KARL MARX

MATHEMATICAL MANUSCRIPTS

TOGETHER WITH A SPECIAL SUPPLEMENT

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Marx and mathematics

INTRODUCTION

This special supplement to Marx's *Mathematical Manuscripts* has grown out of an attempt to write a new preface to it. The supplement has three parts.

PART ONE : HISTORY, contains materials pertaining to the history of evolution of Marx's mathematical investigations, to that of the work leading to the publication of their results and, a bibliography, listing the majority of existing publications the field. The bibliography is not exhaustive.

PART TWO : INVESTIGATIONS, contains four articles inspired by Marx's mathematical manuscripts. Three of them are translations from Russian. They relate Marx's mathematical investigations respectively to : the history of analysis, the study of history of mathematics in the erstwhile USSR and, the logic of Marx's *Capital*. The fourth article provides an outline of the problem of situating Marx's mathematical manuscripts in the history of ideas as a whole.

Parts one and two reflect the past — the work that has already been done. The question arises : where do we go from here ? Marx conducted his investigations in the 19th century, basing himself mainly upon the developments in analysis upto the end of 18th-beginning of 19th centuries (e.g., upto the time of Lagrange). We are living in the last decade of the 20th century. In Marx's lifetime, and even a couple of decades after his death — right upto the first decade of this century — practically, there existed *only one* (now called the classical) trend in mathematical analysis, but now there are *many* (classical, intuitionist, constructivist and non-standard — some of them overlap and, branch out into multiple sub-trends). Indeed, the fact that even to-day we speak of mathematics in the singular, reflects a particular view of it. We must also train ourselves to speak, in the plural, of the mathematics*es*. [This graaphic barbarism has been intoroduced to jolt the reader out of the prevalent singular use of the word *mathematics*, which is morphologically both plural and singular in English. In Bengali and Russian we have greater graphic clarity : *gonit* > *gonitsamuha* ; *matematika* > *matematiki.*] The ontological and epistemological consequences of these more recent developments in the history of mathematics are profound : that which was considered to be *one* has also revealed itself as *many*. The entire problematique of truth and certainty has entered into an era of radical reconstruction all over again. At long last, the strongest bastion of theoretical dogmatism — the monopoly of the classical mathematical paradigm — is crumbling before our own eyes. It is clear, that is why, that there is no point in beginning our investigations from just where Marx left his work unfinished — when he died in 1883. To-day, even a survey of the post-Lagrange developments must base itself upon the contemporary attainments, the frontiers of which are being extended daily, hourly.

PART THREE : MATHEMATICSES, has been planned to provide the reader of this volume with a perspectival update on the relevant developments. Owing to reasons beyond the control of the present author here the cut year is 1987 — when all the five articles included in this part were published in Russian, as part of the proceedings of a symposium on "The Regularities and Modern Tendencies of the Development of Mathematics", held in September 1985, in Obninsk.

All the articles included in this supplement are, to my knowledge, being published in English for the first time. All translations from Russian are mine. It will be noted that all the articles herein included — save my own — are translations from Russian. A number of factors determined this choice. A vast amount of relevant work exists in Russian, and our readers are generally unaware of them — this situation requires correction. Where a collective effort is needed, I was constrained to work single-handedly, without any kind of institutional support. I have deliberately excluded the materials already available in English, since our readers have greater access to them, thanks to our historical contacts with the English-using world. Thus, a very relevant article by *John Kadvany*, A Mathematical Bildungsroman // *History and Theory*, 1, 1989, pp. 25-42, which should otherwise have been reprinted in part three of this supplement, remains excluded. I strongly recommend it for any reader of Marx and Mathematics. I have heard that the Italian publications on Marx's mathematical manuscripts are really very good; but I do not read Italian ; in fact, apart from Russian I do not read any other European language. This personal limitation has also contributed to the inadequacies of this supplement. Let us hope that in future some one will make the other relevant materials accessible to us.

The limitations of the present volume and of this supplement will be overcome with the publication of a better and complete edition of all the mathematical manuscripts of Marx (some 400 pages of them still remain unpublished), as well as with the publication of newer and newer studies on them, executed with ever greater competence ; but more importantly, we must join hands in opening up new frontiers in mathematical theory and practice and, in the cognate disciplines (e.g., in informatics) and technologies, and thus carry forward the critical and transformative spirit embodied in the mathematical manuscripts of Karl Marx .

Calcutta, June 15, 1993. Pradip Baksi.

PART ONE : HISTORY

A NOTE

ON THE HISTORY OF COLLECTING, DECIPHERING, EDITING AND PUBLICATION OF MARX'S MATHEMATICAL MANUSCRIPTS

PRADIP BAKSI

Karl Marx died in 1883. A partial edition of his *Mathematical Manuscripts* came out in print 85 years after his death, in 1968. During the first years of this intervening period, these manuscripts arrived in Germany, together with the other unpublished manuscripts of Marx and Engels, from England. These manuscripts became the property of the Social Democratic Party of Germany[10]. We know that Engels considered Marx's mathematical manuscripts to be important. He expressed his desire to publish them, in his preface to the second (1885) edition of *Anti-Dühring*. This wish remained unfulfilled during his life time. Frederick Engels died in 1895. After his death, it was the Social Democratic Party of Germany that became primarily responsible for publishing the said manuscripts. This party failed to fulfill this responsibility. What is more, one of the important leaders and theoreticians, first of the Social Democratic Party and subsequently of the Communist Party of Germany, Franz Mehring (1846-1919) declared, at the behest of some mathematicians (of whom exactly, we do not know), that Marx's mathematical manuscripts are of no importance [19, p. 14 ; all references in this article are to the entries in the Bibliography appended at the end of Part One of this Supplement, pp.404-408]. Before the Russian revolution of 1917, a Russian revolutionary emigrant David Borisovich Ryazanov (Goldendakh) (1870-1938) worked for some time in the archives of the Social Democratic Party, in Berlin. At that time he noticed that a part of Marx's mathematical manuscripts was not there in the archives. He located them at the residence of an important leader of the Social Democratic Party, Eduard Bernstein (1850-1932). Subsequently Ryazanov approached a leader of the Austrian Social Democratic Party Frederick Adler (1879-1960) and, requested him to take the initiative for publishing the mathematical manuscripts of Marx. Ryazanov's attempt too failed to bear any fruit, but in the process ten mathematical note-books of Marx went into the personal custody of Adler.

After the revolution of 1917 a Marx-Engels Institute was established in Moscow and, Ryzanov was appointed its first director. This institute of Moscow acquired a contractual right to photo-copy the manuscripts of Marx and Engels, from the archives of the Social Democratic Party of Germany. In persuance of this contract Ryazanov and his colleagues demanded the mathematical manuscripts of Marx for photo-copying purposes. It was only then that the authorities of the archives of the Social Democratic Party of Germany could recover the aforementioned ten note-books of Marx from Adler. Ryazanov's efforts in this direction were reported in the July 1924 issue of *"Inprekor"* published from Vienna [64]. At long last in 1925 the Marx-Engles Institute of Moscow succeded in obtaining the photocopies of 865 pages of Marx's mathematical manuscripts [16,p.56.]. A German mathematician E. Gumbeil was already acquainted with these manuscripts. He was brought to Moscow and given the task of editing them. R. Mateika and R.S. Bogdan helped him in the task of deciphering the texts. In 1927 Gumbeil declared that the press-copy of Marx's mathematical manuscripts was ready [ibid]. However, some other associates of the Institute (for instance,

E. Kolman) had a different opinion in this regard [see : 9, p.185 and 29, p.101]. Ryazanov, the first director of the Institute, was expelled from the C.P.S.U in 1931. He was killed in 1938. Vladimir Viktorovich Adoratsky (1878-1945) became the next director of the Institute. Gumbeil was removed and, in his place the task of editing the mathematical manuscripts of Marx was given to Sofya Aleksandrovna Yanovskaya (1896-1966). Initially she was assisted by D.A. Rykov and A.E. Nahimouskaya. In 1933, on the occasion of the 50th death anniversary of Karl Marx, two of his articles on the nature of differential calculus and, an editorial article of Sm. Yanovskaya were published in the journal "*Pod Znamenem Marksizma*" and in a collection of essays entitled "*Marksizm i estestvoznanie*". Upto 1968, those who were interested in the said mss had to remain contented mainly with these publications. After 1932, a Swedish mathematician Wildhaber remained associated, for sometime, with the Moscow-team working on the mathematical manuscripts of Marx. A member of the Soviet delegation to the Tenth International Congress of Mathematicians (1932) declared in one of the sessions of the congress that the entirety of Marx's mathematical writings are going to be published soon [27]. This promise too remained unfulfilled. Arrived the Second world war. During the War the archives and the library of the Institute were shifted to some place in the Soviet Far East. Work of the Institute slowed down. After the War the pace of work picked up slowly. However, Sm. Yanovskaya — the editor of Marx's mathematical manuscripts — was also required to cope with the teaching of mathematical logic in the Moscow University and, with the task of translating and editing text-books of mathematical logic. Her health began to deteriorate. A Congress of Mathematicians was organised in 1950 at Budapest. Here the delegates from all the other socialist countries repeatedly asked the members of the Soviet delegation : when, at long last, are they going to publish the mathematical manuscripts of Marx ? The members of the Soviet delegation had no definite answer to this question [19, pp.205-206]. After the return of this delegation to the USSR, the responsible authorities began to take more vigorous steps. Now Sm. Yanovskaya was given a new assistant: Konstantin Alekseievich Rybnikov. An important event of the 1950s was the publication of a note of Marx entitled "*On The Concept Of Function*" in the journal "*Voprosy Filosofii* " No. 11, 1958)[11]. As the work entered into the 1960s, it was noticed that the manuscripts are opaque in quite a few places (similar problems were being encountered by the editors of many other manuscripts of Marx and Engels). That is why the question of verifying the text once more from the originals was posed with some urgency. Meanwhile — in fact before the Second World War — on the 19th of May 1938, the entire archives of the Social Democratic Party of Germany, inclusive of the manuscripts and letters of Marx and Engels, has become the property of the International Institute of Social History, Amsterdam (see : note [10]). That is why, a Soviet delegation was sent to Amsterdam in August 1964, in search of the necessary papers [19, p. 207]. A member of this delegation was Sm. Olga Konstantinovna Senekina. She noticed, that when — during 1924-1930 — the team of Soviet workers under the guidence of D.B. Ryazanov, was busy in photo-copying some 55,000 pages of the writings of Marx and Engels, then quite a few pages of Marx's mathematical manuscripts remained un-photocopied, due to inadvertence. Now the photo-copies of those pages were obtained. This pushed up the volume of the hitherto known mathematical manuscripts of Marx to nearly 1000 pages (see : the Preface to the 1968 edition of these mss). The editor of the 1968 edition of Marx's *Mathematical Manuscripts,* Sm. S.A. Yanovskaya died in 1966. Two years after her death, in 1968, a 639 page edition of Marx's mathematical manuscripts was brought out under the joint supervision of K.A. Rybnikov, O.K.Senekina and A.Rivkin. In the Preface to this edition, the work that went on around these mss prior to 1931, remains unreported. However, the work of the 1933-1968 period has been described in detail [PV,14-15]. An even more important "silence" of this Preface involves the fact that it is an incomplete edition. This becomes evident to anyone who cares to go through this edition. Why were certain pages of Marx's mathematical manuscripts dropped ? Apparently, the persons responsible for the publication considered these pages to be mathematically insignificant. It goes without saying, that such an approach towards the manuscripts of dead authors is — to say the least — unhistorical.

It may be mentioned here that the work of the Institute of Marxism-Leninism of the CPSU (which evolved out of the Marx-Engels Institute of Lenin's time) has always been affected by the power-struggle within the top leadership of that party. Beginning with Ryazanov, many competent workers were removed and killed. Those who were spared the hospitality of labour camps or death, left or were forced to leave the country. The noble work of editing and publishing the works of Marx and Engels became the subject-matter of self-promotion and intrigues of mean-minded and incompetent persons. In an atmosphere of all-round decadence of Soviet barrack socialism, quite a few generations of that society lost all interest in Marxism, thanks to the criminal activities of the party and state leaders. Those who retained some honest interest failed to remain in the good books of the powers that be. In these circumstances, the 1968 partial edition of Marx's mathematical manuscripts was left just like that for about two decades. Even more truncated editions were brought out in some other countries (see : the bibliography). Words went around that the 1968 edition is by and large satisfactory and, that it would be included in that form in the contemplated complete works of Marx and Engels*. This "consensus" was broken in the wake of Perestroika. A decision was taken towards the end of 1987, to prepare a new and complete edition of Marx's mathematical manuscripts on the basis of all the hitherto available papers. At this stage, the task of editing was given to Sm. Irina Konstantinovna Antonova of the Institute of Marxism-Leninism, Moscow. Towards the middle of 1988 she told the present author that she hopes to complete her part of the job by the end of 1990. Then it would require the approval of the experts of the Institutes of Marxism-Leninism of Berlin and Moscow, before publication. These plans have been overtaken by larger movements of history. Tumultuous changes have taken place by the end of 1991. First the G.D.R. and then the U.S.S.R. has ceased to exist. The ruling communist parties of these two countries have been abolished and the Institutes of Marxism-Leninism controlled by them have been wound up. The publication of a complete edition of Marx's mathematical manuscripts as part of the projected complete works of Marx and Engels has now become a matter of uncertainty all over again. When, where and how they would be published in future or whether they would be at all published ever — who knows ?

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* For a different opinion on this question see : *Malodshii* (PV, 423-424). — Ed.

Notes

1. In the summer of 1857 Marx began to write a series of economic manuscripts in order to sum up and systematize the results of his extensive economic research started in the 1840s and continued most intensively in the 1850s. (In the first half of 1850s he filled 24 paginated and several unpaginated notebooks with excerpts from the works of other economists, books of statistics, documents and periodicals.) These manuscripts were the preliminary versions of an extensive economic work in which he intended to investigate the laws governing the development of capitalist production and to criticise bourgeois political economy. Marx outlined the main points of this treatise in an unfinished draft of the 'Introduction' (one of the first manuscripts of the series) and in his letters to Engels, Lassalle and Weydemeyer. Further economic study prompted Marx to specify and change his original plan. The central place in the series is occupied by the extensive manuscript, *Critique of Political Economy* (widely known as *Grundrisse*), on which Marx worked from October 1857 to May 1858. In this preliminary draft of his future *Capital* Marx expounded his theory of surplus value. After the first instalment had been prepared for publication in 1859 under the title *A Contribution to the Critique of Political Economy*, Marx added several more manuscripts to the series in 1861.

   The manuscripts of 1857-61 were first published in German by the Institute of Marxism-Leninism of the C C C P S U in 1939 under the editorial heading *Grundrisse der Kritik der politischen Ökonomie (Rohentwurf)*. These manuscripts and *A Contribution to the Critique of Political Economy. Part One* are included in vols. 29 and 30 of the English Edition of the Collected Works of Karl Marx and Frederick Engels (Progress, Moscow, 1975-).

2. *Lange, Friedrich Albert* (1828-1875) : German philosopher, economist, neo-Kantian ; member of the Standing Committee of the General Association of German Workers (1864-66), member of the International, delegate to the Lausanne Congress (1867).

3. I began my inquiries about Hegel's mathematical manuscripts in 1980. In this connection, I received a letter from Dr. Helmut Schneider of the Hegel-Archiv, Ruhr-Universität Bochum. In this letter dated the 7th of February 1982, he stated that the mathematical manuscripts of Hegel are neither there in their archives, nor have these mss been published so far. — P. B.

4. *Moore, Samuel* (1838-1911) : English lawyer, member of the International, translated into English Volume One of *Capital* (in collaboration with Edward Aveling) and the *Manifesto of the Communist Party* ; friend of Marx and Engels.

5. In this connection see :

   a) *Zoltan, K.* Marx és a válságok törtvényeinek matematikai tanulmányozása [Marx and the mathematical investigations into the laws of crisis], *Közgazdasági Szemle*, Budapest, 1962, 12, pp. 1464-1483 ;

   b) *Dunayaeva, V.* K voprosu O matematicheskom metode v "Kapitale" K. Marksa [On the question of mathematical method in Karl Marx's "Capital"], *Voprosy Ekonomiki*, 1967, 8, pp. 18-30.

6. "Moor" was Marx's nickname in Marx family. His close friends also called him by that name [see : Marx-Engels Reminiscences. Bengali ed. Progress. M., 1976].
7. See : PV, 19 and 26 : "On the Concept of the Derived Function" and "On the Differential".
8. See : *Hegel, G.W.F.*, Science of Logic (Tr. : W.H. Jhonston and L.G. Struthers), G.Allen and Unwin, London, 1929 ; Volume One, Book One, Section Two, IIC — The Quantitative Infinity, pp. 241-332 [especially: IIC (c) — The Purpose Of The Differential Calculus Deduced From Its Application, pp. 291-320].
9. Here Engels expressed his desire to publish his *Dialectics of Nature* and Marx's *Mathematical Manuscripts* together. This wish of his remained unfulfilled. The *Dialectics Of Nature* was first published in 1925 and a part of Marx's *Mathematical Manuscripts* in 1933, an enlarged but nevertheless incomplete edition of the same came out in 1968.
10. For a general overview of the history of preservation, change of ownership and publication of the manuscripts, letters etc. of Marx and Engels, see : Saha, Dr. Panchanan — *Marx Engelser Pandulipi Kibhabe Raksha Pelo ?* Indo-GDR Friendship Society, Calcutta, 1983.
11. See : O Ponyatii Funktsii, *Voprosy Filosofii,* 1958, 11, pp. 89-95 & PV, 171-177.

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3. *Manoscritti Matematici,* "Dedalo Libri", Bari. 1975 ; Ed. Francesco Matarrese and Agusto Ponzio. Italian translation of Part I of (1.) above, plus two editiorial essays.
4. *Mathematical Manuscripts Of Karl Marx,* New Park Publications Ltd., London, 1983; Ed. Cyril Smith. English translation of Part I of (1.) above, plus the English translations: of the Preface, Appendix, and the relevant Notes of (1.); of three relevant letters of Marx-Engels and, E. Kolman's review of (1.); of the essay "Gegel i matematika" ("Hegel and mathematics") (1931) by E. Kolman and S. A. Yanovskaya; and the article "Hegel, Marx and Calculus" by C. Smith.
5. *Les manuscrits mathématiques,* "Economica", Paris, 1985; Ed. A. Alcouffe. French translation of Part I of (1.) above, plus the editorial essay : Marx, Hegel, et le "calcul".

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PART TWO : INVESTIGATIONS

INVESTIGATIONS INSPIRED BY MARX'S MATHEMATICAL MANUSCRIPTS : A SELECTION

MARX AND HADAMARD ON THE CONCEPT OF DIFFERENTIAL

VASILI IVANOVICH GLIVENKO

1. Two Points Of View on the concept of Differential.

In the history of differential calculus one comes across two basic points of view on the concept of differential.

According to the first, the concept of differential *immediately* reflects some external reality; for the sake of brevity we shall call it the o b j e c t i v e point of view. This was the point of view of the inventors of differential calculus. For them — the differential was an infinitesimal increment of the variable. The external reality reflected in the words " infinitesimal increment ", was somehow thought to be self-evident.

The concept of derivative was not there, its role was fulfilled by the quotient of two differentials. Thus, unlike our objective point of view of the derivative, in this conception the derivative was not viewed as an immediate reflection of some external reality.

Modern analysis too has retained the objective point of view of the differential, though here it has acquired a different meaning. Here , first of all, we separate from among the totality of available variables, those which we consider to be independent, and the differential of such a variable dx is simply considered to be its arbitrary finite increment Δx. For the remaining variables, which are functions of the separated independent variables, some other definitions of the differential are well known ; these definitions are different in form . For the sake of simplicity, we shall limit ourselves to a single function of a single variable x :

$$y = f(x).$$

Then according to Stolz's definition, used in the better text books of modern analysis (those of de la Vallée Poussin and Courant, for example) if $\Delta y = A \cdot \Delta x + \alpha \cdot \Delta x$, wherein A is not depèndent on Δx, and α together with Δx tends to zero, then $dy = A \cdot \Delta x$.

