Associativity*. $a \times (b \times c) = (a \times b) \times c$
iii) *Identity Element*. The set must contain an element e such that:

$$e \times a = a \times e = a, \text{ for each element in the set.}$$

iv) *Inverse Element*. For every element a in the set there exists an element b such that:

$$a \times b = b \times a = e$$

We will follow standard notation and denote this inverse $b = a^{-1}$.

The central concept in abstract algebra is the group; the taxonomies of other abstract algebras generally involve the augmentation or diminution of the above set of rules. Some of these variants that we shall shortly find useful are the concepts of an Abelian group and a semigroup. In the former case, if we were to append a fifth rule to the above four to the effect that the operation must be *commutative*, that is, for every pair of elements:

$$a \times c = c \times a$$

then the group would be called an Abelian group. In the latter case, a set of elements and an operation which only conforms to the first two rules of closure and associativity is called a semigroup. As is to be expected, the less restrictive specification of a semigroup results in much diminished inference concerning its properties. Finally, any subset of the elements of a given group which, by themselves, conform to the rules i–iv is known as a *subgroup*.

One advantage of group theory is that knowledge of a small number of key characteristics of a group will serve to summarize all of the important information about the structure of an algebra. Poincaré observed that the theory of groups is "... the whole of mathematics divested of its matter and reduced to pure form" (quoted in Kline, 1972, p. 1146). Some of this powerful capacity can be illustrated by the examination of the "table" of a group of small order; in this case, a group of order four. The group table displays all of the possible outcomes of application of the $\times$ operation between any two elements of the set.

GROUP TABLE

e	a	b	c
a	b	c	e
b	c	e	a
c	e	a	b

In this example, the group consists of the set of elements (a, b, c, e), and obeys the following rules:

$$a \times a = b;\ e \times a = a;\ e \times b = b;$$
$$a \times b = c = a \times a \times a;$$
$$b \times b = e = a \times a \times a \times a.$$

Inspection of the table is sufficient to reveal that this is indeed a group, since rules i–iv imply that no element of the set can appear more than once in any column or row of the table. The table is symmetric, in that the pattern of entries is identical above and below the diagonal running from the upper left to the lower right hand corner: this is indicative of the fact that the operation is commutative, and thus this group is Abelian. Knowledge of the fact that the group is of order four imposes sufficient restrictions upon the operation such that we know that there exist only two distinct structures for groups of order four, and that they both must be Abelian. Similarly, we know there is only one group structure of order three, and only one structure of order two, and that they also must be Abelian (Durbin, 1985, p. 103). Other theorems of group theory which we shall employ in this paper are: each group can only possess one unique identity element; each element of a group possesses a unique inverse; groups of prime order possess no subgroups except themselves and the isolated identity element (ie., the improper subsets). The reader might confirm these theorems from inspection of the group table.

Groups are more abstract than the more familiar ordinary algebra because they subsume its patterns under more general principles. For instance, suppose we restrict ourselves to the set of integers:

$$(\ldots, -3, -2, -1, 0, 1, 2, 3, \ldots)$$

and consider the group operation of ordinary addition. Inspection will reveal that this operation conforms to all of the rules governing a group: the sum of any two integers is an integer, the element "0" is the identity, the inverse of "n" is "−n", and closure is preserved by specifying that the group is of infinite order. Since addition is commutative, the group is Abelian.

Now instead suppose that we restricted ourselves to the set:

$$(\ldots, 2^{-3}, 2^{-2}, 2^{-1}, 2^{0}, 2^{1}, 2^{2}, 2^{3}, \ldots)$$

If we then specify the group operation to be ordinary multiplication, we find that we again have a group. An important step in coming to understand the abstract power of group theory is to observe that all of the formal patterns produced within this multiplicative group are exactly the same as the patterns displayed by the additive group of integers, so that for all practical purposes, they are the same group. (Here recall Poincaré's quip.) If two groups have the same pattern of entries in their group tables, then they are said to be *isomorphic* to one another.

However brief and inadequate this survey of group theory, it should sufficiently equip us with the means to explain how the critics of quantification and arithmomorphism in economic theory were hampered by their neglect of abstract algebra.

§18. For purposes of illustration, we shall initially focus our attention upon two neoclassical economists who have been concerned with problems of formalization and quantification: Robert Clower and Nicholas Georgescu-Roegen. Clower perceives his work as the tilling of the narrower field of monetary theory, whereas Georgescu-Roegen cultivates the broader field of production theory. Both would have profited immensely from detection of the isomorphisms between their respective programs of research.

The aim of (Clower, 1967) was an inquiry into the determinants of the trivial role played by money in syntheses of Keynesian and neoclassical theories. He decided that the major culprit was the mathematics of then-popular models, which effectively described barter economies in which every commodity indiscriminately performed the functions of money; hence, an independent money commodity was redundant. Of particular interest from our present point of view was his method of demonstrating his point. He presented the following tables as paradigms of different kinds of economies:

barter

	C_1	C_2
C_1	X	X
C_2	X	X

pure money

	C_1	C_2	C_3
C_1	X	X	X
C_2	X	X	0
C_3	X	0	X

non-pure money

	C_1	C_2	C_3	C_4
C_1	X	X	X	X
C_2	X	X	0	0
C_3	X	0	X	X
C_4	X	0	X	X

The C_i are indices for different generic commodities, $i = 1,2, \ldots, n$. The X's are to be interpreted as indicating that the trade of commodities represented by the intersecting row and column is allowed to take place; the 0's indicate that a particular trade is not permitted. Although Clower's overriding concern is with money, the artifice of the tabular format leads him to briefly consider the more fundamental question: what qualifies as a legitimate trade? He takes it as a self-evident axiom that possession of a commodity qualifies as a virtual trade of that commodity for itself; that is, C_i can always be traded for C_i (and therefore, diagonal entries in the tables will always be X). Secondly, he posits that the exchange relation must always be symmetric; that is, if C_i is allowed to exchange for C_j, then it must also be the case that C_j is also allowed to exchange for C_i (thus Clower's tables will always be symmetric around the diagonal). Finally, he defines a money commodity as one which can be traded for any other commodity. From these axioms he deduces the theorem that the simplest economy where money performs a non-trivial function of the coordination of exchange must have at least three distinct commodities. Similarly, the smallest money economy which is capable of containing a subset which functions as a pure barter system must comprise at least four separate commodities. Since many of the models under consideration did not meet these criteria, Clower felt satisfied that he had identified the flaw in their arguments.

The reader will quickly recognize that Clower was groping his way towards an abstract algebric representation of trade.[11] The most critical artifact which prevented him from exploiting the group concept was his insistence upon binary trade/no trade entries in the bodies of his tables. If we temporarily overlook the fact that his table entries are not group elements because the operation is not closed, we can observe that much of what Clower wished to say (and much more) could be expressed using the theorems of group theory.

The imposition of diagonal symmetry upon the tables is a very strong restriction; we know that if we interpreted the operation of exchange as conforming to an algebraic group, then Clower must be insisting that all trades are commutative, and thus the group of exchange must be Abelian. (Possible definitions of the operation and the set of elements are discussed below.) In his own examples, however, this is not a matter subject to choice, since *all* groups of order five or lower must be Abelian. In other words, in order to consider exchange as a group process which is not commutative, we must build models of the economy which possess at least *six* distinct commodities. Moreover, by Lagrange's theorem (Hamermesh, 1962, p. 20), we know that the order of all proper subgroups of any arbitrary group are integer factors of the order of the original group. Therefore, any model of an economy where the number of distinct commodities is prime will possess no barter subeconomies similar to the one exhibited in Clower's table (c). Thus we witness an advantage of the group theoretic perspective: it helps us discern what the previous employment of other mathematical techniques has served to obscure. In the present instance, it indicates that any economic model that treats less than four functionally distinct commodities cannot seriously discuss the separate and distinct functions of money in the economy; and a model which would not surreptitiously impose the condition that all trades are commutative must possess at least six distinct commodities.