Briefly, the differential dy is the principal linear part of the finite increment Δy. Other definitions of the differential are also accepted : according to the most widespread among them,

$$dy = f'(x) \ : \ \Delta x \, ;$$

according to Cauchy's definition,

$$dy = \frac{\lim f(x + h \cdot \Delta x) - f(x)}{h} \qquad h \to 0 \, .$$

But when one intends to explain the most general meaning of these definitions, then one has recourse to Stolz's definition : it is not difficult to establish their equivalence with Stolz's definition.

There are deep going differences among the conceptions of the differential as an infinitesimal increment (in the meaning that was bestowed upon the words "infinitesimal increment" by the inventors of differential calculus), as an arbitrary finite increment or as its principal linear part ; but all the same in all these cases, we deal with the objective point of view about the concept of differential.

In both the cases the differential immediately reflects some external reality, every time, just like the variables x and y themselves.

According to the second point of view, the *derivative*

$$f'(x) = \lim \frac{\Delta y}{\Delta x} \qquad \Delta x \to 0$$

immediately reflects some external reality ; for the sake of brevity we shall call it the o p e r a t i o n a l point of view .

Here the concept of differential reflects the well known aspects of those mathematical operations, from which the definition of the derivative and the computations with the derivatives follow.

From this point of view, the differentials are *introduced* in the form of ratios of differentials, ratios — that are symbolized in the *derivatives* :

$$f'(x) = \frac{dy}{dx}\ .$$

After this, it is not difficult to understand, that the operations with the symbolic ratios $\frac{dy}{dx}$, according to the very rules that are applicable to the algebraic fractions, will not lead to any contradiction. But neither is it mandatory, that we seek an *immediate* interpretation of each and every result that follows from these operations. In particular, nothing obstructs us from viewing the formula

$$dy = f'(x)\, dx$$

(obtained by freeing the aforementioned formula of the derivative of the denominator), as only another expression of the formula

$$\frac{dy}{dx} = f'(x).$$

Substantiation of the operational point of view had to wait for a considerably longer period of time, than what was required for the substantiation of the objective point of view about the differential. The difficulty here was not with establishing the very *possibility* of thus, and not otherwise, interpreting the differential, but rather with the discovery of the *meaning* of such interpretation.

In this direction, the first methodologically exhaustive work was done by K. Marx; this work was written about fifty years ago, but was published only last year [K. Marks, *Matematicheskie Rukopisi* (Mathematical Manuscripts) // *Pod Znamenem Marksizma*, 1933, 1, str. 15-73]. J. Hadamard has treated the differential along the same lines, but differently, in his modern text book [J. Hadamard, *Cours d'Analyse*, Paris, Hermann, 1927, pp. 2-10.].

2. *The Differential of Marx and of Hadamard.*

The well known theorem about the differentiation of a function will play the principal role in what follows : let, as before,

$$y = f(x) ;$$

let x be the function of some variable t ; then

$$\frac{dy}{dx} = f'(x) \cdot \frac{dx}{dt} . \tag{1}$$

Marx's idea is as follows. When the starting point of the differential calculus, the equality

$$\frac{dy}{dx} = f'(x) ,$$

is taken isolatedly, then, at first, it has only a descriptive character. The real result of the operations, through which the derivative is determined, stands on the right hand side of this equality; the left hand side serves only as a symbol of those very operations,which lead to this result. But after the meaning of this symbol has been defined by such an equality, in consonance with this definition, it appears on both the sides of formula (1). [Hadamard proceeds from a formula , which is analogous to formula (1), but involves two variables : if

$$z = f(x , y) ,$$

then ,

$$\frac{dz}{dt} = \frac{df}{dx} \cdot \frac{dx}{dt} + \frac{df}{dy} \cdot \frac{dy}{dt} ;$$

Marx has used a particular instance of this last formula : if $z = x \cdot y$, then,

$$\frac{dz}{dt} = y \cdot \frac{dx}{dt} + x \cdot \frac{dy}{dt} .$$

However, we lose nothing by illustrating their deduction by the simpler formula (1).] But there we are already dealing with operations under similar symbols : they themselves become the object of a calculus. "Thereby the differential calculus appears as a specific type of calculus, already independently operating on its own ground ". Naturally, the formulae of this calculus have the meaning of *operational* formulae : thus, formula (1), upon establishing the connection between $\frac{dx}{dt}$ and $\frac{dy}{dt}$, thereby shows, what one must do,so that having $\frac{dx}{dt}$ one may obtain $\frac{dy}{dt}$. And from this very point of view this formula appears to be even more excessively complex. Since the nature of the variable t has no significance here, reference to this variable — to the differential dt — may be dropped altogether, just as one drops the common factor. According to Marx, this is not only p o s s i b l e, but also n e c e s s a r y : for the removal of the illusion, as though the formula (1) is true only in respect of some really independent variable. Thus we get a new formula

(2) $\quad dy = f'(x) \cdot dx$.

This is an operational *symbol*: once we obtain the formula (2) as a result of computations, it is enough to divide both the sides of this formula by dx, for obtaining the derivative ; it is enough to divide both the sides by dt — for obtaining formula (1) etc. Here the differential calculus as such finds its own natural fulfilment , and the " operational equation " (1), " as a preparatory equation, becomes superfluous, after it fulfils its task of supplying the general symbolic formula for differentiation " (2), " which directly leads us to our goal ". [we may recall , that for Marx , strictly speaking, the equations (1) and (2) are not at issue; he was concerned with the equations

$$\frac{d(xy)}{dt} = y \cdot \frac{dx}{dt} + x \cdot \frac{dy}{dt}$$

and

$$d(xy) = y \cdot dx + x \cdot dy \, .$$

But clearly, that does not change the affairs.]

Hadamard approaches the differential from the same operational point of view, but differently. Having established formula (1), he proposes to write out formula (2), by simply indicating this convention, and this alone, that whatever be the functional dependence of x and y on the parameter t, the equality (1) does hold good.

Hadamard insists on this definition of the differential, since it permits the use of the formulae containing operational symbols, for obtaining the derivatives. Let us assume, that computations with the differentials — in the sense that follows from Hadamard's definition — did indeed produce the formula,

$$dy = A \cdot dx \, .$$

Then one can assert that

$$f'(x) = A \, .$$

Actually, as per our assumption , for any t , we have :

$$\frac{dy}{dt} = A \cdot \frac{dx}{dt} \, ;$$

on the other hand, we know, that for any t :

$$\frac{dy}{dt} = f'(x) \cdot \frac{dx}{dt} \, .$$

From a comparison of these equalities, we get what is to be proved.

Hadamard defined the *second* differentials analogously. Proceeding from the formula (1), Hadamard states the dependence between the *second* derivatives to be

$$\frac{d^2y}{dt^2} = f'(x) \cdot \frac{d^2x}{dt^2} + f''(x) \cdot \left(\frac{dx}{dt}\right)^2 \tag{3}$$

and proposes to write the formula

$$d^2y = f'(x) \cdot d^2x + f''(x) \cdot dx^2, \tag{4}$$

as indicating this and this alone, that the equality (3) holds good, whatever be the functional dependence of the variables x and y on the parameter t. Formula (4) may again be used as the operational symbol for obtaining derivatives.

Let us assume, that computations with the differentials did indeed give us the formula

$$d^2y = A \cdot d^2x + B \cdot dx^2 \,;$$

then we can assert that

$$f'(x) = A \,,\ f''(x) = B \,.$$

Indeed, as per our assumptions, we have for any t:

$$\frac{d^2y}{dt^2} = f'(x) \cdot \frac{d^2x}{dt^2} + f''(x) \cdot \left(\frac{dx}{dt}\right)^2 .$$

From a comparison of these equalities, taking into consideration the fact that the derivatives of t can have any value whatsoever, we get what is required to be proved.

But is it necessary to abandon the definition of the differential as the principal linear part of an increment, in order to be able to thus use the folmulae containing the differentials as operational symbols ? This is not a very simple question and it requires to be discussed separately.

3. *The Operational Point Of View and the Differential as the Principal Linear Part of an Increment.*

We have seen, that the definition of the differential as the principal linear part of an increment demands, first of all, that we apportion one of the variables as independent. In reality, however , there are no absolutely independent variables. Even in the process of solving one and the same problem of geometry, mechanics etc., it is often impossible to consider one and the same variable independent, from the beginning to the end . It is clear, that the formulae of differential calculus, when applied to such problems, will really become full-fledged operational formulae, only if it is not required of us, that having once made a choice of an independent variable we should retain it in that capacity for the entire course of the computations; in other words, if the formulae, containing the differentials, and written with the assumption that x is the independent variable, remain unchanged, even when x turns out to be a function of another independent variable. In this sense, the invariance of the formulae, containing the differentials, thus happens to be an *essential condition* for the concordance of the objective and the operational points of view about the definition of the differential.

The definition of the *first* differential, as the principal linear part of an increment , satisfies this essential condition. Let us recall the proof of this well known fact. At issue here is formula (2) : $dy = f'(x) \cdot dx.$

When x is an independent variable, this formula is obtained as under. First of all

$$\Delta y = A \cdot \Delta x + \alpha \cdot \Delta x \,,$$

$$\frac{\Delta y}{\Delta x} = A + \alpha,$$

$$\lim \frac{\Delta y}{\Delta x} = A \qquad \Delta x \to 0,$$
$$f'(x) = A,$$
$$dy = A \cdot \Delta x,$$
$$dy = f'(x) \cdot \Delta x. \tag{2*}$$

Let us note, that in this argument the value of x is fixed, and Δx arbitrarily tends to zero : consequently, Δx is viewed as a variable, *not dependent on* x. This remark will play a vital role in what follows. As of now, let us return to what we have obtained :

$$dy = f'(x) \cdot \Delta x;$$

this apart, since $dx = \Delta x$, formula (2) quickly follows from it.

Now, suppose that x is not an independent variable, and $x = \Phi(t)$, such that it is also the case that $y = \psi(t)$. Then one may get convinced about the validity of formula (2) as under : Owing to (2^*) :

$$dy = \psi'(t) \cdot \Delta t;$$

according to formula (1) :

$$\psi' t \cdot \Delta t = f'(x) \cdot \Phi'(t) \cdot \Delta t$$

and, finally, owing to (2^*) :

$f'(x) \cdot \Phi' \Delta t = f'(x) \cdot dx$ [from (2^*) · we have $\Phi'(t) \cdot \Delta t = dx$] .

By comparing the last three equalities, we get formula (2). Clearly, (2) may serve as an operational formula, quite equivalent to Hadamard's definition.

Difficulties arise , when we try to define the *second* differential. The definition of the second differential is already included in the definition of the differential as the principal linear part of an increment, and there is no room for any additional "arbitrary " understanding . In fact, according to this definition, the differential dy is itself a function of x, and that is why the second differential d^2y, the differential of the differential, is thereby defined as $d(dy)$; it only remains to be computed. For this, let us note, that from the formula (2^*) it follows that

$$d^2y = d\,[f'(x) \cdot \Delta x].$$

Further, as we have already noted, if we do not wish to make all our proofs of invariance of formula (2) — infinite, then we must view Δx as a variable independent of x. Hence, it easily follows, that the factor Δx stands after the differential sign, while differentiating from x, and we get :

$$d^2y = [\,df'(x)\,] \cdot \Delta x,$$

whence, having (2^*) in view :

$$d^2y = f''(x) \cdot \Delta x^2. \tag{5*}$$

We get the final formula for the second differential, by substituting $dx = \Delta x$:

$$d^2y = f''(x) \cdot dx^2. \tag{5}$$

This formula, now found in the assumption, that x is an independent variable, turns out to be *non-invariant.* In fact, let ,

$$x = \Phi(t)\,,\ y = \psi(t)\,,$$

then, owing to (5*)

$$d^2y = \psi''(t) \cdot \Delta t^2\,;$$

according to formula (3)

$$\psi''(t) \cdot \Delta t^2 = f'(x) \cdot \Phi''(t) \cdot \Delta t^2 + f''(x) \cdot [\Phi'(t)]^2 \cdot \Delta t^2$$

and, finally owing to (5*)

$$f'(x) \cdot \Phi''(t) \cdot \Delta t^2 = f'(x) \cdot d^2x,$$

and owing to (2*)

$$f'(x) \cdot [\Phi'(t)]^2 \cdot \Delta t^2 = f''(x) \cdot dx^2\,.$$

Comparing the last four equalities we get :

$$d^2y = f'(x) \cdot d^2x + f''(x) \cdot dx^2\,.$$

This result does not coincide with formula (5).

The conclusion is clear. If we really want the differential calculus to be a full-fledged *calculus*, if we wish to have the right to use its formulae, as we use the algebra, without examining at every step, how they were obtained, then we shall be satisfied with the operational definition of the differential, as its basic definition. The concept of the differential as the principal linear part of an increment , turns out to be only an interpretation, suitable only for definite particular instances. When , in pursuit of an immediate and objective interpretation of each and every symbol, one accepts the principal linear part of an increment as the *definition* of the differential, i.e. when attempt is made to reduce the concept of differential as a whole to it, then one gets a defective result, since, by so doing, one fails to arrive at the differential calculus *as such*.

4. *The Operational Point of View and the Objective Understanding of the Differential in General.*

Our conclusion may be explained only by stating, that, namely, it is the operational understanding of the differential calculus, which reflects the reality correctly and fully [even if, owing to the fact that in reality, as has already been mentioned, there are no absolutely independent variables].

Does this exclude any objective understanding, whatsoever, of the differential symbols ?

Let us put the question more exactly : given a system of differential symbols dx , dy , d^2x, d^2y etc., mutually related — just as the derivatives of any variable t,

$$\frac{dx}{dt}, \frac{dy}{dt}, \frac{d^2x}{dt^2}, \frac{d^2y}{dt^2} \text{ etc. are } —$$

is any interpretation of these symbols, independent of the variable t possible ?

We saw, that the interpretation as the principal linear part of an increment is possible only for the symbols of the first order dx, dy; from this, however, it does not follow that other interpretations, which would be suitable for the symbols of any order, are not possible.

Such interpretations do really exist. For example, geometrical interpretations are possible.

Let us take a curve defined by the equation :

$$y = f(x) .$$

Let us assume, that on this curve

$$x = x(s) ,$$
$$y = y(s) ,$$

where s is the length of a segment of the curve (from a definite point and, it is positve or negative — depending upon the direction chosen). Now the differential symbols dx, dy, d^2x, d^2y may be interpreted as follows. Let us introduce some arbitrary *constants* not equal to zero, indicated through ds and d^2s and put :

$$dx = x'(s) \cdot ds ,$$
$$dy = y'(s) \cdot ds ,$$
$$d^2x = x'(s) \cdot d^2s + x''(s) \cdot ds^2 ,$$
$$d^2y = y'(s) \cdot d^2s + y''(s) \cdot ds^2 .$$

It is easily seen, that this interpretation satisfies the equalities (2) and (4). The length s of the curve-segment, is a property of the curve — not dependent upon its analytical presentation. That is why the said interpretation too is not dependent on it. Naturally, whoever uses the differentials in this or that part of mathematics, bases his notion of the differential upon those interpretations, to which he is accustomed. Thus, interpretations like the one indicated are constantly followed in investigations pertaining to differential geometry, however, these are not created for the differentials themselves of the co-ordinates x and y, but rather for these or those expressions formed with them, expressions — that are of geometric interest.

It is clear however, that the presence of this sort of interpretation does not, in essence, solve the problem of the differential. Marx solved this problem through a dialectical investigation of how the transition from algebra to the differential calculus was accomplished in mathematics. As a result of his investigations there arose the understanding of the differential calculus, as an algebra of its own kind, constructed over the ordinary algebra — which includes the differential symbols, besides the numbers. The definition of the differential provided in Hadamard's text book shows that mathematicians are also arriving at that understanding of the general character of the differential calculus, where dialectics had arrived in the hands of a materialist philosopher — some half a century ago.

Source : *Pod Znamenem Marksizma*, 1934, 5, str. 79- 85.

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Other Publications :

1. Sur la logique de M. Brouwer // *Bull. Acad. Sci. de Belgique* (5), 14(1928).
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3. Osnovy Obshiei Teorii Struktur. 1937.

MARX'S "MATHEMATICAL MANUSCRIPTS" AND THE DEVELOPMENT OF HISTORY OF MATHEMATICS IN THE USSR

VLADIMIR NIKOLAVICH MOLODSHII

The "Mathematical Manuscripts" of Karl Marx were published in 1968 [1], in connection with the 150-th anniversary of his birth. "All those manuscripts of Marx, which were more or less complete, or those which contained his own comments on this or that mathematical question", were included in this publication — in the original language and in Russian translation [1, s. 3]. A part of Marx's mathematical manuscripts, containing the results of his reflections on the nature of differential calculus, were published in 1933, in Russian translation [2].

For the Soviet historians of mathematics, the mathematical manuscripts of K. Marx, were important supplements to the fundamental works of the classics of Marxism-Leninism, upon which they constantly based their investigations. Here I have in view : Marx's "Capital", Engels' "Anti- Dühring" and "Dialectics of Nature" and, Lenin's "Materialism and Empirio-Criticism" and "Philosophical Notebooks". Marx's "Mathematical Manuscripts" helped the Soviet scholars to better orient themselves on philosophico-methodological questions — questions, that are important for the history of mathematics, which to some extent determined the concrete themes of their investigations, especially on the history of mathematical analysis and of its substantiation in the 17th-19th centuries.

Marx undertook a deeper study of mathematics in connection with his economic investigations [1, s. 4-6]. His mathematical manuscripts show, that subsequently he became interested in purely mathematical problems — in questions pertaining to the problem of substantiation of the differential calculus, and in its history. Marx noticed the deficiencies of the basic conceptions of differential calculus of the end of the 17th-beginning of the 19th centuries and, he began to elaborate his own conception of "algebraic differentiation" and of the philosophico-methodological and historical questions intimatley connected with it [1, s. 6-22]. Marx's conception is essentially different from that of Lagrange.

Marx treated the differential of a function as an operational symbol. In this connection he investigated questions related to the nature of mathematical abstractions and to its symbols, pertaining to making the definitions of the variable and the function more exact and, questions related to the mathematical means of describing movement. Marx discussed the question of regularity of the developments of mathematical conceptions in a "historical essay", in the light of the course of development of the differential calculus and the results of the attempts to substantiate it in the 17th-18th centuries.

The question of the nature of mathematical abstractions plays no small role even in the elaboration of the problems of the foundations of modern mathematics and of mathematical logic. It is enough to recall the struggle between the supporters and opponents of the concept of actual infinity. The dialectico-materialist elaboration of the problem of formation and of types of abstractions constantly drew the attention of some philosophers and mathematicians of our country — like S.A. Yanovskaya [3]. Facts from the history of calculi, axiomatic method and questions of mathematical logic were analysed in their investigations in a new light [4]. Comparisons with Marx's "Capital" were also undertaken. Marx himself had, on more than one occasion, indicated that the logic of mathematics and the logic of "Capital" resemble each other [5] ; and this was not a chance remark. Yanovskaya used the results obtained in this field, while elaborating the question of the epistemological foundations of the criterion of truth in mathematics (the practice of non-contradictority) [6] and, those of the concept of mathematical rigor [7]. The creative output of S.A. Yanovskaya include the papers entitled : "On the so-called Definitions through Abstractions" (1935) and "The Problem of Introducing and Excluding the Asbtractions of Orders Higher than One" (1965) [7].