While these theorems instantiate how group theory might have helped to generalize Clower's results, they are simply extrapolations of his basic themes. A more important application of group theory could reveal that even at this most abstract level of the binary entries of trade/no trade, Clower could not succeed in illuminating the very core of the problem of money. The question that he posed was: under what conditions would the functions of money be non-trivial? Clower correctly noted that money would always be trivial in two-good models. What he did not notice was that as long as trade conforms to a group, and each good trades for at least one other good, and the group is Abelian, any particular commodity can be obtained through a finite sequence of trades, starting from any arbitrary endowment. Unless further structure is imposed upon the model (such as independent transactions costs or other external constraints on the trading sequence), money still has a trivial function in such an economic model. Clower could not observe that the axiomatic imposition of symmetry on his economy acted to neutralize the very role of money which he wished to highlight, because it was isomorphic to a world of barter where any commodity may be directly or indirectly traded for any other commodity. Once one becomes sensitized to the group formalism, one can immediately deduce these results from inspection of the abstract patterns displayed in a group table.

The seemingly harmless assumption that the activity of trade is commutative is freighted with profound and substantial theoretical content, much of which has never been explored in any detail in economics. The elaboration of this content will take up much of the remainder of this essay. In this section it may suffice to simply indicate some of the aspects of the submerged theoretical content. First, commutivity places some implicit restrictions upon the actors, who have up until now remained hidden in the wings. Commutivity means there must exist some set of traders willing to exchange X for Y, and another set willing to trade Y for X in the exact same circumstances (which includes the law of one price). Hence, people must differ in whatever it is that motivates their trading activities, and those activities *must have been coordinated prior to the realization of the trades*. In other words, there must exist diversity, but simultaneously, that diversity is neutralized through the restriction that it can in no way materially affect the outcome. Clower did not see that commutivity neutralized the role of an independent trade coordinator. Without the specification of different trader personalities, commutivity would condone a shoe salesman entering his own shoe shop to purchase shoes, and in fact, this would provide the major vehicle for taking up any slack in effective demand. Secondly, commutivity implies effective reversibility of any trade in time, rendering any attempts to model errors or historical change incoherent. Third, commutivity posits a symmetry which is frequently absent in actual economies. I can take money to the Safeway to buy food; but I can't take food into the Safeway to buy money. Fourth, commutivity imposes some very rigid conditions upon the concept of value, which we shall elaborate below in section §20.

Now let us relinquish the binary entries in Clower's tables, and in the process discover that monetary theory is merely a special case of a more general economic problem. We have noted that the structure of a group requires closure; this would mean that the entries in the tables whose purpose it is to describe trade must themselves be members of the set of commodities. Thus (as should be obvious) Clower's tables are useless in discussing prices, since the elements of his set are merely the names of generic commodities, such as: (Gucci shoes, hot dogs, beer mugs, iron ingots, . . .). In other words, *Clower's conception of trade is not quantitative.* Let us inquire into how we might rectify this serious omission.

Clower treats trades as if they were thoroughly abstracted away from the activities of the people "behind the scenes"; in this section, we shall do likewise. Suppose that there happened to be six discrete "endowments" sitting in a "market". There are also three permissible barter trades, sanctioned by some unspecified mechanism: one particular hat trades for a particular dozen eggs, a second dozen eggs trades for a pen, and three hats trade for ten dozen eggs. Employing the symbol Ⓣ in order to signify the operation of trade, our rules are therefore:

$$
\begin{aligned}
1\text{ hat} \text{ Ⓣ } 12\text{ eggs} &= 12\text{ eggs}\\
12\text{ eggs} \text{ Ⓣ } 1\text{ pen} &= 1\text{ pen}\\
3\text{ hats} \text{ Ⓣ } 120\text{ eggs} &= 120\text{ eggs}
\end{aligned}
$$

If we adopt Clower's axioms that a commodity always trades "virtually" for itself, and that all sanctioned trades are symmetric, then we deduce the further sanctioned trades:

$$
\begin{aligned}
1\text{ hat} \text{ Ⓣ } 1\text{ hat} &= 1\text{ hat}\\
2\text{ hats} \text{ Ⓣ } 2\text{ hats} &= 2\text{ hats}
\end{aligned}
$$

$$\begin{array}{rcl} \vdots & \vdots & \vdots \\ \vdots & \vdots & \vdots \\ \text{12 eggs} \ Ⓣ & \text{1 hat} = & \text{1 hat} \\ \text{1 pen} \ Ⓣ & \text{12 eggs} = & \text{12 eggs} \\ \text{120 eggs} \ Ⓣ & \text{3 hats} = & \text{120 eggs} \end{array}$$

Further, let us provisionally adopt one of the most fundamental assumptions of all of mathematical economics (which has never been discussed, much less evaluated). Let us suppose that the operation of trade is associative, so for instance:

(1 hat Ⓣ 12 eggs) Ⓣ 1 pen = 1 hat Ⓣ (12 eggs Ⓣ 1 pen) = 1 pen

Consolidating all of the permissible trades into a single table, we arrive at:

Ⓣ	*1 hat*	*2 hats*	*3 hats*	*12 eggs*	*120 eggs*	*1 pen*
1 hat	1 hat	?	?	12 eggs	?	1 pen
2 hats	?	2 hats	?	?	?	?
3 hats	?	?	3 hats	?	120 eggs	?
12 eggs	1 hat	?	?	12 eggs	?	1 pen
120 eggs	?	?	3 hats	?	120 eggs	?
1 pen	1 hat	?	?	12 eggs	?	1 pen

Perusal of this table begins to reveal problems in the specification of Clower's tables, as well as problems in the specification of a group to characterize trade. The nature and significance of an "impermissible" trade is left tantalizingly vague in Clower's writings, and it is precisely upon the choice of conceptualization of these prohibited activities that much of the structure of the algebra founders. The question marks in the table signify trades other than those sanctioned by the unspecified "mechanism". If all of these entries were left empty, then we would be violating the first requirement of any abstract group, that any operation defined over a set should be closed. However, if a proscribed trade is not consummated, but instead remains virtual, how should the result be characterized? Taking a cue from Clower's contention that a commodity should always virtually trade for itself, we could posit that every blocked or prohibited trade is equivalent to a virtual trade of the initial commodity for itself, because the initiator of the blocked trade always retains the commodity offered. Thus, as an example:

1 hat Ⓣ 2 hats = 1 hat

In this eventuality, the above table would find all of the question marks replaced with the entry heading the corresponding row. Unfortunately, this emendation would contradict Clower's original axiom of the symmetry of trades, undermining their purported commutative character.

Further attempts to rescue this representation of the algebra of exchange are rendered hopeless by the realization that Clower's axioms are self-contradictory. This is because *this conception of barter exchange in a finite order economy does not conform to the structure of an algebraic group*. Even were we to induce closure in the table by said replacement of the question marks by the row headings, any given row or column contains elements of the set which appear more than once. This violates group rules iii and iv in the sense that the row and column headings do not behave like distinct elements of a group. Taken in isolation, each commodity bundle acts as its own identity and inverse; but this does not extend to the system as a whole, the aggregate of commodity bundles.

Parenthetically, there exists the possibility that our criticism of Clower misses the mark because we have misspecified the group elements as commodity bundles. An alternative would be to specify each group element as consisting of an entire trade, say:

A:	1 hat	$\rightarrow$	12 eggs	
B:	12 eggs	$\rightarrow$	1 pen	
C:	3 hats	$\rightarrow$	120 eggs	
D:	1 hat	$\rightarrow$	1 pen	and so on.

The group operation would in this case be the composition of these transformations: in this example, $A \times B = D$. The economic interpretation of the group operation would be that it identified compositions of trades that would end up "at the same place," in the way that both $A \times B$ and D end up at "one pen".

While there has been some very interesting work based upon this algebraic portrayal of exchange (Ellerman, 1984), it does not come to grips with the problems that concern Clower, (and us, we hasten to add), because it assumes them away at a very primitive level of analysis. First, this version is incapable of confronting the problem of impermissible or blocked trades, because, by definition, only "sanctioned" trades qualify as group elements. Secondly, it cannot explicitly confront the thorny issues of quantification, since it buries the notion of commodity equivalence in the primitive definition of the group operation: "one pen" counts as the "same result" in B and D. The seeming plausibility of this conceptualization ultimately rests upon the purported isomorphism of physical algebras to the algebra of trade discussed above in §16. Third, by focusing attention on the transformation rather than the commodity bundle, it assumes that trades are comparable along some axis in the absence of money, and therefore cannot distinguish situations in which the presence of money is either superfluous or necessary. Fourth, our critique of the incoherence of virtual self-trade also applies to this framework. For all of these reasons, the conceptualization of trade as a composition of transformations will not help us explain why prices are quantitative.

In this extended reconsideration of Clower's research agenda, we are forcibly struck by the persistent frustration of broaching the issue of the role of money in the context of an operation that lacks an identity element. Given the surfeit of trades that map any given commodity back onto itself, money is truly a superfluous concept. Hence Clower's critique dies aborning, because the problem is not restricted to the fundamental misrepresentation of money in mathematical economics; it extends to the fundamental misrepresentation of the operation of exchange.

Turning to the paper by Georgescu-Roegen (1976, pp. 271–296) on measure, quality, and optimum scale, we seem (at first blush) to be very far removed from any of the questions that motivated Clower's writings. Georgescu-Roegen avows his purpose is to demonstrate "that the ordinary concept of efficiency (as well as other equally important concepts of production theory) has no meaning if factors and products are not cardinally measurable." Nonetheless, there are two major similarities. The first, which we have already had occasion to mention, is the thesis that the laws of the prosecution of production processes are not necessarily isomorphic to the manipulation of their physical constituents. Clower suggests that the process of exchange is not adequately represented by the addition of physical units; Georgescu-Roegen holds the parallel brief for economic production processes. The second similarity resides in the fact that Georgescu-Roegen conceptualizes the analytical prerequisites for a plausible model of production by postulating an abstract operation, and then asking what axioms would guarantee that this operation was susceptible to cardinal measurement. It is fascinating that, just as in the case of Clower, he invokes some aspects of the basic structure of group theory without acknowledging it, and therefore misses using the analytical shortcuts provided by group structures. In fact, his axioms of cardinality (Georgescu-Roegen, 1976, pp. 275–279) are nothing other than our group axioms i–iv, plus commutativity and the axiom of Archimedes. As he observes without the aid of group theory, the imposition of an Abelian group structure upon an economic production process is tantamount to positing a world where all transformations consist of the reshuffling of some primal substance; such reshufflings can result in no new emergent properties other than those already inherent in the primal substance (p. 288). In somewhat simpler terms, qualitative novelty is precluded by the symmetry of the Abelian group. This is further corroborated by Georgescu-Roegen's description of what he calls "weak cardinality" (pp. 281–282), which is nothing other than the axioms posited by physicists in order to characterize "gauge symmetry" (C. L. Smith in Mulvey, 1981; and t'Hooft, 1980).

Why has abstract algebra been neglected in economics? Again we must return to the influence of the development of physical science upon conceptions of mathematical formalism in economic theory.

§19. Relatively recently, developments in particle physics have prompted some physicists to reconceptualize the progressive thrust in the history of their discipline as the unfolding of manifestations of symmetries in nature (Galison, 1983, p. 49; Elliott and Dawber, 1979). This revised standard chronicle begins with the recasting of the laws of motion in terms of energetic considerations—precisely those touched upon above in section §6. The goal of a unified theory of nature was given further impetus in the early twentieth century by the development of a theorem by Emmy Noether, which included an early application of the theory of continuous groups (Brewer and Smith, 1981, pp. 16 et seq.). Noether's theorem demonstrates that corresponding to every invariance or symmetry property of a variational theory there exists a conservation law. For example, the statement that the results of most physical experiments do not depend upon their orientation (i.e., the direction in space in which they are pointed) is more formally expressed as the axiom of rotational invariance; and this, in turn, is equivalent to the law of the conservation of angular momentum. Likewise, statements about the invariance of a phenomenon with respect to its temporal location are equivalent to the postulation of the law of the conservation of energy, as well as to the axiom in much of physics that laws of motion are symmetric with respect to the time axis. In this manner, many seemingly separate hypotheses concerning physical phenomena were subsumed under one general pattern.

The power of this approach only became apparent in the twentieth century, after the twin revolutions of relativity theory and quantum mechanics, only to become paramount upon the rise to dominance of subatomic physics (Rosen, 1983). The theory of relativity grew out of an imposed symmetry to the effect that the known laws of motion should be symmetric and invariant relative to any moving observer; and this deceptively simple condition provoked a profound revision in the very algebra of space and time, from the Galilean group to the Lorentz group. Quantum mechanics escalated the dependence of physics upon symmetry principles to a greater degree: "The quantum numbers tell us what kind of symmetries we mean.... Thus, when we come to the smallest objects in the world, we characterize them in quantum mechanics just by their symmetry, or as a representation of symmetries, and not by specifying properties such as shape or size" (Heisenberg in Buckley and Peat, 1979, p. 14). The implementation of this precept is evident in the quark model, where it serves to impose some structure upon a confusing proliferation of types of subatomic particles (Elliott and Dawber, 1979). Group theory was there applied to reduce all known particles (and a few yet to be discovered) to combinations of a small number of abstract qualities. Even more recently, theories of gauge symmetry are the main contenders in the quest to provide a grand unified theory of the four fundamental forces of nature (t'Hooft, 1980).

The lesson of interest for economists resides not in the mere fact that group theory has progressively become more and more indispensable in physics, but rather in the novel attitudes toward mathematical formalism which it has engendered. As physicists have become increasingly resigned to the role of the observer as an inextricable facet of any physical phenomenon, they also have become less sanguine about the existence of any independent preordained natural metric. In their practice, the specification of a metric has come to be seen as the generalization of an equivalence relation, which imposes a symmetry group upon a given state space (Rosen, 1983, p. 142). Hence modern mathematical formalism in physics tends to consist of the postulation of judiciously chosen symmetries with an eye toward the self-conscious construction of the meaning of natural order. Systems with very few salient features are asserted to possess powerful symmetries. For example, in mechanics the absence of all forces is defined as spatial symmetry. In any case where things persist in shooting off to the right, this is interpreted as evidence that we have discovered some external force or influence. The moral of this tale would seem to be that when faced with the phenomenological confusion besetting an empirical question, the first step is to ask: what symmetries am I willing to suggest characterize this situation? The next step is to define order as regular alterations of that symmetry. "Order is broken symmetry" (Salam, in Mulvey, 1981, p. 111) is the slogan of late-twentieth-century physics.