Modern investigators — especially philosophers and logicians — are drawn towards the question of operational strength of mathematical symbols. V.I. Glivenko was the first to offer an extended and purely mathematical evaluation of Marx's treatment of the signs dx and dy as operational symbols [8.1], (somewhat later, M. Fréchet too expressed analogous ideas) [8.2]. Marx's ideas about the operational strength of these symbols are true even in respect of the wider range of materials provided by modern mathematics. As soon as the discussion turns to the scientific interpretations of the reasons behind the emergence and development of effective mathematical conceptions and methods — especially those of the 19th and 20th centuries, the aforementioned fact turns out to be important for history of mathematics.

Before the publication of Marx's mathematical manuscripts, historians of mathematics studied the ideas of the classics of Marxism-Leninism, on the inner-regularities of the development of the sceinces, from the other sources available. But in Marx's "historical essay" on the development of the foundations of differential calculus, they came across Marx's own analysis of the inner regularities of the development of mathematics.

Marx showed that when the necessary conditions are created within the existing mathematical theories, then a new mathematical theory may arise and develop. This new theory "stands on its own legs", when its basic concepts and methods assume the specificities characteristic of it alone; the embryonic forms of these concepts contained in the initial mathematical theories, do not have these specificities. Marx stressed , that a new theory is not perfected and does not get recognition at once ; that happens only through the struggle between its adherents and the followers of the old ideas.

Having compared three conceptions — those of Newton, d'Alembert and Lagrange — Marx observed, that in the period under consideration, the elaboration of the means of substantiating the differential calculus proceeded along the lines of perfecting and making things more exact.

The first investigations inspired by these ideas include : S.A. Yanovskaya's "Misheil Roll kak kritik analiza beskonechno malaykh" ("Michel Rolle as a critic of infinitesimal analysis") [first published in 1947, reprinted in : 3, s. 76-106]; and K.A. Rybnikov's "Ob algebraicheskikh korniyakh differensialnovo ischeslieniya" ("On the algebraic roots of the differential calculus") [9], and "O roli algorifmov v istorii obosnovaniya matematicheskovo analiza " (" On the role of algorithms in the history of substantiation of mathematical analysis") [10].

Subsequently, not only the historians of mathematics, but also mathematicians and philosophers, began to take an interest in the question of inner regularities of the development of mathematics. Elaboration of this question was found to be essential for analysing the nature and mechanism of the scientific revolutions in mathematics.

Marx's "historical essay" helped to reveal the philosophico-methodological foundations of the mistakes and insufficiently substantiated conclusions of some of the leading mathematicians of the centuries gone by. Yanovskaya discussed the foundations of the mistakes of Saccheri's proof of the parallel lines axiom in : [11]. Rybnikov revealed the inexactitude in the arguments of I. Bernoulli and Ya. Bernoulli, in their analysis of one of the questions of variational analysis [12]. E. Ya. Bakahmutskaya compared the critical remarks of K. Marx and T.P. Osipovsky on Lagrarnge's conception of algebraic differentiation [13].

A study of the text-books of mathematics, published mainly in the 17th-18th centuries showed, that the process of making them more perfect at times gave rise to ideas, that went out of the framework of the then prevalent scientific ideas, but subsequently they became components of new conceptions together with a new methodology [14].

A.P. Yuskhevich noted [15], and then S.A. Yanovskaya and N.I. Likholetov showed [16], that from 1804 to 1834, the teaching of differential calculus in Moscow University went through three successive stages, reproducing its "mystical", "rationalistic" and "purely algebraic" forms respectively. Ideas of Cauchy replaced them by the middle of the 19th century. Roughly the same can be said about the teaching of mathematical analysis in the other universities and institutions of higher learning of Russia, during the first half of the 19th century [17 and 18].

Investigations of the Soviet historians of mathematics confirmed the correctness of Marx's critique of the conceptions of differential calculus held by Newton-Leibnitz, d'Alemert and Lagrange, as well as of his position, that nevertheless, the elaboration of the questions of substantiation of differential calculus, as per these conceptions, went along a line of ascent (and that is why it produced concrete results). [The Leibnitz -Newton apparatus of the differential calculus, together with its "principle of getting rid of " the infinitesimals of higher orders, has been provided with a new scientific substantiation in the non-standard mathematical analysis (see : "Matematicheskaya Entsiklopedia", T. III, M., 1982, s. 1019-1020). However, this does not reduce the merit of Marx's critique, since the Leibnitz-Newton apparatus has been based upon essentially different presuppositions in the theories of its inventors on the one hand, and in the non-standard analysis on the other]. Marx's "historical essay" served as a starting point for those sections of some of the text books of history of mathematics, which contained a discussion of the development of mathematical analysis [19]. The second edition of the Great Soviet Encyclopaedia [20] and the "Filosofskaya Entsiklopedia" [21] contain a description of the contents and of the methodological significance of the mathematical manuscripts of K. Marx. Articles were devoted to these manuscripts in many journals, in pariticular in the "Uspekhi Matematicheskikh Nauk" [22], "Pod Znamenem marksizma" [23], "Voprosy Filosofii" [24] and in "Matematika v Shkole" [25]. "Istoria Otchestvennoi Matematiki" [26] contains a brief description of the role of Marx's "Mathematical Manuscripts" in the development of history of mathematics in the USSR, during the last fifty years.

The questions about the stimuli, regularity and forms of scientific revolutions in the mathematics of 19th-20th centuries have principled significance for a dialectical materialist elaboration of the history of mathematics of the same period. Fruitful investigations of these questions are inseparable from the analysis of the law-governed development of the mathematical conceptions and theories as a single whole together with their basic concepts, principles, methods of proof and norms of mathematical rigor. Namely thus did Marx pose and investigate the question of development of the means of stubstantiating the differential calculus from the period of Newton and Leibnitz to that of Lagrange. When the problem of scientific revolutions will gain its proper place in the investigations of our historians of mathematics, then they will be convinced about the fact that Marx's ideas about the regularities of development of mathematics, about the nature of mathematical abstractions and operational symbols,and about the struggle between the new and the old, are capable of helping them more than they did earlier. The present author became convinced about this while investigating the scienitific revolutions in the theory of numbers of the 18th-beginning of the 19th centuries, and in the mathematical analysis and geometry of the first half of the 19th century. Other authors have also recognised and correspondingly used this fact in their investigations [27].

The ideas expressed by V.I.Lenin in his "Materialism and Empirio-Criticism" and in his "Philosophical Notebooks" are also important for those historians of mathematics, who are engaged in the analysis of the scientific revolutions in the mathematics — especially, of the period beginning with the end of the 19th centutry.

The last — 50th — volume of the second edition of the collected works of Marx and Engels has been published recently. This edition includes 39 main and 11 supplementary volumes. This second edition contains nearly 800 new works and 700 letters more than those in the first [see : Sokrovishnitsa revoltsionnoi mysli (k zavcrsheniu 2-ovo izdaniya sochinenii K. Marksa i F. Engelsa), *Pravda*, 1983, 28 January]. In the 11 supplementary volumes, the historians of mathematics will find some new statements of Marx and Engels related to mathematics, to its role in the elaboration of the questions of the social sciences. This is of great significance for the study of the history of mathematization of knowledge in the 19th century.

The entirety of Marx's manuscripts pertaining to the philosophico-methodological questions of mathematics and of its history, has not yet been published. These will be included in the multi-volume academic edition of the works of Marx and Engels, being prepared by the Institute of Marxism-Leninism of the CC of CPSU together with the Institute of Marxism-Leninism of the CC of SUPG [28]. However, a preliminary description of a part of this heritage has been published [29] ; this publication may be used with profit.

Marx read (during 1878-79) Du Bois-Reymond's "Leibnitzian Ideas in Modern Natural Sciences" [30] and noted his statement to the effect that: ".... Aristotle's and Locke's view, that the soul is a *tabula rasa,* is supported by the investigations of Reimann, Helmholtz and others about the axioms of geometry" [29, s. 87]. It would be interesting to reproduce the rest of Marx's conspectus of this speech of Du Bois-Reymond and to compare it with the statements of Lobachevsky, Bolyai, Reimann and Helmholtz about the nature of the presuppositions in geometry. A comparison of these notes of Marx with V.I. Lenin's conspectus of A.Rey's "Modern Philosophy" may also be of use. The following statement of A.Rey, noted by Lenin, in fact develops the aforementioned statement of Du Bois-Reymond, noted by Marx : "By constantly moving further from the space accessible to sense perception and by moving up to the geometrical space, mathematics, however, does not move away from real space, i.e. from the *true relations among things. But rather, comes closer to them*" [31]. At issue here are the mathematical abstractions characteristic of the mathematics of the second half of 19th century and, how the naturalists approached the dialectico-materialist interpretation of their nature. This is an important issue for those who are investigating the history of mathematics of the 19th-20th centuries.

Marx also took notes form the works of Leibnitz and Descartes : from what they wrote about motion, and from the Leibnitz-Clarke correspondence [32], and from some posthumous publication of Descartes [33]. An analysis of these notes would certainly help the historians of mathematics to understand the "Mathematical Manuscripts" of Marx better.

One should not forget, that as of now, the scientific writings, especially those remaining in the shape of manuscripts of Marx and Engels have not been collected in full. One cannot exclude the possibility of discovery of such new materials as may be of interest to the historians of mathematics [see : 28 and 29].

Mathematicians and historians of mathematics of other countries have also taken note of Marx's mathematical manuscripts. Noteworthy in this connection is an essay of D. J. Struik [34], where he has compared Marx's conception of "algebraic differntiation" with the conceptions of Cauchy and his successors. Svyatoslav Slavkov's monograph on Marx's mathematical manuscripts [35] was published in 1963. The German and Italian editions of [the first part of] Marx's "Mathematical Manuscripts"(1968), were published in 1974 and 1975 respectively. H.C. Kennedy read his paper on Marx's mathematical manuscripts in the 15th International Mathematics Congress[36]. Soviet scholars should analyse these materials and ascertain the nature of the influence exerted by the works of Marx upon the development of philosophico-historico-mathematical investigations in the world.

[This is a re-written and updated version of the paper read by the present author at the Second School of History of Mathematics, Liepai, 3-10, VII, 1978. The first version of this paper was published in : *Istoriko-Matematicheskie Issledovania*, Vyp. XXVI, s. 9-17.]

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ON THE OPERATIONAL LOGICAL APPARATUS OPERATIVE IN KARL MARX'S "CAPITAL" AND "MATHEMATICAL MANUSCRIPTS"

V. I. PRZHESMITSKY

I. *On the logic of Marx's "Capital", the logical apparatus of the concrete sciences and, the actual, real contradictions.*

The question of the operational logical apparatus operative in Marx's "Capital" is topical, not only in connection with the necessity of defending Marxism and the foundations of its philosophy, but also in connection with the requirements of the concrete sciences, in particular, those of physics and mathematics. In these sciences, specific logical problems are arising continuously; these problems demand that we turn to a logic, which is more powerful than that ordinary, traditional logic, which has become "mathematical".

Following Marx and Engels we shall use the term "Logic" to indicate the science, wherein the "laws of human thought" are investigated or, which is "a discipline about the laws of the very process of thought " [Marx K., Engels F., *Collected Works*, Russian Edition T. 20, s. 91; T. 21. s. 316] (Eng. ed., v. 25, p. 84 ; Marx-Engels, On Religion (in Bengali), Progress, M., 1981 p. 258 — Tr.). This discipline enters into all the fields of knowledge in the same way, as the method of MOVEMENT of thought from the problems to be sloved to their true solutions, as "the method for seeking new results, for the transition from the known to the unknown", as "the method of investigation and of thinking" [ibid, T. 20. s. 138; T. 21, s. 303] (Eng. ed., v. 25, p. 125 ; Marx-Engels, On Religion (in Bengali), 1981, p. 288 — Tr.). While studying the works of Marx and Engels, one cannot fail to notice, that already in the 1840s, they were using a qualitatively new (dialectical) logic which they themselves developed. After the publication of Marx's "Contribution to the Critique of Political Economy", in 1858 — they openly declared that a dialectical logic has been created, and that it is successfully operative in science. In this connection, they noted, that the scientific result obtained by them in logic is, " in its significance, hardly inferior to the fundamental materialist ideas" [ibid, T. 13, s. 497] (Eng. ed., v. 16, p. 475 — Tr.). This result was obtained on the basis of the principles of philosophical materialism : "to comprehend the specific logic of a specific subject-matter" [ibid, T. 1, s. 325]," ... an understanding of nature as she is, without any extraneous additions..." [ibid, T. 20. s. 513] (Eng. ed., v. 25, pp. 478-479 — Tr.), "one must not introduce any arbitrary sub-divisions" into the subject-matter under investigation, and the logical aspect of the subject-matter must "find its unity in itself" [ibid, T. 40. s. 10] (Eng. ed., v. 1, p. 12 — Tr.).

Marx and Engels began their work in the field of logic, while they were still young. Already in 1837, Marx wrote confidentially to his father : "... this work ... which had caused me to rack my brains endlessl, (since it was actually intended to be a new logic) ... [it is] my dearst child" [ibid, T. 40. s. 15.] (Eng. ed. v. 1, p. 18 — Tr.).

This statement indicates the indisputable beginning of the marxist [? — Tr.] dialectical revolution in logic. The new logic may be derived, with help of strict logical means, from its primary element, its primary foundation, its initial "cell". Such an element must fixate the total overcoming of that one-sidedness of scientific thought — by logic — which emanates from the Aristotelian principle of non-antinomicity of truth. A study of Marx's and Engels' works show, that their purely logical demand is as under : to do away with the metaphysical anti-dialectical prejudice, the globalization of the priciple of non-antinomicity and TO USE "IN THE PROPER CASES BOTH" THE DISJUNCTION ($p \dot{\vee} \overline{p}$) AND THE CONJUNCTION (p & $\overline{p}$), AS PERMISSIBLE LOGICAL FORMS OF TRUTH, understandably, basing oneself upon the possible "use of the opposites" [ibid, T. 20, s. 528] (Eng. ed., v. 25, p. 493 — Tr.), where it is necessary (our stress — V.P.).

Such an attitude to the conjunction (p & $\overline{p}$) as a possible form of truth, fixated/ consolidated in the Principles of logic as a science, is strictly scientifically substantiated. This (substantiation) consists of the two following facts, related to the DIALECTICS OF NATURE : (I) a contradiction in a scientific theory — that, which in the final count takes the form of the conjunction (p & $\overline{p}$), (has) by no means (its origin only in thought) in any and every case; depending upon the concrete conditions, the concrete contents of thought, the foundation and the root of a concrete contradiction lies : either really in thought, or — in the perception of reality and only then in thought, or — in the very essence of the subject-matter of thought, i.e. exclusively in the very nature of things; (II) the set of three basic types of contradictions are divided in scientific theories, into concretely defined sub-sets or classes of fully defined specificities, these are strictly differentiated in the writings of Marx and Engels.

According to Marx and Engels, the first two types of contradictions constitute the class of "trivial" and, "false" or "seeming" contradictions and, they are to be contrasted with the remaining class of "REAL or TRUE" contradictions.

Through their investigations in the "Capital" Marx and Engels made it possible for us to understand the importance of the distinction between the real contradictions characteristic of reality and the contradictions of the sophist type, of that between the REAL contradictions and the really TRIVIAL (ILLUSORY, SEEMING) contradictions, those that give rise to the PARADOXES. It must be pointed out however that ordinary logic lacks the means required for visualising and expressing these distinctions, in so far as these distinctions are not immediately given by the forms of expressions, but rather by the contents thereby reflected. Since the logical form of all types of contradictions happen to be the one and same antinomy — the conjunction (p & $\overline{p}$), the determination of whether this or that real concrete contradiction is genuine and true or an illusion or a paradox is impossible with the help of ordinary logic. For this logic even the task of distinguishing between a REAL contradiction from a sophistic-contradiction, becomes a BACK-BREAKING task in a number of instances.

Now let us take a look at the *real* contradictions of scientific theories.

We may take the following contradiction, as an example of the really false or seeming contradiction, that of the illusory contradiction or paradox, from amidst the writings of Marx and Engels : "why does the capitalist, whose sole concern is the production of exchange value, continually strive to depress the exchange value of commodities ? A riddle with which Quesnay, one of the founders of Political Economy, tormented his opponents, and to which they could give him no answer" [ibid, T. 23, s, 331; *Capital* (Eng. ed.), I, p. 303]. The following example of a real, *genuine,* i.e. true contradiction may also be considered from among the writings of Marx and Engels : "Motion itself is a contradiction — even simple mechanical change of position can only come about through a body being at one and the same moment of time both in one place and in another place, being in one and the same place and also not in it. And the continuous origination and simultaneous solution of this contradiction is precisely what motion is" [ibid, T. 20. s 123 ; Eng. ed., v. 25. p.111]. We may consider, alternatively, yet another example :"... it is a contradiction to depict one body as constantly falling towards another, and as, at the same time, constantly flying away from it. The ellipse is a form of motion which, while allowing this contradiction to go on, at the same time reconciles it" [ibid, T. 23, s 114; *Capital* (Eng. ed.), I, p. 106]

Remarkably, all these types and classes of contradictions, which we meet within the scientific theories, have been — one and all — investigated in the writings of Marx and Engels. The task of recognising and revealing the specificities of these contradictions has been made an ordinary, routine and everyday affair, like that of solving the riddles or revealing the secrets of nature or those of the objects of thought, which have often been termed — after Hegel — as the problems of "solving" (logical) contradictions. Such solutions are carried out by seeking out the essential, natural, intermediate, logically-mediating links. Incidentally, often many such intermediate links are required to be sought, as, for instance, from the standpoint of elementary algebra many intermediate terms are required, to understand that $\frac{0}{0}$ may represent an actual magnitude [ibid, T. 23, s. 326; *Capital* (Eng. ed.), I, p. 290].

The works of Marx and Engels also help us to understand the fact that the difference of the actual real contradictions from the really seeming and false ones — the paradoxes — which are BASIC to logic (and not to any theory of development whatsoever), leads to the following.

1) The real and actual contradictions in theory are antinomical-truths, as truths they may be logically used in appropriate situations either as natural elements of true scientific thought or as logical TRANSITIONS encountered in logical movement of thought — which the developed sciences cannot do without, whereas just like the sophistic-contradictions, the paradox type contradictions cannot play that role.

2) The real and actual contradiction characteristic of an object of thought, is contained in the very ESSENCE of the object or it may even be THAT VERY OBJECT (as in particular, is the contradiction of MOTION), while the really paradox producing contradiction is merely a contradiction between the outer side (and correspondingly, the external appearence) of the object of thought and its ESSENCE (and correspondingly, its inner regularity).

3) From the standpoint of ordinary logic, the real parodox generating contradictions, just like any trivial contradiction — cease to EXIST upon their resolution and, permit non-antinomical representation of the phenomenon indicated by them. In contrast to this, no actual, real contradiction (like the contradiction of MOTION) can be depicted non-antinomically and in this sense they are INDESTRUCTIBLE.

The actual, real contradictions do not vanish even in those situations, when together with their carriers — as is characterisitic of them, they are DEVELOPED — i.e., transformed, unfolded and, are supplemented with new phenomena, with new contradictions derived out of the old ones. Since the development of an object DOES NOT ELIMINATE these real contradictions but only creates a *modus vivendi,* i. e. a form in which they can move forward side by side, a form of their movement. And such in general is the method — explained Marx — with the help of which real contradictions are resolved [see : *Capital* (Eng. ed.), I, p. 106]:

All that has been said above oblige us, following Marx and Engels, to see and to take note of the fact, in all the disciplines (and especially in logic and in philosophy as a whole), that the aninomies and the symbolic conjunctions which express them, may depict, not only the false and trivial contradictions, absurdities and paradoxes, but also the real, actual contradictions. And in that case such a conjunction can be the TRUE form of a TRUE antinomical proposition, VITALLY IMPORTANT FOR THE SCIENCE of logical transition along the path of dialectical negation together with a subsequent negation of the negation and, Marx has graphically demonstrated the justifiability and result-yielding character of such conjunctions in mathematics [see : Marks K., *Matematicheskie Rukopisi,* M., 1968, s. 29-75]. It must be clearly stated here, that now, when the works of Marx, Engels and Lenin are to a significant extent available to all, and are within the reach of every Marxist, we should not fail to notice this fact.