We have already had occasion to observe in section §10 that the track record of economics in justifying its favored conceptions of order has left something to be desired. Instead of stressing the importance of research into the meaning and implications of successful coordination of economic activity, economists attempted to create the impression of natural order by appropriation of a physics metaphor, and then found the critical notions of competition, equilibrium and so forth dictated to them by their newly adopted mathematical procedures.

Although perhaps the most legitimate research program in economics should generate its own mathematical tools simultaneously with its development of the economic theory, the present author is not at all sanguine about the likelihood of that prospect. The history of the economists' envy of the physicists is a heavy burden, not easily or lightly discarded. The interaction of physical and social metaphor pervades our thought in more ways than we might at first imagine. Moreover, mathematical expertise has itself become so separated from practical application in the modern disciplinary boundaries of the university, that sociological forces also militate against that scenario (Kline, 1980). A more realistic and modest proposal would be that, if we are to get our mathematical metaphors from physics, let us at least do it self-consciously, and with greater discrimination and subtlety than did our neoclassical forebears. Instead of arbitrarily appropriating this or that particular physical model as a metaphor, perhaps it would be more useful to contemplate the larger pattern of mathematical theory in the physics of the twentieth century. In this respect, the deployment of symmetry concepts and abstract algebra provides a framework for the conceptualization of order which is not tethered to any particular physical model. In the older, pre-Kuhnian sense, it can serve as a paradigm of explanation.

And so we arrive at the kernel of truth within Defense_1: the question of the appropriateness of mathematical techniques in economics cannot be separated from the conception of order in economic theory. Such an awareness must foster a skepticism toward prepackaged mathematical techniques taken from the physical sciences. The trepidation with which some would regard such a research program might derive from an impression that it would involve repudiation of three centuries of economic thought, leaving us to start, as it were, with a blank slate.

Luckily, the situation is not so drastic as all that.

§20. When and if we revise our understanding of what it means to conduct a self-conscious mathematical economics, we shall also revise our roster of whom we believe to have been legitimately creative mathematical economists. Contrary to the claim of Schumpeter quoted above in section §13, we should like to seriously entertain the idea that Marx was a seminal mathematical economist. By this statement we do not intend to refer to the schemes of expanded reproduction, or the algebra of the transformation problem found in volume III of *Capital*. Neither do we desire to praise the labour theory of value as an insightful manipulation of quantitative concepts.[12] Instead, the specifically mathematical contribution of Marx to economic theory is to be found in the first six chapters of volume I of *Capital*, in the discussion of the problems surrounding the conceptualization of a commodity. These chapters display the beginnings of a self-conscious examination of the problems of symmetry and order described above in section §19, and as such might serve as a point of departure for a reconstruction of mathematical economics.

It is very easy for the modern reader to discount the early parts of *Capital*, where Marx searches for the "common element" that permits the comparison of different commodities, as a regrettable metaphysical residuum of his Hegelian training. A different perspective will reveal this to be an intemperate attitude. The first consideration that should help us read these passages in a new light is the realization that much of the history of economic thought has been absorbed with a question that remains unresolved to this very day: Are "normal" trades the exchange of equivalents, or not? What is the meaning and significance of equivalence of value? The rise of neoclassical theory acted to banish this problem from overt discussion, but did not resolve it. One might initially think that neoclassicism settled the issue by placing itself squarely in the camp of those who maintained trade was of nonequivalents, in the sense that the total utilities to each transactor of any given commodity are divergent; but in practice, the situation is not so clearly defined. First, problems of the trade of equivalents have been recast so as to be subsumed under controversies over the cardinality of utility and/or various inconsistent claims with respect to the interpersonal comparison of utility. Second, the issue was avoided, in part, through the imposition of the law of one price as a condition of equilibrium (Bausor, this vol., chap. 4). Third, the presumption of the trade of equivalents has surreptitiously reentered neoclassical theory through such expedients as the discounting of future utility in order to consititute a present price, and the definition, popular in financial theory, of an efficient market as one which arbitrages away all divergent valuations.

Marx deserves attention because he correctly identifies the question of the trade of equivalents as the necessary point of departure for a mathematical economics; it is the other side of the coin of a theory of economic order. Equivalence in trade provides the benchmark and the definition of the putative voluntary character of trade, as was argued by many before Marx (cf. Mirowski, chap. 5, forthcoming). More importantly for our purposes, in the most elementary sense, there can be no equilibrium of nonequivalents in the absence of a prior specification of an equivalence relation. The absence of all forces for change are conceptualized as the equivalence of some critical index. But then, once an equivalence relation is posited for trade, then the stability of nonequivalent "equilibria" becomes adventitious and problematic. This is one way to understand the vagaries of the history of game theory (see this vol., chap. 7), as well as the history of neoclassicism: the law of one price, in conjunction with the imposition of an "auctioneer" or trade coordinator whose job it is to enforce it, are required in order to impose a single metric upon an otherwise chaotic agglomeration of preferences.

This insight can be rephrased in terms of Georgescu-Roegen's work quoted above in section §16. Laws generally take the form of simple linear relations because two or more cardinal variables have an immediate connection: there is no qualitative residual which remains uncaptured in the statement of the law. To insist that "normal" trades are exchanges of nonequivalents is to condemn economic theory to the partial and flawed quantification of economic relations, and thus to relinquish all hope of finding economic laws. To posit the equilibrium trade of nonequivalents is to assert that a set of fundamental quantitative considerations directly govern trade, and yet are beyond the ken of mathematical expression. To state this in terms of the physics metaphor: since there exists no symmetric ground-state which is characteristic of the absence of all forces, there are no guidelines as to how one should conceptualize the manifestation of forces outside of the ground-state (Weyl, 1952, p. 25).

The trade of equivalents is not an empirical issue. For Marx, it was a prior condition for the quantitative comprehension of a capitalist economy. If one accepts this viewpoint, then most of the Marxian prose about the search for an illusive common element shared by all commodities can be reinterpreted in more modern terms as a search for the appropriate abstract algebra to provide the structure requisite for capitalist exchange, and which would serve as the vehicle for the equivalence relation. In this reading, the first six chapters of *Capital* are divided up into preliminary remarks on the conditions any such algebra must meet, then a sequence of successive abstractions or approximations to the algebra from pure barter to a fully monetized economy, and finally to the invocation of symmetry conditions isomorphic to the equivalence relation for the purpose of isolating broken symmetries. Notably, these discussions of the algebraic characteristics of trade take place entirely prior to any specification of the mechanisms of price setting.

Accepting the trade of equivalents as a theoretical imperative, Marx asks what format the abstract algebra should assume. He then proceeds to assert a thesis, broached above in section §16, that economic quantities are not isomorphic to the algebras which characterize their physical constituents: "This common element cannot be a geometrical, physical, chemical or other natural property of commodities. Such properties come into consideration only to the extent that they make the commodities useful" (Marx, 1977, p. 127). Thus Lancaster's insight that the metric of use is not the metric of exchange was broached over one hundred years ago. Next, as a corollary to this first thesis, he insists that a physical commodity cannot be used to measure itself in exchange (Marx, 1977, p. 140). Translating this into more modern concerns, *contra* Clower, commodities do not virtually trade for themselves. Although it would be excessive to credit Marx with understanding of the formal aspects of this problem, this condition is a necessary prerequisite for the presence of an identity element in group theory. If for every a, the result $a \times a = a$, then there can exist no unique a^{-1}. Moreover, the construction of any equivalence relation must begin with the imposition of the postulate of reflexivity (i.e., $a = a$), a condition virtual self-trade tends to undermine (Rosen, 1983, p. 26). Denial of virtual self-trade analytically posits an algebra of commodity trade separate and distinct from an algebra of physical qualities. Comprehension of this fact prompts doubts about the logic of any economic theory asserting that any commodity is by itself sufficiently capable of serving as "numeraire."

After these preliminary considerations, there follows a section of *Capital* that has baffled many commentators. Here Marx posits a sequence of four "forms of value": the simple relative form, the expanded relative form, the general form, and the money form. This profusion of differing forms of value would surely seem superfluous unless one understood them as successive algebras which potentially might characterize exchange. In order to justify this interpretation, let us recast them in terms of modern algebra.

A *simple relative* algebra would correspond to our elaboration of Clower's simple barter economy. Within this format, for every bundle of commodity a traded for a bundle of commodity b, $a \textcircled{T} b = b$. Marx here insists that this is an incomplete and degenerate conception of value: "The expression of the value of the commodity A in terms of any other commodity B merely distinguishes the value of A from its use-value, and therefore merely places A in an exchange relation with any particular single different kind of commodity, instead of representing A's qualitative equality with all other commodities and its quantitative proportionality to them" (Marx, 1977, p. 154). In other words, this conception of the operation of exchange precludes any algebraic group structure.

To illustrate this point, consider the following four-good barter economy, consisting of endowment bundles (a, b, c, d).

Table 6–1: Marx's Simple Relative Form of Value

$\textcircled{T}$?	a	b	c	d
a	?	b	c	d
b	a	?	c	d
c	a	b	?	d
d	a	b	c	?

Ignoring for the moment the question of what should be entered on the diagonal, we can immediately observe that this particular specification of barter can never be represented by an algebraic group, because a group table can only display a single appearance of any element of the set in any row or columm. Even if we should attempt to impose an external identity element upon this structure by replacing all of the question marks with e, each element would still lack a unique identity. This occurs because $b \textcircled{T} a = c \textcircled{T} \mathrm{a} = d \textcircled{T} a = a$, so that appending a further trade for a, we find $b = c = d$. These exchanges fail to display a distinct identity and a distinct inverse, or as Marx puts it, there is no coherent expression of value. Further, this is a closed and finite system, and as such, is incapable of expressing the abstract unity of trade amidst the phenomenal diversity of goods, the quantitative character of value as distinct from the qualitative differentiation of physical manifestation of endowment bundles. Just as in Clower's case, there can be no number in this system. There are no symmetries, so there is no conserved entity. Equivalence is not sufficiently defined.

The movement to an *expanded relative* algebra is due to the recognition that value in exchange cannot arise in an isolated barter situation, but rather must be itself premised upon the supposition of an infinite expansion of commodities, even if this expansion is only virtual.[13] The quantitative conception of value is not contingent upon or limited by the (arbitrary) actual endowments present in the marketplace. In modern terms, the "expanded relative" algebra postulates an operation upon an infinite set. A single particular generic commodity is asserted to conform to the operation of the addition of integers:

Table 6–2: Marx's Expanded Relative Form of Value

+	*0*	*1*	*2*	*3*	*4*	....
	1	2	3	4	5	....
	2	3	4	5	6	....
	3	4	5	6	7	....
	4	5	6	7	8	....
	:	:	:	:	:	
	:	:	:	:	:	

Initially, this form of value seems to violate Marx's proscription that a commodity cannot trade for itself. A more careful interpretation would suggest that some specific commodity is made subject to the algebra of addition of its own units independent of the operation of trade. Notice that these units are not "natural", but rather externally imposed and enforced, since we have as yet no analytical idea of the reasons why traders may decide to hold this commodity. Superimposed upon the algebra of this particular commodity is the operation of exchange for other endowments, which, as yet, possesses no algebraic structure. If we designate the unit of the chosen algebraic commodity n, then the operation of trade can be represented by a roster of permissible trades:

$$4n \textcircled{T} a = a; \quad (4n \textcircled{T} a) + (4n \textcircled{T} a) = a + a'; \ldots$$
$$12n \textcircled{T} b = b; (12n \textcircled{T} b) + (12n \textcircled{T} b) = b + b'; \ldots$$
$$27n \textcircled{T} c = c; \text{ and so on.}$$

In the expanded relative form, one might jump to the conclusion that by means of the operation of exchange all commodities become subject to the same algebra of addition as the chosen algebraic commodity, but this would be premature. As Marx suggests, "The defects of the expanded relative form are reflected in the corresponding (simple) relative form" (Marx, 1977, p. 156). We can observe that the operation of exchange still cannot constitute a group, because all trades still take the form of $x \textcircled{T} y = y$. One might object that the existence of the algebraic commodity could be employed to obviate this criticism in the following manner: repeat the trade $4n \textcircled{T} a = a$ three separate times, and then reverse the operation so that $3a \textcircled{T} 12n = 12n$, $12n \textcircled{T} b = b$, and therefore $3a = b$. The flaw in this reasoning is that the algebra of the particular commodity cannot be assumed to apply to other commodities without the imposition of further severe restrictions. In this instance, there is as yet no unique identity element corresponding to the operation of exchange, so there is no reason to believe that the repetition of any given trade will produce the identical result. (That is, we do not have reason to believe that $(4n \textcircled{T} a)$ followed by $(4n \textcircled{T} a)$ results in $2a$.) Even more critically, we have no reason to believe that the operation of exchange has an inverse; for example, that $4n \textcircled{T} a = a$ implies that $a \textcircled{T} 4n = 4n$. These are not merely technical caveats. Allowing these amendations to the theory of value would presuppose that exchanges have been standardized in such a manner that a sequence of trades over time can be treated as isomorphic to multiple trades at a single point in time and space, although a little introspection should reveal that there is little in our experience that would render this axiom self-evident. Moreover, as we indicated in section §18, neither is it obvious that all trades are commutative. An imposition of commutivity would imply that any trade that is contracted can be undone, that the activity of exchange is reversible, and that some value characteristic of commodities is conserved. Finally, it is not obvious that the order in which trades are consummated has no influence upon the final outcome. In the expanded relative form, the only thing that may legitimately be said to be conserved is the identity of the single algebraic commodity. Therefore, the equality relation defined over the exchange operation remains deficient and degenerate because the terms on both sides of the equation cannot change places across the "equals" sign (Marx, 1977, p. 157). In more technical terms, a semi-group will only possess an equivalence relation if the operation is transitive, reflexive, and symmetric (Ljapin, 1974, p. 36).