Here it may be indicated as a positive fact, that mathematicians in the USSR have already noted, that the formal-logical type of thinking and the very principle of non-antinomicity may, under certain circumstances lead even " to the appearance of delusions and mistakes "[see : Rybnikov K. A., *Vvedenie v metodologiyu matematiki* (Introduction to the methodology of mathematics), M., 1979, s. 50]. And that is why, this type of thinking "did not and does not occupy a central position ... in developed human consciousness. The acquisition of mathematical knowledge, and its composition, includes within itself a lot of elements that are not amenable to formal-logical analysis. Quite a few of the methods of mathematical "operation" happen to be "non-logical" [Rybnikov, *op. cit.*, s., 56-57]. And in so far as even to-day, we are required to use a "means of logical deduction which arose in the past" [*ibid,* s. 83-84], including the principle of non-contradiction as the principle of non-antionomicity, "notwithstanding the evident logical discrepancies and lack of explanations of the operational part of analysis", when "the problem of construction of models and their corresponding definite logical requirements have come to occupy the fore-court "[*ibid*, s. 94]. Here the development of mathematics (as well as that of theoretical mechanics and physics) is in ever greater need of a recognition of that DIALECTICS OF NATURE in logic, about the non-naturalness of which N.N. Luzin, D.D.Morduhai-Boltovsky, P.S. Novikov and others have spoken repeatedly.

II. *The operational logical apparatus of the "Capital" and of the "Mathematical Manuscripts" of K. Marx.*

The operational logical apparatus adduced in the courses of traditional logic, called forth to ensure the logical movement of scientific thought, does not guarantee the banning of false identifications, antinomies or disjunctions and, even of logical arbitrariness in scientific thought. The logic of the "Captial" of Marx not only guarantees the banning of logical arbitrariness in scientific thought, but also ensures its successful development : reproduction of the essence of the object under investigation in all its contradictoriness, consistency of thought, its truth, broadness of the logical frame and of the genralisations adduced, the necessary strictness and, together with it, versatility of the conclusions. It is that is why, namely, that the logic of "Capital" is at least as superior to the ordinary logic "as the railways are to the medieval means of transport", to borrow an expression from Engels [Marx K., Engels F., *Collected Works*, Russian Edition, T. 13. s. 48; Eng. ed. v. 16, p. 476].

The following logical devices have been employed in the "Capital" for ensuring the reproduction of the essence of the object of investigation in the movement of scientific thought: contradictions connected with the solution of the problems under investigation have been revealed and formulated — these contradictions reflect the essence of the problems to be solved. Such a logical order is clearly visible in Marx's formulation of the question of essence of commodity, of the capitalist form of social production and, that of the distribution of capitalist profit and capital [ibid, T. 13, s, 47-48; Eng. ed., v. 16, pp. 475-476].

Further, the contradictions, which have been formulated as fragments of the reality under investigation, and which appear as riddles or as secrets of the object of thought, are solved purely logically, i.e., in thought. This does not change anything in their content, yet removes the veil of secrecy from their face, removes the veil of mystery. In so far as the realisation of such an operation, while resolving the real, and even seeming, contradictions, i.e., paradoxes, demands that not only the external aspect, the appearance of phenomena, but also their internal side, the regularity, and that is also the essence of the OBJECT OF THOUGHT be taken into account ; here thought, while realising the logical operations, moves from the formulation of the questions to their true solutions, without tearing itself off, either from the essence of the object of thought or, from the essence of the scientific problems to be solved.

Truth is the most valuable property of thought and, that is why, it is first of all the duty of LOGIC to control the truth of scientific thought. Since, namely, "LOGIC = THE QUESTION OF TRUTH" [Lenin V. I., *Collected Works,* Eng . ed. v. 38, p. 175]. While performing this duty, the logic of Marx's "Capital" — as distinct from ordinary logic — proceeds from the fact that concrete truths happen to be both non-antinomical and antinomical. That is why, as already stated above, the new logic, the logic of "Capital" — as distinct from the Aristotelian and modern formal logic — proceeds from the demand that BESIDES "EITHER-OR", "BOTH THIS — AND THAT" is also TO BE EMPLOYED in the RIGHT PLACE. At a definite stage, this demand wards off that situation, when actually true propositions are declared to be false on the basis of the fact that the positions opposed to them, formulated earlier, turn out to be true — and this is not a sufficient ground for declaring the earlier ones false. Here we are, in the main, speaking of the ready-made propositions and concepts. This is what I would like to point out at first.

Marx has also pointed out : "Truth includes not only the result but also the path to it. The investigation of truth must itself be true; true investigation is developed truth, the dispersed elements of which are brought together in the result" [Marx K., and Engels F., *Collected Works*, Eng. ed., Vol. 1, p.113.]. That is to say, we must take care both of the consistency and of the starting points of the movement of scientific thought. The logic of "Capital" takes this into account. From the very beginning it directs the subject of investigation along a path which rejects all mysticism and scholasticism and, ensures "THE JOURNEY TO THE THINGS, AS THEY ARE, I. E. UPTO TRUTH" [Marx K., and Engels F., *Collected Works*, Russ. ed., T. 1, s. 29].

In view of the fact that the logic of "Capital" and of the "Two manuscripts on the differential calculus" of K. Marx permits in the right place, a recourse to truth equally with structural disjunction as well as with conjunction — the actualisation of the formal-logical kind of *consistency of the movement of scientific thought* becomes impossible. That is why, in this logic, the movement of scientific thought is actualised as a movement from the statement of the question to be answered, to a true answer of it. Here the very process of movement of thought is actualised in the form of a consistent unity of two — in a certain sense contradictory to the each other — dialectical-logical processes, appropriately termed as : the ascent from the concrete to the abstract and, the ascent from the abstract to the concrete.

The ascent from the concerte to the abstract leads to an unification and organisation of the scientific data, from there one may proceed to a true solution of the problem under consideration. These data are embodied in the necessary premises of the ascent to the concrete strictly-scientific abstractions, which are called the initial abstractions for the ascent from the abstract to the concrete. Initial abstractions, either as a kind of INITIAL "CELL" of the object of thought or as a kind of true answer of the question under consideration, or as a kind of "LOGICAL BRIDGE" over the problems to be solved, turn out to be suitable for such an ascent. We shall discuss these two types of initial abstractions and ascents in detail in another paper.

The ascent from the abstract to the concrete is an intellectually realised theoretical transition according to the corresponding laws of reality (these laws are logical and special-scientific); it is a transition from the already prepared strictly-scientific initial abstractions corresponding to the concrete scientific content, to the concrete knowledge sought — to the true answer of the question under consideration.

In order to ensure that the unity of the initial abstractions of the ascent, with the laws of the reality connected with them, was natural and, that they be dependable theoretical Elements and be the logical path of ascent to the true answer of the question under consideration — logic must take into account not only the external aspect, the appearance, but also the essence of the object or phenomenon considered. That is why dialectical logic must seek or elaborate the neccessary concrete-scientific abstractions or laws of reality, initial to the ascents, only in indissoluble unity with the corresponding concrete sciences — and not otherwise. That is why in the "Capital" — where the object of investigation pertains to economics and history — this logic functions in an unity with political economy and history of mankind. In the "Two manuscripts on the differential calculus" — where the object of investigation, the differential, is a mathematical object — this logic functions in an unity with mathematics. It is understandable that this unity of the dialectical logic with a corresponding concrete science must take place only there and to that extent, where and to which extent it happens to be necessary.

The dialectical logic of Marx and Engels attains a broadness of logical frame and scientific generalisation, since it aims at using the unity of opposites: of the individual (the particular and the singular) with the general, of the concrete — with the abstract, of the real — with the possible, and of the determinate — with the indeterminate [Aristotle underrated the latter, but the indeterminate should not be underestimated] (see : Lenin V. I., *Collected Works,* Eng. ed., vol. 38, pp. 359-360). That is why, while rising from the concrete to the abstract, Marx reduces the general contained in the things to their most generalised logical expression [Marx K., and Engels F., *Collected Works*, Russ. ed., T. 3., s 180]. On this road one is required to raise scientific thought above the level of ordinary logic, i.e. above the level of the logical connections of identity and difference, of affirmation and negation, above the level of the Aristotelian laws of excluded middle and non-contradiction and, correspondingly of the formulae :

$\urcorner(p \& \bar{p})$ and $(p \dot{\vee} \bar{p})$,

from among which neither one is for ever-true, nor for ever-false (but rather, both are only sometimes-true and sometimes-false), and thus rise to the level of connecting the opposites: AFFIRMATION and NEGATION, to express which one uses $(p \dot{\vee} \bar{p})$ together with $(p \& \bar{p})$.

This required representation of this law of unity of the opposites AFFIRMATION and NEGATION is superficially expressed more fully and deeply within the framework of its abstract universality, in the language of modern symbolic logic, by the formula :

$$\forall p\{[\urcorner p \& \urcorner \bar{p}] \dot{\vee} [p \& \urcorner \bar{p}] \dot{\vee} [\urcorner \bar{p} \& p] \dot{\vee} [p \& \bar{p}] \dot{\vee} [(p \& \bar{p}) \& (\urcorner p \& \urcorner \bar{p})]\}.$$

In logic we shall call this formula and the concrete law which it expresses, Engels' logical law and formula of disjunction-conjunction without forgetting the fact that this is only one of the possible PARTICULAR expressions of the ESSENCE and "NUCLEUS" of DIALECTICS, of the law of unity of opposites.

In view of the universality of this law and of the formula that expresses it, while solving a concrete problem — the object of thought, characterized by a certain predicate in p and the predicate itself, is chosen in an extremely generalised form. That is to say, always only an individual indeterminate example is chosen, from amongst the set of all the examples of a given genus, as the object of thought, which potentially carries any form inherent to their identity and difference. This holds good also for the predicates which figure in thought as per necessity. Thus scientific thought protects itself from being suppressed PREMATURELY within the limits of the Aristotelian frame-work of such conditions as, the "one and the same object", at "one and the same time", in "one and the same relation" etc.; this provides the possibility of retaining, in the movement of thought from the statement of the question to its true answer, all its possible potentiality, which can not be immediately realised and utilised in full.

The logical means that regulated, as an instrument and as a method, the strictness and the versatility of scientific thought in the "Capital" and in the other writings of Marx and Engels, has the following poles.

When thought moves from a question to its answer, those boundaries are not to be lost sight of, within the limits of which, Engels' law and formula

$$S=\{[p \& \urcorner \bar{p}] \dot{\vee} [\urcorner p \& \bar{p}]\}$$

is adequate for the object of thought. Within these boundaries they (this law and formula) demand extreme concreteness from scientific thought (and thereby this thought becomes extremely strict). But this formula may be used only within the limits of its actual range of applicability (and herein the dialectical logic of Marx and Engels retains within itself all that is really valuable and true in the ordinary logic). The movemnet of thought from the statement of a question to its true answer — according to the law that connects the opposites within the frame-work of a necessary abstract universality, which is regulated within the frame-work of Engels' law and formula of disjunction :

$$S=\{[p \& \urcorner \bar{p}] \dot{\vee} [\urcorner p \& \bar{p}] \dot{\vee} [\urcorner p \& \urcorner \bar{p}] \dot{\vee} [p \& \bar{p}] \dot{\vee} [(p \& \bar{p}) \& (\urcorner p \& \urcorner \bar{p})]\}$$

by way of excluding from it every time those terms, which lose their meaning as per the given conditions and upon solution of a given problem — alone can ensure the extreme strictness and fluidity of thought.

Source : *Voprosy dialekticheskoi logiki : printsipy i formy myshleniya* (materialy postoyanno deistvuyuschevo simpoziuma po dialekticheskoi logike). AN SSSR. Institut Filosofii, M., 1985. s. 70-81.

ON THE PROBLEM OF SITUATING MARX'S MATHEMATICAL MANUSCRIPTS IN THE HISTORY OF IDEAS

PRADIP BAKSI

I

Karl Marx completed his school education in 1835, with "a good knowledge of mathematics" [20, 644], which included arithmetic, alegebra, geometry, trigonometry and infinitesimal calculus [25, 157 ff]. However, he did not study mathematics in any university department. In the universities of Bonn and Berlin, he attended lectures on law, Greek and Roman mythology, Homer, history of modern art, anthropology, logic, geography, Isaiah and Euripides [20, 657-658 and 703-704]. While attempting an elaboration of a philosophy of law of his own, as a 19 year old student of Berlin University, he expressed his dissatisfaction ,on a methodological plane, with "the unscientific form of mathematical dogmatism" [20, 12]. In the same year he composed three poems in jest, and gave them the common title : *Mathematical Wisdom* [20, 545-546] . Two years later, in 1839, he drafted a *Plan of Hegel's Philosophy of Nature* [20, 510-514], three versions of which have come down to us ; they contain references to mechanics. His *Note books on Epicurean Philosophy* [20, 403-509], dating back to the same year and, his doctoral dissertation, written during 1840 and 1841 and submitted to the University of Jena in April 1841 : *On the Difference Between the Democritean and Epicurean Philosophy of Nature* [20, 25-105], contain evidences of his continuing philosophical interest in the fundamental physico-mathematical concepts. Thus, in spite of a lack of formal mathematical education at the university-level, mathematics was always present in Karl Marx's intellectual horizon, in some form or the other, even during his formative years. In the latter half of the 1840's Marx's interest in mathematics was rekindled by the requirements of his investigations in the field of political economy. But within a few years this interest began to draw sustenance from other sources too : for instance, in July 1850 we find him discussing the then emerging materialist conception of nature and human history in the light of the developments in mechanics and in the other sciences [19, 67-69] ; in April 1851 his friend Roland Daniels was imploring him to take up the study of physics in conenction with the projected preparation of an encyclopaedia of the sciences [8, 113]; and in September-October that year Marx did study a treatise on the history of mathematics and mechanics [PV, 109-112]. After that he went through the different branches of elementary mathematics all over again and, made a special sutdy of ordinary algebra and differential calculus. These studies continued for the next thirty odd years and, came to an end only with his death.

In "... mathematics Marx found the most consistent and at once the most simple expression of dialectical movements" [19, 31-32]. He was drawn to mathematics owing to the " many points of contact between mathematics, philosophy and dialectical logic" [16, 587]. In his more or less complete mathematical manuscripts he investigated the dialectic, the being and the becoming, the nature and history, of the fundamental concepts of differential calculus. That is why only in the context of the history of interaction of mathematical and philosophical thought can we hope to take our first steps towards a proper assessment of Marx's contributions in this field .

II

On the strength of the evidences available till date, it may be asserted, that mathematical activities began on our planet in the Neolithic era. It developed further in the centres of first urbanization: in ancient Mesopotamia, Egypt, China and India. This early mathematics was — to use a modern term — constructive, primarily oriented towards the construction of mathematical objects. The early texts containing arguments around mathematical objects and techniques grew somewhat later : in ancient Greece. Though some other civilizations — namely, the ancient Indian civilizations — had a formidable theoretical culture of their own, the interaction of mathematical and philosophical thought has been found to have been most prominent in ancient Greece alone.

It is commonly held that mathematics became theoretical with the geometrical investigations of Thales (624-548 B.C.). Thales was exposed to the mathematical practices of the ancient Egyptians, who in their turn inherited the mathematics of ancient Chaldea (Mesopotamia). Anaximandrus (611-545 B.C.) of the Ionic school founded by Thales introduced the concept of indefinite magnitude into mathematics. Another Ionic thinker Heraclitus of Ephesus (530-470 B.C.) was one of the pioneers in the conscious employment of dialectical reason in philosophy, in connection with the problem of conceptualization of change. These theoretical concerns gradually culminated in the first noticable disquiet with the indeterminate magnitudes : in the reactions of the school of Pythagoras (580-504 B.C.) to numerical analysis. And finally, in the paradoxes associated with the name of Zeno of Elea (b. 475 B.C.), the problem of definite description of the indeterminate assumed explosive dimensions. These paradoxes have a close parallel in the problematique of *Milinda Prashna* associated with the name of king Milinda or Menander (140-110 B.C.) [24].

The discovery of irrational proportions in the school of Pythagoras and the paradoxes formulated by Zeno led ancient Greek mathematics to the door steps of a "crisis of foundations". To meet this challenge Eudoxous of Cindus (408-355 B.C.) developed the method of exhaustion, already introduced by Hippocrates of Chios (450 B.C.). In the Eleatic school the concern with the indefinite went side by side with the development of the *reductio ad absurdum* argument. The problems thrown up by the contradictions involved in the attempts at a definite description of the indefinite, generated attempts to demonstrate something by expelling the obvious formal contradictions. And thus the grounds were created for raising the question of dialectics of mathematics more categorically. This the Socratics did .

In Plato's (428/427-348/347 B.C.) *Republic* we find a Socrates dissatisfied with the lack of conceptual clarity of the empirics and the Pythagoreans, proposing an investigation into the dialectic of the fundamental concepts of the existing mathematical disciplines [27, 510-511 and 524-533]. Plato attempted to put his dialectics and mathematics on a common foundation in his theory of idea-numbers. An analogous approach may also be found in Aristotle's (384-322 B.C.) remarks on an universal mathematics [3, 42]. However, it was Aristotle's systematization of logic in the image of the existing Greek mathematical theory, which influenced later mathematical developments most strongly.

Euclid (approx. 300 B.C.) benefited greatly from the critical movement of Plato's academy, revising the principles of geometry (Eudoxus was associated with this movement). After that the method of exhaustion was further developed by Archimedes of Syracuse (287-212 B.C.). Study of the conic sections was introduced earlier by Menachmus and Plato, this was further developed by Apollonios (approx. 200 B.C.) and his followers. With these developments ancient Greek mathematics reached its zenith.

In the period that followed, at first the Hellenic centres of the mediterranean countries (like Alexandria) became the repository of the ancient Greek attainments in mathematics. Afterwards the centres of mathematical and philosophical investigations in the Greek tradition shifted to Western and Central Asia. Meanwhile Chinese and Indian mathematicses were not standing still. For a brief review of the mathematical attainments of the people of ancient and medieval Asia see : [31], [32], [33]. After the 6th century A.D. the Greek, Chinese and Indian mathematicses interacted with each other, in the centres of learning of Arabic and Persian speaking Asia. In consequence we witnessed those developments in arithmetic, algebra, geometry, trigonometry and mechanics, without which the subsequent emergence of the differential and the integral calculus would not have been possible. During the 10th-13th centuries medieval southern Europe became aware of these achievements through Hebrew and Latin translations of the available literature in Arabic. This period also witnessed a renewal of European interest in ancient Greek Philosophy and literature as a whole. At around the same time, however, the living contacts between the mathematical traditions of Asia and Europe started getting snapped. The crusades (11th-13th centuries) provide the background to these contacts and their subsequent demise.

These contacts were to be re-established some four or five centuries later, as one of the results of the colonial expansion of the European capitalist powers in the East. By this time Europe became the centre of scientific activities. The new achievements of European sciences of the modern age trickled down to the littoral towns of the colonies through the distortive filter of the colonial educational policy of the European powers. However, even this meager ration of new knowledge ushered in a process of renewal of learning in some of the colonies. The efforts of Tafazzul Hussein Khan and Raja Rammohan Roy in the realm of mathematics[1], indicate the beginning of this renewal in our own country. Marx was in general aware of this process, and of its limitations; see, for example, his article entitled *The Future Results of the British Rule in India* (written in 1853) [22, 29-34]. We have lived through and are still living through the consequences of this rule. Science education in the erstwhile colonies still remains a "lagging-behind-model" of the same in the advanced capitalist countries. Add to this lag of the present, the near total absence of awareness about our past attainments in the sciences. Those who study the history of ancient and medieval Indian sciences — a large part of which is occupied by mathematics — are promptly ticked out as purveyors of "soft science" and as nationalist propagandists. Of course the emergence of the study of history of science is connected with the rise of pätriotic consciousness in our society, and it is a significant phenomenon in the history of the sciences in India, for that reason alone. But its role does not get exhausted just there, it is merely the first lap of a long and interesting journery. The study of the history of ancient and medieval sciences has profound contemporary significance. We shall mention just one example.