The *general form* of value carries the elaboration of symmetries two steps further. First, it posits the requirement that only generic ("freely reproducible") commodities be taken under consideration, and that these commodities be treated symmetrically with the numeraire commodity of the previous "expanded relative" form. Thus each generic commodity, considered in isolation, is required to conform to the infinite algebraic group of addition. Each of these additive groups is symmetric, which implies that the global quantity of the commodity is conserved with respect to the agglomeration of commodities into bundles. Economically, apples can be added to apples; oranges can be added with oranges. The additive group of each of the commodities is isomorphic to that characteristic of the other commodities; indeed, they are identical. They thus all share the same identity element, namely, the zero. In economic terms, we are no longer tethered to a given configuration of endowments in a particular marketplace; instead we now contemplate an infinitely expandable economy.

Only at this stage of value are goods being treated as if there were no qualitative distinctions being made between any finite sequences of their generic units; the traders view them as indifferent manifestations of the same economic object. Thus it is only at the stage of the general form of value that the attributes of the traders themselves and not just the physical attributes of the commodities enter into the proceedings. A prerequisite of a regularized algebra of exchange is the existence of traders socialized to accept and acquiesce in the very existence of generic commodities.

The second aspect of the general form of value is the introduction of the conception of exchange as the composition of mappings of the individual groups associated with each generic commodity. In the example presented in Table 6–3, exchange is portrayed as a mapping of the "units" of commodity A into the "units" of commodity B according to the map α; whereas the reverse exchange is portrayed as a map β from B to A. When presented in this manner, the composition of mappings from one commodity group to another is entirely general, and therefore can express any conceivable configuration of price determination. The imposition of certain restrictions upon the mappings will begin to delimit the forms which prices may assume. For example, if the mappings $\{\alpha, \alpha', \beta, \beta', \gamma, \gamma'\}$ are all "onto," then all quantities of the second commodity are assigned some quantity of the first commodity in exchange. If the composition of these mappings is "one to one," then there is at most one quantity of the second commodity which is assigned to some quantity of the first commodity in exchange. Unless the composition of mappings is not both one to one and onto, trades will not be determinate, at least in the sense of leading to unique outcomes.

Even with these assumptions, this "general form of value" is inadequate to quantify the operation of exchange. One way to see this is to note that, in the general form, prices are not expressed as ratios; rather, they are complicated functions of the quantities of both commodities involved, may not be additive, and may not be the same for different units of the same commodity. Moreover, the operation of exchange is not yet well-defined, because the absence of closure in the second step of Table 6–3 precludes the imposition of the simplest algebraic structure. The heart of the problem is that the mappings have not yet been sufficiently abstracted from the identities of the commodities themselves.

The gist of Marx's general form of value is that the algebraic properties of commodities do not determine the algebraic properties of exchange. Trade itself must also be conceptualized as a group. As (Marx, 1971, p. 143) put it in his critique of Samuel Bailey:

[The object is to explain] ... the proportion in which one thing exchanges for an infinite mass of other things which have nothing in common with it ... for the proportion to be a fixed proportion, all those various heterogeneous things must be considered as proportionate representations of *some common unity*, an element quite different from their natural appearance or existence."

In order to achieve this status, there are further stringent restrictions which must be imposed. First, the mappings of commodities must comprise a closed set. Second, there must be an identity element in this set: some exchange which preserves all the other mappings and endows the operation with quantitative stability. Third, each mapping must have an inverse: an exchange which "undoes" the previous exchange. The appearance of the question marks in Table 6–3 signals the absence of the latter attributes: there is as yet no map which takes a commodity group back into itself, and there is no clear idea of the outcome of the reversal of an exchange, such as the composition of α and β.

Our discussion in section §18 above of the incoherence of virtual self-exchange should make us very wary of the "natural" assumption that the identity element in exchange is provided by the self-identity of the commodity itself. One thing we do not observe in markets is people swapping identical commodities. This means that the commodity groups developed in the expanded relative form of value cannot provide the basis for the group properties of exchange. Instead, what is required is that the very notion of a mapping of a commodity group has to be redefined in terms of a map from the commodity to some index M such that:

$$A \xrightarrow{\alpha} M, M \xrightarrow{\alpha} A; \quad B \xrightarrow{\beta} M', M' \xrightarrow{\beta} B; \quad C \xrightarrow{\gamma} M'', M'' \xrightarrow{\gamma} C.$$

What is this intermediate mapping which will serve to render trades a quantitative phenomenon? The artifact which provides an identity element for the group of exchange (as opposed to the groups of generic commodities) is *money*.

Thus we arrive at Marx's fourth and final form of value, the money form. In every value form prior to the money form, prices were not expressed as numbers because the structure of exchange could not meet the requirements of an equivalence relation. Only by means of the imposition of a group structure which exhibits the same composition of mappings independent of the theory of price will the act of exchange be the exchange of equivalents. Money is the artificially instituted invariant of any price system, the identity map in the group of exchange. Now we can begin to rephrase Clower's insight, and to make it more precise: a monetary system must exhibit certain attributes which cannot be found in an economy constituted solely of arbitrary physical endowments, and one of these attributes must be the existence of a unique money commodity. As (Marx, 1977, p. 190) wrote, "a duplication of the measure of value contradicts the function of that measure." Restating it in the terminology of abstract algebra, a group may only possess one identity element.

Table 6–3: Marx's General Form of Value

First Step: Individual Commodity Groups

commodity A

0	1	2	3	4....
1	2	3	4	5....
2	3	4	5	6....
3	4	5	6	7....
⋮	⋮	⋮	⋮	⋮

commodity B

0	1	2	3	4....
1	2	3	4	5....
2	3	4	5	6....
3	4	5	6	7....
⋮	⋮	⋮	⋮	⋮

commodity C

0	1	2	3	4....
1	2	3	4	5....
2	3	4	5	6....
3	4	5	6	7....
⋮	⋮	⋮	⋮	⋮

Second Step: Trade as a Composition of Commodity Groups

	A	*B*	*C*
A	?	α	α'
B	β	?	β'
C	γ	γ'	?

$A \xrightarrow{\alpha} B \qquad A \xrightarrow{\alpha'} C \qquad C \xrightarrow{\gamma} A$

$B \xrightarrow{\beta} A \qquad B \xrightarrow{\beta'} C \qquad C \xrightarrow{\gamma'} B$

Table 6–4 Money Form of Value

1	α	β	γ	α/β	α/γ	β/α	γ/α	$\cdots$
α	1	$\frac{\beta}{\alpha}$	$\frac{\gamma}{\alpha}$	$\frac{1}{\beta}$	$\frac{1}{\gamma}$	$\frac{\beta}{\alpha\alpha}$	$\frac{\gamma}{\alpha\alpha}$	$\cdots$
β	$\frac{\alpha}{\beta}$	1	$\frac{\gamma}{\beta}$	$\frac{\alpha}{\beta\beta}$	$\frac{\alpha}{\beta\gamma}$	$\frac{1}{\alpha}$	$\frac{\gamma}{\beta\gamma}$	
γ	$\frac{\alpha}{\gamma}$	$\frac{\beta}{\gamma}$	1	$\frac{\alpha}{\gamma\beta}$	$\frac{\alpha}{\gamma\gamma}$	$\frac{\beta}{\gamma\alpha}$	$\frac{1}{\alpha}$	
$\frac{\alpha}{\beta}$	β	$\frac{\beta\beta}{\alpha}$	$\frac{\beta\gamma}{\alpha}$	1	$\frac{\beta}{\gamma}$	$\frac{\beta\beta}{\alpha\alpha}$	$\frac{\beta\gamma}{\alpha\alpha}$	
$\frac{\alpha}{\gamma}$	γ	$\frac{\gamma\beta}{\alpha}$	$\frac{\gamma\gamma}{\alpha}$	$\frac{\gamma}{\beta}$	1	$\frac{\gamma\beta}{\alpha\alpha}$	$\frac{\gamma\gamma}{\alpha\alpha}$	
$\vdots$								