D.H.H. Ingalls (1951), *C. Goekoop* (1967) and *V.A. Smirnov* (1974) have, among others, indicated the need for further investigations into the *logic of relations* present in *Gangesa's*

(13th century) *Tattva Chintamani* [29]. Such investigations are already being undertaken and will be undertaken in future, with greater competence; these will provide us with a more complete picture of the logico-mathematical activities conducted on our planet, and consequently, with greater insights for dealing with the outstanding problems in this field. Here a pertinent problem may be posed (sometime in 1974-75, my teacher Pabitra Kumar Roy made me aware of it, in Santiniketan) : in India we had a long tradition both in mathematics and logic; the great names of *Gangesa* and *Bhaskaracharya* (12th century; infinitesimal method) are associated with them; and yet, the subsequent developments in mathematics and logic leading to the emergence of mathematical logic took place in Europe, and not in India — why?

It is customary to attempt an externalist answer to this (or to any other similar) question, in terms of the relative stagnation or dynamism of the modes of production. Such attempts are valuable (for sociology of knowledge) in their own right. But how are these traits (stagnation and dynamism) of the historically existing modes of production (e.g. Asiatic Mode of Production, Capitalism etc.) *mediated* into the sphere of production and reproduction of knowledge, into that of mathematical and logical thought in particular ? We still do not have a definitive answer. As of now we can only propose a strategy for future investigations.

In ancient and mediaeval Indian mathematics the deductive aspect (arguments etc.) had a subsidiary position, the constructive aspect was preponderant [on this see : *Uspensky V.A.*, What is a Proof ? // Theme No. 6 in : Reflections on seven Themes of Philosophy of Mathematics, in part three of this special supplement]. This resulted in a considerable development of the algorithms of arithmetic, algebra and trigonometry. In contrast, the deductive moment was very strong in ancient Greek geometry. Classical Greek logical theory was abstracted mainly out of this geometry. On the other hand, ancient Indian logical theories were, in the main, abstracted out of the ancient Indian prescriptive grammatical tradition, which again was constructive (algorithmic), with its stress on the construction of "algebraic" sutras for the normative conduct of linguistic activities. Perhaps this grammatical tradition itself served as the model for the ancient Indian mathematical disciplines. F.J. Staal did not overstate the point when he asserted that : "Historically speaking the grammatical method of Panini has been as fundamental for the Indian thought, as is the geometrical method of Euclid for the western [European] thought" [30]. The modern European developments in classical mathematics and logic, culminating in the emergence of mathematical logic towards the end of the last century, re-created the ancient Greek unity of the deductive moments of mathematics and logic, on a newer plane. It is only in this century that the constructivist[2] trend has begun to assert itself in the world of mathematics (and in the cognate disciplines) as a whole. And perhaps it is not accidental, that the Indians are again making their presence felt — this time through the technological end of the spectrum, in the field of computer softwares. Falling in line with the dominant tradition in history of mathematics and logic, so far, we have been studying the ancient Indian texts of mathematics and logic, through the methodological filter of classical (western) mathematics and logic. We may now attempt a *constructivist reading* of them. Hopefully, this time there will be lesser paradigm-mismatch.

Now, before we get back to the developments of mathematical and philosophical thought in medieval and modern Europe, we must mention, further, that in the period spanning from the fall of ancient Greek civilization right upto the emergence of the new bourgeois civilization in

15th century Europe, mankind witnessed several attempts to construct encyclopaedias of the sciences. Well known among these are : the *Naturalis Historia* of Plinius Secundus Gaius, in 37 volumes (70 A.D.) ; the *Risala - i- Ikhwanus - Safa* edited by Jayad bin Rifaa, in 51 volumes (9th century); *Imago Mundi* of Petrus de Allienco (1410) and, *Margarita Philosophica* of Gregor Reisch (1496). Direct and / or indirect cumulative impact of such attempts to collect the totality of existing human knowledge contributed to the regeneration of an interest in dialectics in medieval Europe. Thus, the grounds were prepared for a closer interaction between dialectical reason and mathematical investigations on a more advanced level. However, theology was the principal arena for the development of dialectical reason in medieval Europe. And consequently we find a Nicholas of Cusa (1401-1464), mixing the ideas of a dialectical logic based on religious mysticism "with the emerging notions of mathematical analysis" [5, 81].

With the rise of capitalism in Europe came its individualist philosophy dominated by rationalist metaphysics. In this very period the operations with variable magnitudes and functions were highlighted in mathematics, through the investigations of Descartes (1596-1650), Leibnitz (1646-1716), Newton (1642-1727), Euler (1707-1783), d'Alembert (1717-1783), Lagrange (1736-1813) and their contemporaries and followers. Among them first Descartes and then Leibnitz toyed with the idea of an universal mathematics. Such developments further widened the scope for investigations into the dialectics of mathematics. But the practising mathematicians took a different course ; in a sense a natural course — that of exhausting the limits of formal reason. Bolzano (1781-1848), Dedekind (1831-1916) and Cantor (1845-1918) arrived at the conclusion that to deal with the fundamental concepts of the mathematics of the variable magnitude, i.e., of the differential and the integral calculus — the derivative and the integral — in a proper manner, infinite sets must be very precisely investigated. Cauchy (1789-1857), Weierstrass (1815-1897) and others developed the theory of limit. Consequently, the edifice of classical analysis was erected upon set theoretic foundations. But almost simultaneously, the well known paradoxes of the set theory arrived on the scene. Russell (1872-1970), among others, arrived at analogous paradoxes through his studies in mathematical logic. Thus the formal developments in classicial analysis and mathematical logic prepared the grounds for a second "crisis of foundations " of mathematics (the first "crisis of foundations" was associated with Pythagorean numerical analysis and Zeno's paradoxes). In response there arose three schools : logicism, formalism and intuitionism. The arguments that followed led to considerable modifications, even abandonment, of some of their respective positions, in the wake of Kurt Gödel's (1906-1978) famous results about the incompleteness and inconsistency of even the most elementary formal systems [13]. Gradually, the emergence of the intuitionist, constructivist and non-standard analyses, brought an end to the monopoly of classical analysis, in the second half of this century. [But in the countries with a backward current mathematical culture, like our own (reflected in the terribly poor state of the investigations into the foundations, history and philosophy of mathematics in our country), classical analysis is often the only analysis "available" in the class-rooms of mathematics, till date.]

During this entire period of formal developments, spanning the whole of the 19th and nearly half of the 20th century, mathematical epistemology faced the famous antinomy posed by Kant

(1724-1804) : "the observable world is finite but we can not find its limits in space and time ; therefore the world is not finite but infinite, and there exists only the search for the limit according to the regulative requirements of reason" [5, 84].

The dualism inherent in this position was, to begin with, philosophically overcome within the idealist tradition of classical German philosophy itself, namely, in the dialectics of Hegel (1770-1831), wherein "... for the first time the whole world, natural, historical, intellectual is represented as a process, i.e., as in constant motion, change, transformation, development ; and the attempt is made to trace out the internal connection that makes a continuous whole of all this movement and development" [10, 34]. However, having remained chained to idealism, even such a grand attempt shrouded itself in obscurity and mysticism. This mysticism left its obvious mark in all of Hegel's writings on the topical problems of the sciences, mathematics included.

We have the testimony of Engels to the effect that Hegel left behind him "numerous mathematical manuscripts"[3]. But in view of their non-availability[4], as of now, we are constrained to fall back upon the relevant chapters of his *Science of Logic,* in our efforts to take a look at Hegel's excursions into mathematics [15, 129-170 and 198-344]. For Lenin's comments on the same see : [18, 116-119]. Hegel attempted an explanation of the mathematics of his time, especially of the differential calculus. But owing to his predetermined idealist point of departure, this attempted explanation remained artificial from the very outset. He merely dressed up his categories, like the "Quality", "Quantum", "Determination" etc., with mathematical trappings, especially with the frills of differential-calculus-terminology. At best it was an expression of some of the concepts of his dialectics in a mathematical language, in so far as it was possible; but it could not lay bare the objective dialectics of mathematics. Hegel himself conceded that his attempt is merely a philosophical explanation of the existing mathematical practice [15, 319].

In contrast to Hegal, Marx (1818-1883) studied the nature and history of the concepts and symbols of differential calculus and concluded that they are *operational.* Forty four years after Marx's death, in 1927, Jack Hadamard, an intuitionist mathematician, arrived at similar conclusions regarding the general nature of the fundamental concepts of differential calculus [see : *Glivenko V. I.,* Marx and Hadamard on the concept of Differential // Part Two of this special supplement, first article]. The intuitionists like L.E.J. Brouwer (1881-1966) and many early constructivists were unaware of Marx's mathematical investigations. Even to-day the overwhelming majority of the mathematicians, philosophers and historians of ideas are unaware of them. A quarter of a century has elapsed since the publication of the 1968 edition of Marx's mathematical manuscripts. Why then this lack of awareness about them, even in the late 20th century atmosphere of information boom? Why the responses to these manuscripts are so few in number ?

The reader of these lines may have noted the long gaps in, and the protracted nature of, the history of the efforts to unravel the dialectics of mathematics. Well, such is reality : ".... dialectical thought — precisely because it presupposes investigation of the nature of concepts themselves — is only possible for man, and for him only at a comparatively high stage of development (Buddhists and Greeks) and it attains its full development much later still through modern philosophy... " [11, 223]. It may further be mentioned in this connection that the stage of theoretic thought associated with the emergence of materialist dialectics was preceeded, among other things, by the following encyclopaedic attempts to classify the existing branches of knowledge : that of the French encyclopaedists (1751- 1780), under the editorship of Diderot and d'Alembert; Saint Simon's (1760-1825) incomplete attempt, and Hegel's attempt to philosophically sum up the results of the natural sciences of the Newton-Linnaeus school. The end of the positivist attempt of Comte (1798-1857) and the beginning of the attempts of Marx, Engels, Daniels, Schorlemmer etc. are near contemporaneous.

To go back to the "silence" around Marx's mathematical manuscripts : intuitionist mathematics and Marx-studies could not (in spite of certain proximity of Marx's position in mathematics with those of the intuitionists) join hands and study the mathematical legacy left by Marx. Emergence of the constructivist movement from within the intuitionist trend has perhaps created more favourable grounds. In its current phase the constructivist movement has rejected Brouwer's "semi mysitical theory of the continuum" [4, 308], but has retained his general theory about the operational nature of the standard mathematical quantifiers and connectives.

So much by way of a contextual retrospective of the problem under consideration. Now, on to the problem itself.

III

A. To-day we are in a position to extend Marx's study of the differntial calculus to the study of the symbolic calculi in general. We may investigate the dialectics of the various alternatives in set theory, analysis, topology and, theory of categories. Through a two-way association of Marx-scholars and mathematicians engaged in the study of Marx's mathematical manuscripts, the mathematical heritage left by Marx may some day get integrated into the mainstreams of mathematics, just as inside of a hundred odd years after the publication of the first volume of Marx's *Capital,* his contributions in political economy got integrated into, and began shaping, the mainstreams of economic thought.

B. However, the relevance of Marx's mathematical manuscripts does not get exhausted there. In his fundamental articles on the nature and history of differential calculus, Marx has shown that the transition from ordinary algebra to differntial calculus proper, involves an *inversion of method,* expressed through the genetic development of the characteristic concepts and symbols of this calculus (differential, derivative, dx, dy, $\frac{dy}{dx}$ etc.) culminating in their future operational role (indicating strategies of the steps to be taken). I.S. Narsky has pointed out that the methodological scope of this discovery of Marx goes beyond the framework of mathematics — it applies to the meaning of signs in general [26, 156 ; quoted in : 7, 94]. Thus the theoretical apparatus of Marx's mathematical manuscripts is of relevance to the study of sign systems, to *Semiotics*[5]. Through semiotic investigations, its methodological relevance extends to the study of all the disciplines as sign systems, as "languages", i.e., as systems of articulation. Expectedly, such studies will reveal the characteristic "internal forms" of the different disciplines and their interrelations, and thus facilitate the ongoing process of integration of the sciences and disciplines. In this vast world of semiotic investigations into the structures and functions of disciplines, extended from the computer technology to the art forms, the various trends of mathematics also have places of their own, along with the ordinary languages and other sign systems.

C. In contrast to Hegel, who attempted a philosophical explanation of the existing mathematics, Marx proposed a new way of doing mathematics bereft of metaphysics, idealism, mysticism, obfuscation and sleight of hand. In others words Marx attempted to *change* the existing practice of mathematics, its existing reality. This attempt to change the existing state of affairs of the sciences of this world — for example, of the classical political economy in the *Capital*, of the classical natural sciences in the *Dialectics of Nature*, and of classical analysis in the *Mathematical Manuscripts* — is what distinguishes the materialist dialectics from that of Hegel, and for that matter from all previous dialectics (in this connection let us recall Marx's celebrated eleventh thesis on Feuerbach). In so far as Marx and his friends were not contented with mere philosophical interpretations of the world and its sciences, the significance of their theoretic activitives became trans-philosophical. They attempted a radical reconstruction of the entire structure of human knowledge.

This attempt began towards the middle of the 19th century, but its fuller contours are gradually coming to light only in the second half of the 20th century. The first edition of the first volume of Marx's *Capital* was published in 1867. The first edition of its last (fourth) volume (edited by K. Kautsky) was issued in 1910 ; but that edition of the fourth volume (*Theories of Surplus Value)* had many radical defects; and ultimately, the final (third) part of this last (fourth) volume of *Capital* in the edition prersently in use, was brought out only in 1962. Engels' *Dialectics of Nature* was first issued in 1925; but a more complete version of Engels' studies on the dialectics of the classical natural sciences of his time could be brought out only in 1973[12]. The first edition of Marx's *Mathematical Manuscripts* came out in 1968. Marx's *Ethnological Notebooks* were brought out in 1972 [23]. The manuscripts of the first materialist attempt at investigating the dialectics of the interface of the biological and the social sciences, *Microcosmos : A Draft Outline of Physiological Anthropology*, prepared in 1850 by Marx's friend Roland Daniels (1819-1855), was brought out only in 1987 [9]. A large part of Carl Schorlemmer's (1834-1892) manuscripts on the history of chemistry remains unpublished. Nearly half of the notes and manuscripts of Marx and Engels[6] remain unpublished too ; these include : a part of Engels' notes and manuscripts on the history of England, Ireland and Germany, history of philopsophy and history of military science and, Marx's notes and manuscripts on agriculture, agro-chemistry, history of technology, geology, biology, physiology and, a part (nearly 400 pages) of his mathematical manuscripts. Publication of these notes and manuscripts and their translation into the various languages of the world will go a long way in developing our conception of the theoretical heritage of Marxism. As of now, we have just begun to understand that at the level of cognitive activities, Marxism constitutes an encyclopaedic endeavour at changing the (then and even now) prevalent ways of cognising the world. One of the open questions about the Marxist attempts in the realm of human knowledge, is about their relation with the dominant scientific programmes[7] of our time, namely, with the atomistic, Cartesian, Newtonian and Leibnitzian programmes.

It appears now, that the task of situating Marx's mathematical manuscripts in the history of ideas unfolds itself at many levels : at the level of completing text editing and publication, that of textual studies, that of situating these manuscripts within the structure and history of Marxism, that of situating them in the history of mathematical and theoretic thought, and that of situating the emerging conception of Marxsim in the history of scientific programmes. All these tasks are interrelated. And finally, we should not go about them in the spirit of an archivist of ideas or with that of a traditional historian of the past, but must take them up in the spirit of Marx, i.e, by simultaneously engaging ourselves in a comprehensive and systematic, case by case, synchronic and diachronic, study of all the newly emerged and emerging sciences and technologies of our time. In view of the gigantic strides of human knowledge since the days of Marx, and especially in late 20th century, these tasks have to be tackled by scientific collectives, aided by the latest attainments of the information processing technologies.

NOTES

1. Tafazzul Hussein Khan, the vakeel or ambassador of Nawab Asaf-ud-Daulah at Calcutta during the Government of Marquis Cornwalis (1738-1805) translated into Arabic Apollonios' *De rationis sectione*, Newton's *Philosophia Naturalis Principia Mathematica*, Thomas simpson's *Algebra* and William Emerson's *Mechanics*, during 1788-1792 [2, 39-40].

   Raja Rammohan Roy (1772-1833) wrote a modern treatise on *geometry* in Bengali [6, 407].

   These manuscripts remain untraced till date.

2. On *Constructivism* see : note 111a to Marx's *Mathematical Manuscripts* in the present volume and, *Nepeivoda N.N.*, Emergence and Development of the Concept of Constructivisability in Mathematics// Present Volume, Special Supplement : *Marx and Mathematics*, Part Three, last article.

3. F. Engels wrote to F. A. Lange on March 29, 1865 : "I can not leave unnoticed a remark you make about old Hegel, who you say lacked the more profound kind of mathematical and natural-scientific training. Hegel knew so much mathematics that none of his pupils was equal to the task of editing the *numerous mathematical manuscripts* he left behind. The only man I know who understands enough mathematics and philosophy to do this is Marx " (my emphasis — P. B.) [21, 173].

4. In response to a personal inquiry from the present author, Dr Helmut Schneider of the Hegel Archiv, Ruhr-Universität Bochum, has informed in his letter dated February 7, 1982, that neither does the Hegel Archives possess the originals of Hegel's mathematical manuscripts, nor were they ever published.

5. *Semiotics* is the study of signs (Greek *semeion* = sign). A sign is a sensorily perceptible material object, action or, event, which denotes or represents another object. Semiotics has its origins in some of the ideas of the American philosopher and logician Charles Sanders Peirce (1839-1914) and the Swiss linguist Ferdinand de Saussure (1857-1913). The range of semotic investigations are extended over all patterned communication systems : from the simplest signal systems, through the ordinary languages used by people, right upto the special languages of various disciplines. These are traditionally divided into three parts : syntactics, the study of structure; semantics, the study of meaning ; and pragmatics, the study of actual use. The growth of these investigations have given rise to prominent American French, Italian, Czech, Polish, Estonian and Russian schools of Semiotics. But in spite of both intensive and extensive developments the problem of constructing a synthetic conception of sign remains open. Such a conception must answer the

question of genesis of signs, as well as that of the generic functions of signs within a system. So far the *genetic* and *generic* approaches to the sign systems have been posed as "either-or" alternatives. A systhesis of these approaches within a "both-and" perspective is very much on the agenda of semiotic research. The mathematical manuscripts of Marx may provide this relevent perspective, in view of the fact that they contain such a conception of the characteristic signs of one language, that of the differential calculus.

6. For a brief outline of the contents of these notes and manuscripts see : [1], [17], [25] and, [28].

7. The concept of *scientific programme* has evolved out of the modern investigations in history of science, philosophy of science and science of science. The basic tenets of a scientific theory, its major premises, are always formulated within the framework of a scientific programme, which sets the ideal of scientific explanation and organisation of knowledge, and also formulates the conditions under which knowledge is considered to be both authentic and proved. The origins of this concept can be traced back to Imre Lakatos (1922-1974) ("research programme") and Thomas Kuhn (1922-) ("paradigm"). It has been further elaborated in subsequent research and alternative concepts of scientific programme have been proposed. A prominent worker in this field Piama Pavlovna Gaidenko (1934-) wrote on this concept :

" So what is a scientific programme, and why has the need arisen for such a concept?

Unlike a scientific theory, a scientific programme, as a rule, lays claim to cover all phenomena and to provide an exhaustive explanation of all facts, i.e., to an universal interpretation of everything existing. A principle or a system of principles formulated in a programme, is, hence, *universal in nature*. The well-known tenet of the Pythagoreans — "all is number", is a typical example of the concise formulation of a scientific programme. A scientific programme is most frequently formulated within the framework of philosophy (it is no accident that Engels speaks of the principled impossibility for the natural sciences "to free themselves from philosophy") [see: 11, 209- 210]. Its creators are scientists who, at the same time, also come forth as philosophers: after all a philosophical system, unlike a scientific theory, is not inclined to distinguish a group of *its own* facts, but lays claim to the universal significance of its principle. (It is precisely in analysing the structure of scientific programmes and the forms of their ties with the scientific theories, as well as in examining the evolution and change of programmes, that philosophy and history of philosophy can and must help history of science in solving its tasks).

Nevertheless, a scientific programme is not identical with a philosophical system or a definite philosophical trend. Not every philosophical system can produce a scientific programme. A scientific programme should contain not only the characteristics of the object under examination but also the, closely connected with these characteristics, possibility of elaborating a corresponding method of research. Thus, a scientific programme, so to say, determines a definite method of building a scientific theory by providing the means for the transition from a general world-outlook principle advanced in a philosophical system, to the revelation of the ties between the phenomena of the empiric world. Thus, three diverse scientific programmes came into being on the basis of ancient [Greek] philosophy: atomistic (which found its realisation in scientific theories only in modern times); mathematical (Pythagorean-Platonic, which already found its realisation in ancient times — in Euclid's *Elements* and in the mechanics of Archimedes); and finally, Aristotle's continualist programme, on the basis of which the first physical theory — the physics of the Peripatetic school — came into being. The major scientific programmes in modern times were created by Descartes, Newton and Leibnitz.

*** *** ***

A study of the emergence evolution and, finally, death of scientific programmes, the emergence and consolidation of new programmes, as well as changes in the types of ties between programmes and the scientific theories based on them makes it possible to reveal the internal ties between science and that cultural-historical entity within the framework of which it exists. Such an approach also makes it possible to trace the historically changing nature of these ties, i.e., to show how the *history of science* is internally linked with the *history of culture*.

The fact that within a definite historical period not one but two and even more scientific programmes can exist side by side, which, according to their initial principles, are opposed to each other, does not allow for a simplified conclusion about the contents of these programmes, relying on some "primary intuition" of the given culture, but *calls for a more thorough analysis of the "composition" of that culture, of the diverse tendencies coexisting in it.*

On the other hand, the existence of more than one programme in each period in the development of science shows that the idea that the history of science is an uninterrupted, so to say "linear", development of definite originally set principles and problems is unjustified. The very problems which are being tackled by science, change in the course of its history : each historical period sees their essentially different interpretation" [14, 134-137].

Such in brief is the meaning and significance of the concept of scientific programme.

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PART THREE : MATHEMATICSES

MATHEMATICSES : PAST, PRESENT AND FUTURE

MATHEMATICS AND ITS HISTORY IN RETROSPECTIVE

ADOLF PAVLOVICH YUSHKEVICH

The present paper is a revised and supplemented version of a report read at the All-Union Symposium on "The Regularities and Modern Tendencies of the Development of Mathematics", held in September 1985, at Obninsk. The report dealt with the changes that have occured in our outlook about the development of mathematics from the ancient times to the modern period, as a result of the recent historico-scientific investigations. While preparing this paper I thought that it would be advisable to provide a prefatory retrospective, as to how the past of mathematics is viewed to-day : it would be a short retrospective review of the historico-mathematical investigations themselves. The literature used for preparing this paper is too vast, and the list that follows the text of the paper contains references to only a few books or papers, indicated against their corresponding numbers, more often only the name of the author of a work and the year of its publication has been mentioned.

Understandably, here we shall be dealing with only some of the changes that have taken place in our ideas about the development of mathematics at the different stages of its formation as a science. Nowadays, sometimes we come across the view that the mathematics of ancient Egypt, or Babylon, or China was yet to become a science and it became one only in ancient Greece.However, the historians of science are yet to agree as to which fields of learning may be called a science and which may not be.The present author is merely of the opinion that the aforementioned view is not sufficiently substantiated, and on this more later.

A Historiographic Retrospective. Historiography of mathematics dates back to antiquity. One finds its odd elements in the works of Plato and Aristotle, whose pupil Eudemus of Rhodes (incidentally, not a mathematician) was the first to author a treatise on the history of gemoetry. Afterwards, invididual scholars did turn their attention to the history of mathematics; but their work have long lost all significance. The study of the history of science was highly valued by such leaders of scientific and philosophical thought as F. Bacon and G.W. Leibnitz; and in the days of Enlightenment, its leading ideologists saw the motive force of progress in the growth and spread of knowledge, wherein mathematics and mechanics (inclusive of celestial mechanics) became the leading sciences. The first fundamental work on the history of these disciplines and of some parts physics — "The History of Mathematics" by the Parisian academician J.F.Montucla — was published in this period. Its first two-volume edition appeared in 1758 and, the second,much enlarged four-volume edition came out during the period 1792-1802, only after the death of its author. This work was carried out through to the end by the astronomer J.F. Laland and mathematician S.F.Lacroix [1]. This book was a great work of its time ; in spite of the then unavoidable gaps, inexactitude and the dated methodology, the modern reader will find interesting information in it, which however, should be used with circumspection. Almost simultaneously with the publication of the second edition of Montucla's work, came out a two-volume general history of mathematics by another academician of Paris — S. Bossu (1st ed., 1802), and a four-volume history of the physico-mathematical sciences by a professor of the Göttingen University A.G.Kestner (1796-1800).

The scope of the investigations into the history of mathematics was continuously widened during the 19th century.The study of the primay sources of the mathematics of the people of medieval Orient began, the first translations of the works of the Arab and the Indian mathematicians appeared, critical editions of the works of the Greek authors Euclid, Archimedes, Apollonius and of the others were prepared, similar editions of a number of classics of the modern times were also begun, sometimes to be concluded only in the 20th century — these included the works of the mathematicians from R.Descartes and P.Fermat to A. Cauchy, B.Riemann and K.Weierstrass. The work in this direction has continued with greater intensity in the present century. Thus, the old plan of publishing the complete collected works of L.Euler (which began in 1911 and is nearing its completion only now) is being realised. The works of K.F.Gauss, N.I. Lobachevsky, G.Grassmann, P.L.Chebyshev, A.Poincaré, D.Hilbert and of others have either been published in full or in selections [for more detailed information see: the books by G.Loria [2] and K.O. May [3], which mention the classics published upto 1946 and 1973 respectively]. During the last decades of the 19th century M. Cantor, V.Buonkompayne, G.Enestrem and V.V.Bobynin took the initiative to start the first journals of history of mathematics[see : 2] and, the first courses on this subject were introduced in some of the universities — this is a rare phenomenon even to-day. The literature on the history of mathematics grew and, the famous four-volume history of mathematics by M.Cantor was published during the period 1880-1907 [4]; however, the fourth volume of this book was written by a group of scholars under the editorship of this great historian of mathematics. The work of Cantor covers the period upto 1799. It is still an useful reference book, though in certain parts it has become entirely outdated; what is more, in it the development of mathematics is viewed only in itself, outside the framework of general history and, often not even in connection with the mathematical natural sciences. The two excellent books by G. Zeiten (actually by I. Syuten) on the history of mathematics upto the beginning of the 18th century, at first published during the years 1893-1903, are of a different character : there the mathematical treatment of the subject-matter is much more deep in comparison to Cantor's work ; it is true though that they contain less of the details. Both of them have been translated into Russian [5]. In this period the interest in history of mathematics grew considerably among the mathematicians themselves, especially in the history of those disciplines in which they specialized. Hence the works on history of geometry by M. Shal (1837), those of A.Todd-Hunter (variational calculus, 1861; theory of probability, 1865), A. Enneper (elliptical functions, 1876), I. Yu. Timchenko(theory of analytical functions, 1899) and others. The aforementioned Zeiten was an outstanding specialist in algebraic geometry and a person of broad outlook.

Towards the end of the 19th and the beginning of the 20th centuries, the growth in the interest about history of mathematics was considerably promoted by the great German scientist and one of the initiators of the movement for the reform of mathematics teaching in the secondary schools, F.Klein. A three volume monograph on elementary mathematics, treated from the point of view of higher mathematics emerged out of his lectures read to the teachers of Göttingen University. First published in 1903, this book is saturated with historical materials. Its Russian translation saw two editions [6]. The history of elementary mathematics by the German pedagogue and scholar J.Tropfke [7], first published in a two-volume edition (1902-1903) and then extended upto seven volumes(1921- 1924), was mainly intended for teachers. After a long gap K.Vogel and his collaborators decided to prepare a new edition of "Tropfke" and in 1980 a structurally quite excellent edition was brought out; this book on the history of arithmetic and algebra fully corresponds to the contemporary state of our knowledge on the subjects[8]. The subsequent new "Tropfke" (geometry) has not followed, however, owing to the demise of K.Vogel. Yet another widely known history of elementary mathematics was authored by the American scholar F. Cajorie; it was published in 1896. Its Russian Translation (1910) is accompanied by highly valuable supplements from the translator I. Yu. Timchenko [9].

Like many other mathematicians F. Klein too attached a great cognitive significance to the history of mathematics. He took the initiative to include innumerable historical informations into the famous German six-volume encyclopaedia of the mathematical sciences (1898-1934). Klein wrote in the preface of this practically nearly all-embracing collective work [10], that in it there should not only be a concise and generalised presentation of the modern condition of the mathematical sciences with their applications in the other sciences and in technology, but also a description of the evolution of the mathematical methods from the beginning of the 19th century should be provided with the help of carefully selected records and reference literature. There exists an incomplete French version of this encyclopaedia (1904-1914). One finds a fuller representation of the history of mathematics in the Italian encylopaedia of elementary mathematics — published in 3 volumes and 6 books under the guidence of L. Berzolari, J. Vivanti and D. Jili, in the years 1932-1950[11]. Here, apart from the informations contained in the main text on the history of elementary mathematics, there are independent sections on the main trends of modern mathematics and on the questions of didactics; and they follow the section on elementary mathematics. This "Encyclopaedia" is very rich and is within the reach of the students of the first years of any university. But unfortunately, it is not well known beyond the borders of Italy. Running ahead, let me add here : historico-mathematical essays or sections of essays occupy a prominent place in all the three editions of the Great Soviet Encyclopedia, thanks to the unfailing directions of the editors of the GSE, in particular of V.F. Kahan and A.N. Kolmogorov.

The ever growing interest in the history of mathematics and the recognition of its status as an independent and important section of the entire system of the mathematical sciences, in the 19th and 20th centuries, may be illustrated with the help of many facts; from among them I shall adduce only three. Two of them belong to the very beginning of the 20th century; they are, both related to the Second International Congress of Mathematicians held at Paris, in the summer of 1902.

The first incident is D. Hilbert's famous paper on the "Problems of Mathematics", read in this Congress on the 8th of August. In this paper 23 real problems from various areas were posed; they exerted a strong stimulating influence upon the subsequent development of mathematics. Hilbert's problems are usually viewed from the mathematical angle of vision,and this is understandable. But there is another side to this affair : Hilbert's judgements on the perspectives for the development of mathematics and the sorting out of its especially real problems, are based on a deep going analysis of its previous development. In his own words : "History teaches us that the sciences develop uniterruptedly. We know that every age has its own problems, which are either solved or are set aside as furitless and, substituted by new ones, in the next epoch. In order to conceive the possible character of development of mathematical knowledge in the near future, we must take a look at those questions which still remain open, and survey those problems, which have been posed by modern science, whose solution we expect in the future. It seems to me that such a survey of the problems is especially contemporaneous today, at the dawn of a new century" [12]. Hilbert was an eminent mathematician of the end of the 19th and beginning of the 20th centuries, like A. Poincaré,and it is thus that he posed the question of the study óf the past of mathematics, in the interest of a creative prognostication of the perspective for mathematics.

The second noteworthy fact was the placing of M.Cantor's paper "On the Historiography of Mathematics" in the same Congress. It was one of the 4 plenary reports. Cantor began his survey with Montucla's work. There were 6 sections in the Congress. The sessions of the section on history and bibliography were held jointly with the section on teaching and methodology; these sessions were presided over by G. Cantor. The plenary report of V.Volterra on the life and work of three great Italian mathematicians — E. Belti, F. Briosky and F.Cazorati — was also historico-biographical in character. In all the subsequent congresses since then, there was always a section on the history of mathematics.

As the third example, mention may be made of the five-volume Soviet "Mathematical Encyclopaedia" (1973-1985). It contains innumerable historical informations and references as a matter of course, though there are no historico-mathematical articles proper.

Omitting the events of the historico-mathematical life till the end of the first world war, which disturbed the normal course of scientific progress, let us turn to the last half a century, marked by ever growing activisation of the historico-mathematical investigations, wherein a special mention must be made of the last 20-30 years. [One must mention, however, that, namely, in the years 1914-1919, F.Klein read his remarkable lectures on the development of mathematics in the 19th century, to a small circle of listeners, who gathered in his flat. These lectures were later on prepared for publications by R. Courant and O. Neugebauer; they were published in 1926, a year after Klein's death. A Russian translation of the first, historico-mathematical, part of these lectures was published in 1937 [13]. The second part is devoted to physics at the end of the 19th and beginning of the 20th centuries and to its mathematical apparatus; it contains short historico-scientific digressions, but they play a subordinate and insignificant role in it.] In this period many socio-historical, general cultural, ideological and scientific-organizational factors were in operation. We shall not list all of them, nor enunciate them in terms of their importance and shall be mentioning only some of them.

First of all, history of mathematics, like that of the other sciences, is organisationally constituted, with material support both at the international level and at that of the individual states. In 1929 the International Academy of History of Sciences was created and the first International Congress of the History of Sciences was held, at the initiative of a group of leading scientists from many countries. At present this Academy has nearly 230 full and corresponding members in its rolls; they are from many countries (26 of them are from the Soviet Union) (from the earstwhile USSR - Tr.). This Academy publishes its journal since 1948, and since 1968 has begun awarding a prize for outstanding scientific excellence, in the name of the great French historian of science A. Coire. After the founding of the UNESCO, an

International Union of the Historians of the Sciences was organised as its subsidiary, just like the International Union of Mathematicians, Specialists in Mechanics etc., all of which consisted of seperate national unions. The Soviet Association of the Historians of Science and Technology was established in 1957. There is no formal, juridical connection between the International Academy and the Internaional Union of History of Science, but in reality the members of the Academy occupy all the leading posts in the Union; and there are Soviet scholars among them. An important job of the Union and of its national sections is to organise international congresses (from 1929 to 1985 there have been 17 of them), national conferences, and symposiums of a general, as well as of a specific, character — dedicated to individual disciplines, problems, jubilee celebrations etc.

The most important pre-condition for speeding up the progress of investigations on the history of mathematics and of the other sciences, is the preparation of better workers with suitable specializations, in the institutions of higher learning and, the establishment of institutes of history of science. In different countries this has been done in different ways: great success has been achieved in this respect in the FRG, GDR (now Germany -Tr.), USSR (now exUSSR -Tr.), France and, of late in China; the USA has not been mentioned here, since there the preparation of workers and the organization of researches have their own specificities, and there is no scope for dwelling upon them in the present paper. As an example, here we shall briefly narrate the state of affairs in the USSR, mainly in the two centres at Moscow — one in the University, and another in the Academy.

Before 1917, (aforementioned) V.V.Bobynin taught an optional course on the history of mathematics, in the Moscow University, for quite some time. In the 30s, this course was renewed and later on made compulsory, A regular scientific seminar on the subject began to function since 1933 — it obtained all-union recognition ; research studentship on the history of mathematics was introduced, doctoral and post-doctoral work began to be defended in this specialization. After the second world war a special section was created for the histories of mathematics and mechanics, students' seminars were organised, and graduation theses were introduced in these disciplines. All this brought forth tangible results. This section is connected with the other kindered organizations in Leningrad, Kiev, Tashkent etc., as well as with the corresponding centre in the Academy of Sciences, USSR.

An interest in the history of mathematics existed in the Academy of Sciences of the USSR, since long. In the first years after the October Revolution, the Academy at first ordered the printing of A.V.Vasiliev's book on the development of mathematics in Russia from the epoch of Euler to that of Chebyshev (1921), and then of B.V.Steklov's book on mathematics and its significance for mankind (1923) — a book saturated with historical material. Like V.A.Steklov, many other members of the Academy, namely, A.N. Krylov, V.I.Smirnov, S.I.Vavilov, T.P.Kravets, P.O.Kuzmin, N.G.Chebotarev etc.— this list may be extended considerably — took an active interest in, and did study, the history of the physico-mathematical sciences. In the 30s and 40s certain measures were adopted, with the aim of imparting a more regular character to the investigations on the history of the sciences and technology, conducted in the Academy. The founding of the Institute of History of the Natural Sciences in 1945, and after its amalgamation with the Commission on the History of Technology in 1953, its transformation into the Institute of History of Natural Science and

Technology (with a branch in Leningrad) — was of decisive significance. It is the biggest institution of its kind in the world. Here a highly qualified group of historians of mathematics works in close collaboration, not only with their fellow-workers in Moscow and in the other Republics of the Union, but also with many foreign scientific centres and individual scholars, above all with those from the GDR, FRG (now united Germany — Tr.)., China, USA, France, Czechoslovakia (now the separated Czech and Slovak Republics — Tr.) and, Switzerland. In this Institute too, a permanent scientific seminar on the history of mathematics works and, specialists — research students and doctoral candidates — are helped in their work.

The total number of the more or less active historians of mathematics to-day, is not known. According to a directory published in 1978 [14], at that time their number was nearly 1500, now it must be considerably more and, probably, about 2000. Here, among other things, one must have in view the fact, that now investigations in this field are being conducted, not only in those countries, where the corresponding tradition has long since been established, but also in those, where earlier there did not or almost did not exist a national contingent of historians of the sciences; such countries include : the many Arab states, Turkey, India, China, Japan, Canada, those in Central and South America, and of course those Republics of the USSR (now CIS — Tr.), which constituted — socially and culturally speaking — the backward periphery of the Russian empire, before the October revolution. One of the consequences of the global decolonization of the earlier possessions of the imperialist states, has been a rapid growth of interest in them, in their own cultural past, and in general, in history.

Investigations in this field grew almost every month. The need to publish them, in turn, replaced the periodicals, that had been discontinued since the beginning of the 20th century. New historico-scientific journals or series of occasional thematic collections appeared; after the second world war their number grew — and continues to grow considerably.

A list of such publications, keeping it limited on the one hand to those that are more specialized, and on the other — to the most well known, in the chronological order of their appearence, is as under : "Istoriko-matematicheskie issledovaniya" ["Historico-Mathematical Investigations"] (1948-), published in the Russian language (wherein the papers of foreign authors are printed in their Russian translations; however, these publications mainly contain papers of the Soviet scholars); "Archive for History of the Exact Sciences" (1960-), publishes papers in the main European languages, save Russian (this is connected with the purely external conditions of publication — in this journal the papers of the Soviet authors are printed in other languages); and finally, the organ of the Mathematical Commission of the International Union of the Historians of Science — "Historia Mathematica" (1974-), publishes papers in 10 European and Oriental languages, as well as information on scientific activities, reviews and bibliographical surveys. There exists no data about the publications on the history of mathematics, though one may get some idea about their number from the fact that the 30 issues of the "Istoriko-matematicheskie issledovaniya"(owing to purely external reasons they were not published during the years 1967-1977) contained 600 papers. Apart from the aforementioned publications, there are the collections published by the Section of History of Mathematics and Mechanics of the Moscow University, by the history of mathematics seminars of the A.Poincaré Institute of Nantes and Toulouse and, the new journals being published in India, Japan etc. All this has not only opened new opportunities for publication, but has also stimulated further investigations.