Finally, in Table 6–4, we observe actual prices. In this table, there are no longer any physical commodities *per se*. There are only abstract quantities of money which act as the linear mappings from one commodity group to another. The entire table is based upon the principle that one money unit equals $\alpha(\mathrm{A}) = \beta(\mathrm{B}) = \gamma(\mathrm{C})$. Because the theory of value is analytically prior to any theory of price, there is no explanation of the actual values (α, β, γ); a further theory is required to make them determinate. All the table says is (in the first row) the price of α units of A is one money unit, the price of β units of B in terms of A is β/α, the price of γ units of C in terms of A is γ/α, the price of α/β units of A in terms of money is $1/\beta$, and so on. Although the set of generic commodities generating money prices is only of order three in this example, the resulting group of exchange is of infinite order, but is closed and has unique identity and inverse elements, as can be observed from the structure of Table 6–4. Prices are explicitly rational numbers, and the group is Abelian, as can be observed from the skew-symmetry of the table.

The Abelian character of the money form of value is very critical to the understanding of the way in which a money economy differs from a barter economy. The existence of money creates the transitive structure of exchange. In the example in Table 6–4, one unit of B is traded for β units of money, which can then be traded for α/β units of A. These α/β units of A are then traded for money, which in turn is used to purchase C at the rate of γ/α. The final result of $(\alpha/\beta)\times(\gamma/\alpha) = \gamma/\beta$ is the same ratio which would be found in a more direct exchange of B for money and the result for C. Only in the money form of value is this conception of equivalence in exchange well-defined.

Having persevered through this difficult section, the reader may still be puzzled by the insistence that the operation of exchange conform to a group structure. Have we labored mightily only to demonstrate the obvious, that prices are expressed as rational numbers in a monetary economy? On the contrary: we are now prepared to explicitly define the prerequisites of legitimate quantitative exchange in a monetary economy. They are: (1) The commodity should preserve its identity through the exchange process (Sohn-Rethel, 1978); (2) buying nothing should cost nothing; (3) the order in which the items are presented for purchase should not influence the total amount paid for an aggregate; (4) dividing the aggregate into subsets and paying for each subset separately should not affect the total sum paid for the aggregate; (5) if an item is bought and then returned, the net result should be zero; and (6), everyone should pay the same price for the same item. In other words, we have identified legitimate trades as *symmetric* trades. (Actually, the group matrix is skew-symmetric.) One should not interpret this stricture to mean that all trades conform to these conditions in any and all circumstances; casual empiricism suggests the opposite. One should instead interpret these conditions as the ideal, or the benchmark, of legitimate exchange: these are the ideal conditions which sanction the imposition of rational numbers (in the guise of prices) upon exchanges. Another way of stating this is to say that rational prices require that *value is conserved in exchange*. As long as trades are constrained to be legitimate in this sense, then "value" exists as a phenomenon apart from the physical characteristics of any particular commodity, possessing a stability that is consistent with expression as a rational number. The fact that value as a quantity assumes a separate existence suggested to Marx that value was embodied in the commodity in the form of abstract labor time; but we should observe that the former idea is neither necessary nor sufficient for the latter to be true.[14]

The critical importance of the symmetry conditions and the group structure of exchange for Marx was that it provided a framework within which he could examine what was, in his view, the most vexing and most significant problem in all of political economy: where did the "extra" or surplus value come from? What are the ultimate wellsprings of economic expansion? Marx saw quite clearly what neoclassicals forget: "With reference to use-value, it can indeed be said that exchange is a transaction by which both sides gain. It is otherwise with exchange value" (Marx, 1977, p. 259). If the rules of legitimate exchange imply that value is conserved in the process of trade, then the process of legitimate trade cannot be the locus of economic growth. "In its pure form, the exchange of commodities is the exchange of equivalents, and this is not a method of increasing value" (Marx, 1977, p. 261). If we were to construct an analogy with twentieth-century physics, we would see the search for the ultimate source(s) of profit and growth as the search for the locus of broken symmetry. This is the logical beginning of a theory of economic order. Since a buyer is also a seller, a producer also a consumer, the explanation of surplus must be located in some subset of the economy where the basic symmetries of legitimate trade are either absent or broken. This structure of explanation must hold whether or not one is a partisan of the labor theory of value. It follows directly from the fact that prices are quantitative.

§21. It is a pity that Marx's work on the formal aspects of value just happened to antedate the formal development of group theory. Later in the century, searching for a developed formalism, economics turned to nineteenth-century physics to provide the paradigm, and as a direct result of that initial choice, economists became advocates of the dogma that exchange was "naturally" quantitative, believing that their discipline was founded on physical algebras provided by nature. As Marx put it with his customary ascerbity, it encouraged "the illusion to arise that all commodities can simultaneously be imprinted with the stamp of direct exchangeability, in the same way it might be imagined that all Catholics can be popes" (Marx, 1977, p. 161).

There is nothing simple about a commodity, and there is nothing natural about the quantitative fact of its exchange. If we might state the major thesis of this paper in a direct and provocative manner: only certain forms of mathematics are appropriate to the discussion of the economic sphere in modern society, and only those forms are isomorphic to the artificially instituted algebra of capitalist exchange. The social construction of the algebra of exchange takes place on two levels: the first, Marx's relative value form, is the construction of the generic commodity, such that there are a class of "identical" objects which can be characterized by a single number; and the second, similar to Marx's general and money forms, is the creation of a value index separate from the commodities themselves, which possesses its own (somewhat different) algebraic character. These stages are simultaneously a framework for economic analysis and a rough description of the actual dynamic of capitalist development. Many historians have noticed the trend toward the standardization of commodities and toward the expendability of any particular human personality in the production process as part and parcel of capitalist economic history, but few have understood it as necessarily constitutive of the creation of an algebra which will structure and govern trade. The development of the institutions of money and accounting have also been claimed to accompany capitalist development, but most (with the exception of Sombart and a few others) have seen them as an insignificant subset of technological innovation, whose only purpose is to grease the wheels of a preexistent trade. These historical phenomena, which neoclassical economics has tended to treat as adventitious or of secondary importance, are precisely the locations of the social construction of the algebra of exchange.

The social construction of an economic metric is inherently an historical and institutional phenomenon. Serious research into the evolution of this process would carry us too far afield from our present concerns. Nonetheless, it is critical for our present argument to insist that the construction of the quantitative incarnation of commodities and prices is never comprehensive nor complete: it is an ongoing affair. Money is such a protean institution that, as soon as a government seems to fix its identity through legal tender legislation and the sanction of legitimate credit institutions, the actors contrive and conspire to make it something else (Kindleberger, 1984). Or, in the same vein, as soon as an industry seems to succeed in standardizing a commodity, technological change and product differentiation undoes the situation. The social construction of value is doomed to the same fate as Sisyphus: no sooner is the illusion of the identity through time fabricated, then the very normal operation of the system serves to undermine it.