We have stated above the approximate number of the investigators in the field of history of mathematics, and have added that their number is growing. We must mention in this connection the fact that simultaneously the mathematical preparation of these investigators has also improved — and this as a result of an improvement in the education of higher mathematics, determined by the rapid progress of the mathematical sciences themselves. The linguistic training of the historians has also improved:now original researches have become impossible or do become inferior in quality, without a solid knowledge of many languages.

The directory of historians of mathematics [14] contains the names of quite a few mathematicians — specialists in this or that branch of this science. Generally speaking, the relation of the mathematicians with the history of their own science has not been and is not uniform. It has lived through times of rise, as well as those of fall.

There are specialists, who are not at all interested in the history of science. On the other hand, one can count the names of dozens of mathematicians, among them there are those who are of the highest class, who are not only interested in the history of mathematics, but are active in this field (in particular, they add historical sections to their manuals), and they also organisationally co-operate with the progress of history of mathematics, at the level of organization of science. It is impossible to list the names of all such scholars. It is enough to name from among those of the older generation : P.S. Alexandrov, A.D.Alexandrov, B.L.van der Waerden, A.Weil, H.Weyl, J.Dieudonné, A.N.Kolmogorov, V.I.Smirnov, D.J.Struik ; and from among the younger ones — J.Dombr, H.Koch, Yu. I.Manin., A.N.Parshin, A.D.Solovev, V.M.Tikhomirov and K.Uzel. Of course this is a personal selection, and quite fortuitous at that, and the names of many a top mathematicians have been left out here — mathematicians who are systematically building bridges between mathematics and its history.

The hightening of the interest in history of mathematics among the mathematicians, especially among the scholars with a broad range, in the first half of the 20th century, had, in part, been conditioned by the crisis in the foundations of mathematics and the discussions generated by it, which drew the attention of many scholars to the historical retrospective. It was also influenced at a different level (in the first place, in our country), by the publication of the Russian translation of a part of the "Mathematical Manuscripts" of K.Marx, in 1933. [A greater part of Marx's Mathematical Manuscripts were published in 1968, though a complete edition of them remains to be published. —Tr.] One of the consequences of the aforementioned discussions has been a tempestuous progress of mathematical logic and, the follow-up action still continues. During the last few decades, the "storms" in the development of informatics and of the adjacent fields, as well as a revolution of its kind in computational mathematics, has again drawn the attention of a number of specialists to history. Clearly, to-day, in principle new paths are being outlined for the development of mathematics and, one of the means of trying to understand the paths of its further development, is to turn to its retrospective.

Cooperation among the mathematicians — the historians and the specialists, has already become an imperative necessity in our time. It is necessary for both the groups, and it is already yielding good results. Perhaps, here priority should be accorded to : 1) the publication of the classics and 2) to writing generalised works on the history of mathematics of the modern and the recent times. It is enough, for instance, to mention the publication of the collected works of Euler, Gauss, Lobachevsky, Ostrogradsky, Riemann, Chebyshev, G.Cantor, Lyapunov and of Markov Sr. All these modern publications are accompanied by commentaries, without which it is difficult, and in any case less effective, to study the works of the said scholars. In this respect the recent editions of the classics of mathematics, are as a rule qualitatively superior to those of the 19th and early 20th centuries.

In respect of the works on the history of mathematics, one may mention the multi-volume Soviet history of mathematics from the ancient times to the beginning of the 20th century [15], the publication of which is still in progress, and the French history of mathematics of the 18th-19th centuries, wherein, in many sections, the 20th century too has been dwelt upon [16]. The books dealing with the history of mathematics in large geographical areas, as for instance, the collective works on the history of mathematics in our country, which cover the subject almost upto our time [17, 18], are of great value.

Jointly authored books, of the type just mentioned, are generally speaking preferable to the monographs produced by single authors : in our time one person can not produce a balanced and thoroughgoing work on the history of mathematics from the ancient to the modern times. To be spicific, the American mathematician M. Kline could not do it; his book [19] contains very interesting and competently written chapters, yet, in spite of its volume — containing 1248 pages — there are very substantial problems, related to important mathematical disciplines, for instance, regarding the theory of probabilities, and in respect of some regions, like China etc.

We have already mentioned two generalised works on the history of mathematics [15 and 16]. Now, a few words about the general orientation of the Soviet and the French collectives are in order. In the French case it was perhaps determined by the leader of the group. In the Soviet work, mathematics has been considered, not only at the level of its ideational development or self-development, but also as a social phenomenon, in its interconnections with the social requirements, with the other sciences, engineering, philosophy etc., briefly speaking, in the interconnections of the superstructure with the base (it is not for the present author — a member of the editorial and authorial collectives — to judge, how far this attempt has succeeded). In contrast, in the French work, attention has been concentrated, save in a few points of the introduction, upon the self-development of the ideas of the so-called "pure mathematics", which have almost exclusively been considered at the level of their immanenial interconnections.[In the introduction of this work it has been said that "the most elementary concepts of modern mathematics" have been considered "in their historical contexts" and, in interconnection with their applications in the natural sciences. However, in the course of the work this declared objective has been very timidly realised.] It must be stressed, that in this work, generally speaking, one finds a very deep and substantial mathematical analysis of the historical process : almost all the authors are specialists in their respective fields of mathematics, who have painstakingly studied the essential literature on a given question, including many works of the mathematicians of our country (and this is not true of M.Kline's book). What we have said about this book [16], also holds good for an earlier work of N. Bourbaki — a remake of the historical essays contained in the various volumes of their "Elements of Mathematics", which have been published since 1939; this re-writing was mainly done by A.Weil and J.Dieudonné [20].It may be stressed here, at the same time, that in the recent years, one more often notices a greater awareness about the social factors related to the development of mathematics, also in the foreign literature on the history of mathematics. The widely popular "Concise History of Mathematics" by the American geometer D.J.Struik, which was first published in 1948 and has since been reissued many times in English and in numerous translations, including one in Russian [21] — happens to be a notable example of this kind of book. Unfortunately, the valuable supplement by I. B.Pogrebyssky, characterizing the mathematics of the first half of the 20th century — laid aside by the author — has been dropped, in the latest Russian edition of this book.

Along with the generalized works, the last half a century has also seen the publication of many original works, including such monographs, as have substantially deepened our knowledge, as well as of translations from the Oriental languages — less known to the circle of historians of mathematics of Europe and USA — into the languages of Europe. These works encompass a very large time span and geographical territory. In the short list that follows we shall indicate only the names of the authors and the years of publication.

One must begin this part of the historiographical survey, with V.V.Struve's edition of the Moscow-Egyptian-Papyrus (1930), which significantly augmented our knowledge of the mathematics of *ancient Egypt* — earlier this knowledge was almost exclusively based upon the so-called Rhind Papyrus(1877). Next, we must mention the publication of and investigations upon the cuneiform *Sumero-Babylonian* texts by O.Neugebauer (1934-1951), F.Turo-Danjen (1938), E.M.Broins(1957), A.A.Waimann (1961) and others. In the field of *ancient Greek* mathematics one has to mention, at least the works of O.Bekker (1933-). M.Ya. Vygotsky (1941), I.G.Bashmakova (1958-), van der Waerden (1950), A.Sabo (1955-) and J.P.Vernan (1962). The study of the mathematics of the *middle ages* has been conducted in a number of regional directions. E.I.Bereozkina transalted almost the entirety of the so-called "Ten books" from *Chinese* into Russian ; she also came out with a preliminary survey of her investigations, in a book published in 1980. The Japanese scholar I. Mikami gave us the first sufficiently adequate description of the history of mathematics in *China and Japan*, in the English language (1913). His subsequent important papers are in Japanese and remain almost unknown in Europe, till date. In China proper, important investigations began later, first of all in the works of Li Yan and Tsyan Baotzun (1935-1937); their main essays still remain to be translated in the European languages. At present a large group of Chinese and European specialists are working on this problematique, and many important discoveries have been made — which have often been described only in the Chinese literature.,In the recent years, the original works of K. Sheml (of France), A.K.Volkov and those of the other young specialists have been published. Chronologically speaking, among the comprehensive works, first comes the volume devoted to mathematics in the multi-volume history of civilization in China, published in 1954 ; this joint work of J.Needham and Wang Ling contains a very rich bibliography, which is now, understandbly, somewhat dated [see :the bibliographical survey in the just mentioned book by E.I.Bereozkina, which, naturally, does not contain any reference to the publications since 1980.] Yet another direction of research had the mathematics of *India* as its subject matter. The first stage of the investigations in this field has been summed up in a two-volume book by B.Dutta and A.N. Singh (1935-1938);

subsequently, interesting investigations were conducted into the infinitesimal mathematics of India of the 15th-16th centuries by S.T.Rajagopal and T.V.Vedmurty(1949-). Afterwards other indian scholars, and to a lesser extent European scholars also conducted their investigations in this area. A.I. Volodarsky's book (in Russian) on the mathematics of medieval India came out in 1977. A great cycle of work has been conducted on the mathematics of the *Arab countries, Iran and Central Asia.* Here one must at least mention the names of P. Lukei (1938-), E.S.Kennedy (1947-), R.Rashid (1968-), Kh. Vernet (1952-), Kh. Samso (1966-) and, from among the Soviet scholars those of B.A.Rozenfeld (1951-), G.P. Matvievskaya (1961-), as well as their many colleagues and pupils. [In 1983 G. P. Matvievskaya and B.A.Rozenfeld published a bibliography of the literature on Arab mathematics and astronomy, in three voumes. It is a much more detailed bibliography, than the one published by G. Zuter in 1900-1902]. Finally, the fourth direction in the study of the history of medieval mathematics : its study within the frame-work of the *European region.* Here, the aforementioned K.Vogel — engaged in the study of the development of elementary mathematics in medieval Europe (and Byzantium) — made great contributions. However, the numerous works on the higher mathematics of medieval Europe, are of special interest. Here significant contributions have been made by P.Duhem,V.P.Zubov (1947-), A.C.Crombie (1953), G.L.Crosby Jr. (1955), M.Clagett (1959-), K.Wilson (1960), G.L.Busard (1961-), J.Murdoch (1961), V.S.Shirokov (1978-) and others, who have continued, and introduced more clarity to the investigations of the pioneers. Compared to the earlier understanding of the subject, medieval mathematics and its role in the global progress of science now stand illumined in a completely new light.

The principal works on the mathematics of antiquity have in the main been summed up in B.L.van der Waerden's well known monogaph (1950), and in the history of medieval mathematics, penned by the present author (1961); both of them have been translated in a number of languages; and in view of their years of publication, both appear somewhat outdated on a number of points, in the light of our present level of knowledge of the subject.

Neither the Arab countries, nor Europe knew of book printing with the help of moving types, in the middle ages — it began only in the middle of the 15th century — and, books were brought out in the manuscript form. That is why the historians of the sciences of this period are required to look for manuscripts and the search yields rich hauls. But on the other questions too, history of mathematics is largely indebted to archival investigations : these are related to the works of Newton, Leibnitz, Euler, Cauchy, Bolzano, Ostrogradsky, Bunyakovsky, Chebyshev, Kovalevskaya, Weierstrass, Dedekind, Luzin and others. In the instances herein mentioned and in many other instances, the obtained archival materials were of great significance, not only for the exact dating of various discoveries or for solving the questions of disputed priorites but also for the discovery of hitherto unknown aspects of the creativity of the great scholars, of the activities of large scientific collectives, of the international scientific community, of the emergence of scientific contacts among individual scholars and among the institutions, in which they worked, etc. As an example one may mention the three volumes of L.Euler's correspondences, letters that he wrote to the Peterburg Academy from Berlin, in the years 1741-1765. In this period he was a foreign member of the Peterburg Academy and, a full member of the Berlin Academy (he returned to Peterburg in 1766, where he had earlier worked from 1727 to 1741).

The complete history of mathematics, like that of any other science, consists not only of the history of its ideas, but also of the history of the people who created that science, that of their collectives. In this connection one may mention the fact that specialists and historians of mathematics (as well as those of the other sciences) took part in compiling the famous 16 volume dictionary of scientific bioghraphies, published under the editorship of Gillispi (1970-1980). Same is true of that big Italian biographical dictionary (1975), which contains less detailed biographies, as compared to the former, but is very rich in illustrations and provides a substantial survey of the development of the sciences since 1875 (more than 500 pages). Paucity of space forbids us to dwell upon the books about individual scholars, though they too are an inseparable part of the historico-scientific literature.

During the last 25 years a large number of monographs have been published on the history of individual disciplines;these are highly useful even for the specialist mathematicians. It is impossible to give a complete list of these monographs here and one is constrained to limit oneself to a few examples. All of them are of a very high scientific standard, though at times quite subjective in their evaluations of the role of individual discoveries or scholars. Such are some of the published monographs : on the history of the theory of numbers and algebra (G.Vussing, 1963; L.Novy 1973; A.Weil, 1983; B.L.van der Waerden, 1985; I.G.Bashmakova and E.I.Slavutin, 1985), on the development of set theory and theory of functions (F.A.Medvedev, 1965-1982; J. Kassina and M. Giiemo, 1983), on the history of the foundations of analysis from Euler to Riemann(A. Grattan-Guinnes, 1976; a book on R.Dedekind — P.Dugak, 1976; U. Bottazzini, 1981), on the history of the trigonometric serieses (A.B.Paplauskas, 1966), on the history of the theory of functions of the complex variable (S.E.Belozerov, 1962), on the history of differential equations and of functional analysis (K.Trusdell — on the equations, 1960; V.A.Dobrovolsky, 1974; V.S.Sologub, 1975; E.Lutsen, 1981), on computational mathematics and computing machines (G.Goldstein, 1977; I.A.Apokin and L.E.Maistrov, 1974), on the theory of probabilities (L.E.Maistrov, 1967 and 1980), on non-Euclidean geometry (B.A.Rozenfeld, 1976), topology(J.K.Pon, 1974), and logic (J.M.Bochenski — in English — 1961; N.I.Styazkin, 1964; T Kotarbinski, 1965). It is an incomplete list, but even if it is supplemented with a few more names of hitherto unmentioned monographs, even then that would not encompass all the basic mathematical disciplines. Work in this direction is of first order importance, and it is still continuing. Here, owing to insufficiency of space, we shall not be able even to mention many collections, devoted to the work of individual scholars, the development of this or that discipline in a given country, that of the different fundamental concepts, like the number, function, infinitary magnitudes, differential, integral etc. etc., and the activities of the individual institutions, academies, societies, periodicals etc. At times, even a series of articles (for example, those of O.B.Sheinin on the history of the theory of probabilities and its applications) is of no less importance,than this or that book.

Let us conclude the retrospective of the historico-mathematical investigations here, and turn to a retrospective of mathematics itself. The problems selected herein for consideration, make no claim to completeness and, naturally, they express the interests of the author. As far as possible, the following exposition follows the chronological order of development of mathematics and, takes care of its regional specificities.

Neolithic Mathematics. Our judgements about the formation of the earliest mathematical notions, formed in the pre-historic times, are based on archeological data, sometimes, upon the written legends and inscriptions, preserved on the architectural structures and utensils, on the pictures found on the rock surfaces of the cave dwellings ; and, finally, our ideas on the subject are also developed in analogy with the mathematical knowledge of those people and tribes, who are, or were a short while ago, situated at the lowest levels of cultural development. For some time now, the earliest developed culture known to us has been, the one that existed on the Indus river basin, in the middle of the 3rd millennium B.C. — the so-called Mohenjo-daro culture. Mathematical texts from this culture have not survived, and the inscriptions that have remained intact, have not been deciphered. We have extremely meager data about the arithmetical and geometrical knowledge of this culture, and still less — about its history; it appears to be close to the culture of Sumer. However, results of the investigations into the culture of Mohenjo-daro, are not in confirmity with the hypothesis regarding the introduction of this culture in the Indus Valley by some of the Aryan tribes (i.e. these studies do not confirm the hypothesis of B.L.van der Waerden ; on this more later). Recently, an even more ancient culture has been discovered in Upper Egypt, but it remains almost uninvestigated.

The earliest preserved Egyptian representations of numbers date back to the first half of the 4th millennium B.C., but the two, aforementioned , so far preserved, basic mathematical papyruses date back to the first centuries of the 2nd millennium B.C. The Babylonian cuneiform texts are divided into three basic categories : 1) the most ancient economic texts of Sumer, 2) the tables for multiplication, division and other operations, often also meteorological tables — dating back to the end of the 3rd millennium and, 3) some even later collections of problems — approximately belonging to the 9th-7th centuries B.C. All these written documents are close in time to the Indus Valley Civilization and, all of them go back to even earlier periods; whether or not there existed any direct contact between these civilizations, that however, remains to be established. A somewhat authentic information about the ancient Indian mathematics of the subsequent centuries, belong to a much later period ; it is related to that epoch when the religious books — the Vedas — were composed. It is contained in some essays, enunciating the rules for the *construction* of sacrificial altars, in the so-called "Śulva-sūtras", written, probably, in the 6th and subsequent centuries B.C.; these have come down to us in several variants. The Chinese culture is also very ancient, but it is practically impossible to isolate the authentic facts from the legends, contained in the later chinese chronicles. [For example, about the awareness of some particular instances of the theorem of Pythagoras in the 12th century B.C., and with its generalised form — in the 6th century B.C.] There is no doubt about the fact, that already in the school of Mo Zi, the philosopher and logician, i.e. in the 4th century B.C. or even earlier, the Chinese attained a high level of mathematical knowledge. By then, probably, many of those problems about which we came to know from the most ancient mathematical and mathematico-astronomical works — "Mathematics in Nine Books" and the "Treatise on Gnomon" — were already formulated and the methods of their solution were found; these books became famous through their editions published around the beginning of the Christian Era.

Sometime ago, in 1983, B.L. van der Waerden attempted a partial reconstruction of the mathematics of the Neolithic epoch. It predates the Egyptian, Babylonian, Indian and Chinese mathematicses and serves as their most important primary source [24]. In the opinion of this leading algebraist and outstanding historian of science, there existed a very highly developed mathematics in the territories of Central Europe and Great Britain, somewhere between 3000 and 2500 B.C. ; afterwards, this mathematics spread towards the South and the East, into the territories of Egypt, Babylon, India and China. Its traces are to be found in ancient Greece too — and this includes the works of Euclid and Diophantus; however, it was the Greeks who radically reorganised this ancient mathematics and created a deductive science based on definitions, postulates and axioms. It will not be possible for us, here, to enter into a detailed analysis of the ideas of van der Waerden. His book contains many interesting and valuable remarks, but his entire conception has been, on the whole, less convincingly argumented. Three basic arguments have been put forward in the author's introduction. The first among them — the presence of Pythagoras' theorem and of its application for transforming a rectangle into a square, in the "Śulva-sūtras", wherein the sides of the right-angled traingles used in the constructions are proportional to the "Pythagorean triplets" of natural numbers. "Pythagoras' theorem" and the extensive tables of Pythagorean number triplets were well known in ancient Babylon. From this, van der Waerden concludes — following A. Zaidenberg (1978) — that there existed some kind of a common source, of the Babylonian algebra and geometry, the Greek geometrical algebra and, the Indian geometry. The second argument—the existence of a large number of similar problems in the ancient Chinese "Nine Books of Mathematics" and in the ancient Babylonian texts, assuming in particular a knowledge of Pythagoras' theorem. And the third argument — towards the end of the 70s, a number of archeologists studied some megalithic structures, erected on some platforms, for ceremonial rituals, as well as for definitely oriented astronomical observations. These platforms are bordered with menhirs, placed along circular, elliptical or oval lines or along the circumference of forms flattened out into circles. It is possible, some times, to determinately inscribe individual integral-numerical Pythagorean triangles into these figures. Such megalithic structures were erected since the middle of the 4th millennium B.C. and were widespread in Central Europe, Great Britain, Ireland etc., in the first half of the 3rd millennium B.C.; and, according to van der Waerden and Zaidenberg, this testifies to the existence of a highly developed mathematics in the Neolithic epoch and it influenced the entire subsequent development of mathematics.