The history of Western economic reasoning is the story of a futile search for the natural value unit, be it gold, or abstract labor, or the standard commodity, or generic abstract utility. Once discovered, it is always promised that this holy grail will once and for all put an end to the confusion engendered by social change. This quest is quixotic; yet, also, it has been one of the prime motivations behind the mathematization of the economics discipline to date. Had the neoclassical partisans of the mathematical method paid more attention to the foundations of mathematics, they might have become more sensitive to the futility of their venture. After Gödel, few believe that any formal algebra can be both fully complete and fully consistent. Moreover, the economic actors already behave as if they knew it.

To see the quest for a natural economic metric as futile is not to counsel despair, however. Instead, it envisions that the reconstruction of a mathematical economics will be at least as pragmatic as the economic actors whose aims it seeks to describe. The economic actors do not fully "understand" the system (contrary to the faddish peccadillos of the rational expectations school); but they do have a very real need to make causal claims about their activities in the economic sphere. In order to do so, they impose strong symmetries upon the processes of trade, in the form of the six conditions described above in section §20. The postulation of such symmetries is decisive, because it implies the simultaneous construction of an equivalence principle (Rosen, 1983, p. 108). In this instance, it is interpreted as a mandate that legitimate trades are trades of equivalents. The conjuration of equivalence is necessary for the construction of causal statements, in the sense that equivalent states of a cause then imply equivalent states of an effect. In physics, one links causal states with effect states by imposing the restriction that both sets of states possess the same energy. In economics, one causally links the antecedents with the consequences of an exchange by imposing the restriction that both states possess the same value. For the mathematical economist, this will mean that the group properties of any chosen formalism will be severely restricted.

In the most general of theories, the mathematical economist will employ the organizing principle that the symmetry group of the cause should be a subgroup of the symmetry group of the effect (Rosen, 1983, p. 117). This heuristic principle can help further research in two different ways. The first, which Rosen (1983, p. 119) calls the "minimalistic use," takes a known cause and works out the minimal symmetry of the effect. An example of this research strategy has already been developed in this paper. If exchange conforms to a certain algebraic group, then it is a theorem that there can exist but one unique identity element. This theorem can be translated into the economic sphere by showing that any economic system predicated upon two or more monetary units or commodity standards (such as a bimetallic currency) will evince an unstable measure of value. The second way to use the symmetry principle is what Rosen (1983, p. 136) calls the "maximalistic use." Here one isolates a known effect and attempts to locate an unknown cause. If the symmetry characteristics of the effect are known, then the symmetry principle sets an upper bound on the symmetry characteristics of the cause. Quoting Rosen:

> . . . the first step towards a theory is to determine the ideal symmetry that is only approximated by the phenomena. . . . Then to obtain as symmetric a cause as possible we try to construct a theory such that the cause will have a dominant part . . . possessing the ideal symmetry of the effect, and another, symmetry-breaking part, which does not have that symmetry. In the (possibly hypothetical) limit of complete absence of symmetry breaking, the dominant part of the cause produces the ideal symmetry of the phenomena, while the symmetry-breaking part brings about the deviation from the ideal symmetry. (Rosen, 1983, p. 136)

The maximalistic use of the symmetry principle could serve to clear up one of the most convoluted and muddled areas in economic theory: the theory of profit. The effect we wish to explain is the expansion of value in the capitalistic process. This is an asymmetry, a change in the magnitude of value over time. To begin the explanation, we posit the symmetric base line of constant value through time. This is the previously discussed exchange of equivalents. Next, we posit a symmetry-breaking phenomenon which induces the deviation from ideal symmetry. One might accomplish this in the same manner as Marx, insisting that the value of the output of a production process is asymmetric with respect to the value of the wage, because the labor contract does not partake of the character of the exchange of equivalents. Or, as the author himself might suggest, the function of credit is to increase the aggregate magnitude of the value unit apart from the trade of equivalents. In either case, causal explanation then limits the potentials of what can be quantified, what algebras may be employed, what is conceived of as being constant, and so forth.

This would be the beginning of a mathematics grounded in economic theory, rather than vice versa.

Notes

[1] The position that the linguistic isolation of mathematicians is justified is softened considerably in (Koopmans, 1957). Nevertheless, the attitude that the isolation is the reader's, and *not* the writer's problem, can be traced back to the work of Walras (for example, Walras, 1960).

[2] See Kline, 1980, pp. 271–272; Wittgenstein, 1976, 1978; Wright, 1980; Hacking, 1984, pp. 101–111; and Putnam, 1983.

[3] Fisher (1926, pp. 85–86) openly displays this fact in a table which presents the correspondences between the physics and economics labels for variables in the same mathematical model. For a detailed commentary, see Mirowski (forthcoming, ch. 5). Although Fisher and the other neoclassicals did not realize it, one area in which the analogy did not carry over into economics was in the law of the conservation of energy. See Mirowski (1984b; 1984c).

[4] It has already been formally admitted that the axioms of revealed preference are isomorphic to a subset of thermodynamics. See Hurwicz and Richter (1979).

[5] There are many similarities between this analysis and the discussion in Georgescu-Roegen (1971) of "Arithmomorphism." See also the discussion in Katzner's essay in this volume, ch. 5.

[6] Quite obviously there exist neoclassical models which allow for inventory accumulation, inflexible prices, price discrimination, and so forth. What this statement means is that such models, by their very structure, cannot be members of the class of Walrasian or Arrow-Debreu models if they allow the so-called "disequilibrium phenomena" to feed back into the determination of a unique general equilibrium. This was the critical insight of Clower (1965). In actual practice, the models that purport to incorporate these phenomena finesse this problem by inevitably being cast in a Marshallian partial equilibrium framework.

[7] For a further elaboration of these issues, see chapter 4 by Bauser. The "law of one price" is a major component of the definition of equilibrium imported from physics. In brief, it states that all trades of generic units of a commodity will be contracted and realized at a single uniform price. Some further discussion can be found in Mirowski (forthcoming, ch. 5).

[8] If some believer in the inevitable progress of mathematical sophistication really needs a contemporary example, let him consult Georgescu-Roegen (1976, p. 286) for a critique of the errors of Frank Hahn.

[9] Partial exceptions to this sweeping generalization are found in Georgescu-Roegen (1971),

Katzner (1983), and chapter 5 of this volume. The author would like to acknowledge the influence of these seminal works.

[10]The Arrow-Debreu predisposition to characterize a commodity by an exhaustive enumeration of the accompanying state of the world (an apple at 8 P.M. on Tuesday on the Boston Common in the rain after a bout of jogging but before a drink with friends ...) would thus appear to undermine the very algebraic attributes upon which it leans so heavily to provide a metric. If, in essence, every commodity is unique in an economic sense, then there are no grounds for quantitive comparison, no cardinality, and certainly no prices. In respect to this problem, see the discussion of Georgescu-Roegen below.

[11]In recent conversations, Robert Clower has informed me that he produced an as-yet unpublished lengthy manuscript in the late 1960s which explored the implications of group theory for the issues broached in his 1967 article. I have not yet seen this manuscript.

[12]I have argued elsewhere that Marx was the last serious expositor of a labor theory of value precisely because developments in mathematics and physics caused substance theories of value to be superseded in the later nineteenth century. See Mirowski (forthcoming, ch. 4).

[13]This insight can be traced back to Aristotle's *Politics*. Aristotle (1962, pp. 21–29) contrasts the wealth of the household and barter trade, which he considers bounded, with exchange for the sake of acquisition, which is potentially boundless.

[14]Unfortunately, Marx's embodied labor values do not possess the properties necessary to qualify them as cardinal numbers. For elaboration, see Mirowski (forthcoming, ch. 4).

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