The decisive argument of van der Waerden is as follows : the discovery of the Pythagorean theorem and of the Phythagorean number triplets, were great discoveries, and the great discoveries of mathematics, physics and astronomy are, save in the rare cases, made only once; independent discovery of the said theorems and number triples in ancient Babylon (around 2000 B. C.), India, Greece (where they were well known not later than the 7th-6th centuries B. C.) and China, is improbable. Some unknown people took all these with them in the course of some migrations to the East.

This argument cements the entire conception of van der Waerden. It has been illustrated with the examples of momentarily invented epicycles and eccentric circles, of the establishment of the sphericity of Earth, the heliocentric system of Copernicus, the three laws of Kepler, and the laws of motion of Newton as well as his law of universal gravitation, the laws of optics etc. Independent discoveries — for example, of the non-Euclidean geometry by Lobachevsky, Gauss and Bolyai — are very rare. There is inexactitude in van der Waerden's enumeration; for example, R. Hooke discovered the law of universal gravitation independently of Newton; it is true, however, that he could not construct a system of celestial mechanics. The defect however, is not with the particular instances of inexactitude; independent discoveries are by no means a rarity in the history of mathematics and of the sciences in general. Here are some examples : the logarithmic tables of Napier and Briggs, the calculating machines of Shicard and Pascal, the analytical geometry of Descartes and Fermat, the differential and integral calculus of Newton and Leibnitz, the theory of elliptical functions of Abel and Jacobi, Dedekind's and Zolotarev's theory of cut, the special theory of relativity of Einstein and Poincaré, Urison's and Menger's topological theory of measure... This list may be indefinitely extended further and, in general, in the given realm of questions, it is difficult to count and mutually compare the probabilities. One way or the other, according to van der Waerden, when a theorem like that of Pythagoras, is found in different countries, then the best course open is to accept the hypothesis of their dependence upon a primary source and to use it, as a heuristic principle.

It stands to reason, that the question of dependence or independence of identical discoveries in different cultural environs, requires to be investigated. Only this much is certain, that the solution of this question must not be based upon highly indeterminate probablistic estimates and unprovable presuppositions about the course of development of humanity. Having put forward his hypothesis and heuristic principle, van der Waerden himself then and there notes many points of contact between the mathematics of China and Babylon or India and Greece; incidentally, these comparisons, made by him, are highly interesting and deserve serious attention. But if such points of contact, yet to be studied in their full scope, did exist, then it is legitimate to ask oneself : were not the theorem of Pythagoras and the Pythagorean triplets born in the civilizations of Mesopotamia, from where they spread out in different directions ? Why assume the existence of a highly developed Neolithic mathematics in Europe, in the 4th-3rd millennium B.C., about which we practically know nothing, when we know for certain that a Sumero-Babylonian mathematics did exist, which is known to us, at least in part ? And what makes the hypothesis of a single source more preferable to the hypothesis of independent discovery of the theorem of Pythagoras, in course of the progress of architecture, that developed upon the ground reality of the general civic and ritual requirements of the people of a number of regions, which did attain similar levels of culture, at approximately the same time ?

About the integral numerical Pythagorean triangles, which may be inscribed within the contours, along which menhirs were placed in a number of instances, it is not at all understandable, as to why the builders of the structures were in need of them. Traces of such triangles were not retained. And the contours themselves — be they spherical, flattened out and consisting of the arcs of circles of different radii, oval or even near elliptical — were outlined, one should think, with the help of simple string contraptions. Right-angled triangles are not necessary for all such constructions.

Ancient Orient. While going over from the unwritten evidences of mathematics to the most ancient written mathematical documents, one must first of all turn to Egypt and Babylon. Here the information at our disposal, is clearly fragmentary, but nevertheless, it does allow us to judge the systems of numeration, methods of computation and predominant types of problems of both the civilizations. It appears, that in Balylon, that component of mathematics was considerably more developed, which we can call algebraic : here we find cycles of problems expressed in quadratic equations or in systems reducible to them — the Egyptian papyruses do not contain such problems; we have already mentioned the fact that the theorem of Pythagoras and the Pythagorean numerical triplets were known to the Balylonians. One of the Egyptian papyri dating back to the beginning of the 2nd millennium B.C., contains a simple algebraic problem, where the sum of the squares of the unknown quantities is given, there exits a given linear interconnection among the unknown quantities and the solutions — 6, 8 and 10 — happen to be double of the simplest Pythagorean numerical triplets; but the text does not mention any triangle, thus it would be hasty to conclude about any acquaintance with the general theorem of Pythagoras and with the Pythagorean numerical triplets in ancient Egypt.

During the last few decades, there were only a few substantial discoveries in this section of the history of mathematics : the most interesting among them being the "Balylonian numbers" detected by A.A. Waimann (1957) — these are numerical triplets, expressing the ratios of length of some line segments, parallel to the bases of a trapezium, which divide it into paired bands of equal area; these numbers turn out to be Pythagorean numbers, their sums, and differences. But the study of the mathematics of Babylon and Egypt produced a number of interesting reconstructions of those methods with the help of which various problems were solved there. In both the cases, the texts contain only calculations, providing the solution or even, straight off, the answer, without any explanation : the enunciation is prescriptive in character and does not include any such element, which we would have now termed theoretic. It is clear, however, that the solutions of many problems could not have been obtained purely empirically. This is true, for example, about the rule for calculating the volume of a truncated pyramid (Egypt) or, about the solution of quadratic equations (Babylon); it is not likely, that the rule for the summation of the sequence of the squares of natural numbers and, the correlation among the Babylonian and Pythagorean numerical triplets (Babylon) or, the (approximate) equality of the area of a circle with that of a square, a side of which is equal to 8/9 of the diameter of that circle, were detected accidentally. All the texts of the ancient Oriental mathematics, known to us, highlight only one side of it : these are either manuals for solving a definite type of problem, or collections of exercises with answers, and sometimes with verifications. There is no doubt, that there were mathematicians with a command over the tools of arithmetical, algebraic or geometrical deductions. This apart, we do know that when the texts under consideration were being composed, mathematics had already attained such a level of development, that apart from the problems generated by the direct requirements of economic, political and technological practice, it also handled those problems which were bereft of all practical significance, those which arose in course of the development of mathematics itself. The computation of the area of a rectangle with given sides, is an elementary practical problem, solved arithmetically. Reflection on it gives rise to an abstract algebraic problem : the determination of the sides of a rectangle in terms of a given area and perimeter.

Some historians of science (A. Sabo, I.G. Bashmakova, P.P. Gaidenko and others) put forward the view, that only in Ancient Greece did mathematics arise as a science, when significant portions of this discipline began to be constructed in the form of deductive axiomatic systems. They opine, that there exists no ground for thinking, that mathematical knowledge was formulated in ancient Egypt or Babylon, into such systems; there, many results were obtained empirically or through unsubstantiated generalization from particular modes of calculation or measurement. Notwithstanding this, the following remark of N. Bourbaki [21] is fully convincing : it is not possible to view the entirety of Babylonian algebra as a simple collection of exercises, solved empirically, to the touch, and if it does not contain any "proof" in the formal sense of the word, then some sort of, not yet fully realized, logical arguments were put forward. By using the analogue of a mathematical terminology, used in another context, we may call ancient Oriental mathematics — "piece-wise deductive".

It has already been said in the beginning of this paper, that the words "proof" and "science" do not have any univocal meaning when they are viewed in their history, different contents were put into them at different times. The refusal to call the mathematics of Egypt and Babylon — science, because there are no proofs in their written documents, is as groundless, as the exclusion of impressionism or the abstract school from painting, because they are "not realistic", and of the works of Eluard or Khlebnikov from poetry, as these do not resemble those of Verlen or Blok. The same is true also in respect of the ancient mathematical texts of China and India.

Ancient Greece : Hellenism. After Egypt and Babylon, it is natural to turn to Ancient Greece. There are several aspects of the problem of the sources of Greek mathematics, the first among them being the question of Oriental influences. Taking it up in the case of Egypt, B.L. van der Waerden now, as they say, elevates it, in the ultimate analysis, to a hypothetical European culture of the Neolithic epoch; O.Neugebauer takes into consideration the emergence of the theories of irrationality, proportions and integrations within Greece and thinks that the Greek geometrical algebra shows Babylonian influence, which became stronger in the beginning of Hellenism, and he raises doubts about the rôles of the Ionic school and of Pythagoras ; I.G. Bashmakova is of the opinion that Pythagoras is the creator of mathematics as a science; A.Sabo gives precedence to the influence of the Eleatic school and the introduction of the rule of contraries; L.Ya. Jmuid has recently raised doubts about the presence of Oriental influence ... It is apparent that opinions are changing and, clearly, the discussion around this question will continue. Perhaps, the question of formation of Greek mathematics should be considered within the wider frame-work of social and ideological development of the entire Mediterranean culture. Here the reader is recomended to get acquainted with the materials published in the collection : "Metodologicheskie problemy razvitiya i primeneniya matematiki" ["Methodological Problems of Development and Application of Mathematics"] (M., 1985), especially with the section : "Metodologicheskie aspekty stanovleniya matematicheskovo znaniya" ["Methodological Aspects of Formation of Mathematical Knowledge"]

While dealing with the problem of formation of the mathematical deductive method, in that specific form, which it assumed in Ancient Greece, i.e. in the first place, in the axiomatization of geometry (but not of arithmetic, the reason behind which has been investigated by S. A. Yanovskaya in 1958), from the very first steps one runs against the non-univocality of the possible interpretations of the ancient idea of the infinite, especially in the early stages, and of the corresponding terminology. What gave rise to this idea? How to understand the "apeiron" of Anaximander [apeiron — a concept introduced by Anaximander of Miletus (c. 610-546 B.C.) to denote boundless, indefinite, qualityless matter in a state of contsant motion — Tr.] or the "aporias" of Zeno of Elea (c. 490-430 B.C.) [aporia — a problem which is difficult to solve, owing to some contradiction in the object itself or in the concept of it — Tr.] ? There is no doubt about the need for discussing these questions, but we are still far away from their unanimous solution.

Having mentioned the names of Anaximander and Zeno of Elea, one has to state that the problem of infinite did have a decisive impact upon the entire methodology of Greek mathematics, upon its various aspects. 60 years ago H.Weyl wrote, that mathematics is the science of infinity and if an intuition of the infinite was characteristic of the Oriental world, where it did not give rise to any question, then the Greeks reconstructed the polar opposition of the finites and the infinites into powerful instruments for the cognition of reality. Unfortunately, the opinions about the problem of infinity and about the infinitesimal methods of the ancient Greeks, are so divergent, that one has to refrain from characterizing them in the present paper.

In the recent times, unfortunately, the study, almost only, of the "Arithmetic" of Diophantus, has come to occupy one of the foremost positions in the study of the history of Hellenic mathematics. I.G. Bashmakova was the first to produce a deeper study of this work, utilising the tools of modern algebraic geometry (1972-); soon it was independently extended to the study of R.Rashid — the so called Arab Diophantus, by J.Sesiano (1974-) and, to that of the so-called "Diophantine analysis" upto the epoch of Fermat (I.G. Bashmakova and E.I. Slavutin, 1984); these studies threw a new light upon the formation of this field of mathematics, which played an important role in the development of theory of numbers and algebra. In a recent book on the development of the theory of numbers (1983), A. Weil has related the entire work on indeterminate analysis upto the time of Vieta [1540-1603] and Bachet [1581- 1638], with the pre-history of the theory of numbers, since, therein, the attention was fixed, not only on the search for the integral, but also on that for the rational solutions. Weil has considered the works of Diophantus mainly in connection with those of Fermat, whom he considers to be one of the founders of the modern theory of numbers, together with Euler, Lagrange and Legendre.

The "Arithmetic" of Diophantus exerted a direct or (and) indirect influence — mediated through some unknown links — upon the development of Arab algebra. It has been a great influence. We must mention here the fact, that so far the very emergence of the "Arithmetic", has almost always been viewed as an isolated event in the development of the mathematics of the Alexandrine epoch. This work determined a trend of thought different from the classical one, proposed by Euclid, Archimedes and Apollonius. To all appearence, it was the result of a synthesis of the Classical and the Oriental traditions; the creation of the empire of Alexander of Macedonia and, after its fall — the emergence of several Hellenic states, created the general historical preconditions for this synthesis. There are common elements in the works of Diophantus and in those of Hero of Alexandria, who lived nearly two centuries before the former. However, there still exists a very big gap in the primary sources known to us, which can not be filled by the data on hand about the problem of piers, related with the name of Archimedes. The ancient method of solving this problem is still not known to us. The possible connection of "Diophantine Analysis" with the mathematics of India and China also remains unknown. In India and China too, the integral and rational numerical solutions of different kinds of indeterminate algebraic equations, did occupy an important place.

The Middle Ages. As we go over to the Middle Ages, we must first of all state that this term is unsatisfactory, even if because of the fact that in different regions its natural boundaries belong to different centuries. In the absence of a better name, however, we shall be using this term. In places it began at the junction of the pre-christian and the christian eras, at others even later and, came to an end in the 15th-16th centuries. The states that existed in this period, over considerable parts of the territories of China, India, the Arab countries and Europe, were in the main similar types of feudal (? — Tr.) economic and political formations [while discussing the ancient and the middle ages, the question of the Asiatic etc. modes of production can not be/should not be avoided — Tr.]. They attained almost the same levels of technical and material culture. It stands to reason, that the exchnges of material and spiritual values that took place among these regions, were by no means regular and, were interrupted by wars and internal disorders. A natural consequence of all this has been the emergnce of similar practical problems before the mathematics of these four regions. In some of the works — written in the period 1958-1961 by the present author, often in collaboration with B.A. Rozenfeld — a conception of medieval mathematics has been proposed, wherein it has been considered as a single whole; but the specificities inherent to the mathemtics of each of these regions have been taken note of there; these specifics were largely the consequences of even earlier scientific, philosophical and religious traditions prevalent in these regions. That is why, it would be better to consider the changes that have taken place in the retrospective of mathematics, during the last few decades, separately, within the frame-work of each region. It must be mentioned initially, that during the last few decades considerable success has been achieved in the study of the medieval mathematics of many Oriental countries ; and, to a great extent this was made possible by the decolonization of the territories under the control of the imperial powers, the emergence of independent states in Asia and Africa, as well as owing to the fast progress in those Republics of Central Asia and of the Caucasus, that were backward areas before the October revolution in Russia and, were in the best of circumstances — second grade areas of the Russian empire.

China. Our knowledge of the development of mathematics in China has greatly increased in the recent years. European scholars obtained their first solid informations about Chinese mathematics through an English language book (1913) by the Japanese scholar I. Mikami. In the first half of the 20th Century, work was conducted in this field in Europe and in China, mainly independently of each other. During the 30s-60s, considerable contributions were made by Li Yan and Tsyan Baotzun, and by Mikami, who continued his investigations; however, they wrote mainly in Chinese and Japanese and, for a long time their books and papers were accessible to only a few European or American historians of science (now the number of sinologists and historians of mathematics have increased). That is why the publication of a generalised work on the mathematics of China, by the English sinologist (and biologist) J. Needham, was an event of great significance; Needham wrote this book (1959) in collaboration with the Chinese mathematician Wang Ling. Later on the work gathered momentum in China and in Europe. Many of the classical works of Chinese mathematics were translated in the European languages, and books were written about the great mathematicians of China. From among these translations one must first of all, mention the Russian edition of the tracts of the so- called "Ten Books", which have been provided with commentaries. The last of these "Ten Books" was written in the 7th century and, the earlier "Nine Books of Mathematics" were published some where near the beginning of the Christian Era. E.I. Bereozkina's translations of these works were published in the period 1957-1985; and in 1980 she initially summed up the results of her investigations in a special monograph, wherein she has considered the attainments of the mathematicians of China upto the beginning of the 14th century, but somewhat more briefly. K.Vogel prepared a German translation of the "Nine Books of Mathematics" (1968) and, this is accompanied by his own commentaries. A collection of papers on the same book and on its most important commentary, composed in the 3rd century by Liu Huei, has been published in 1982, in the Chinese language, along with an English resume. The authors of these papers — Bai Shanshu, Li Di, Shen Kanshen and others — made a significant contribution to a fuller study of the "Nine Books of Mathematics". [The present author is greatful to A.K. Volkov, for translating parts of the big chapters of this collection, as well as Shen Kanshen's review (1985) of E.I. Bereozkina's book (1980) mentioned above — from Chinese. Kanshen has justly stressed the importance of the commentaries of Liu Huei, in his review of Bereozkina.] For nearly one and a half thousand years the mathematicians of China, on very many instances, took their cue from these "Nine Books of Mathematics", and this explains the special attention that has been paid to it ; see : the bibliography prepared by the German sinologist G. Kogelshats (1981).

The epoch of pre-decline flowering of mathematics — above all of algebra and theory of numbers — in ancient China of 13th century, has been the subject matter of a number of important investigations. A Belgian scholar U. Libbrecht (1973) made a detailed study of a treatise by Tsin Tsziu Shao ; Leim Lai Young, who works in Singapore, published (1977) an English translation of a treatise by Yang Hue; J. Go (1977) published a French translation of the works of Zhu Shijie, and used therein the special symbolism devised in that epoch. K. Sheml (1982), just like Go, used a semi-symbolic language, in his doctoral dissertation on an algebraic treatise of Li Ye — a contemporary of Qin Jiushao. Unfortunately, this dissertation, as well as a much earlier work by another French sinologist K. Shrimpf (1963), on the mathematics in China upto the 7th century, has not been published.

In this connection, special mention must be made of the papers of Ho Pen-Iok (Malaya) on Qin Jiushao, Zhu Shijie, Li Zhi (or Li Ye), Liu Huei and Yang Hue, in the 3rd, 8th and 14th volumes of the American Dictionary of Scientific Biography [22].

As a result of all these investigations our knowledge of the development of mathematics in China, has been greatly extended. It appears now that its arithmetico-algebraic component is richer than what it was thought to be, in the beginning of this century. At the same time, a number of questions remain unsolved and far from all the important primary sources have been studied till date. Undoubtedly, there had been mutual interaction among the mathematicses of China, India and the Arab countries, which was reflected even in Europe : the migration of similar or identical problems, and not merely the deductions constructed upon the shaky principle of *post hoc ergo propter hoc*, testify to this effect. Modern historians of mathematics do take note of all these moments, but by far not all of them are clear and definitive.

P. Liukei has advanced a hypothesis about the transference of the rule of two false positions and of the methods of extracting the square roots and cube roots of rational numbers, to the Arab countries from China; but it lacks certitude. Perhaps the Belgian sinologist L. van Hee was the first to put forward the idea, prevalent till recent times, that Liu Huei solved the problem of measuring inaccessible objects and the distances upto them, by basing himself upon the similarity of traingles. According to a recently advanced hypothesis, Liu Huei used some methods which are characterestic of the Greek geometrical algebra. We have already mentioned the noteworthy proximity, of the treatment of the basic ideas of geometry, in one of the works of the Moist school 4th century B.C., with a somewhat earlier ancient Greek treatment of the same. Exactly in the same way, the similarity between Liu Huei's methods of approximate calculation and those found in Archimedes' "Measurement of Circle" — notwithstanding their inessential technical differences — has been mentioned more than once. All of this leads to an idea about the existence of scientific contacts between China and the Hellenic countries : trade was conducted between China and the Roman empire. In the more recent years an observation of D.R. Wagner (1978), to the effect that the ancient Chinese mathematicians used the so-called principle of Cavalieri, in their studies of the problem of cubature of a sphere, has become an object of special interest in Europe ; apparently, it was known much earlier, to Mikami (I have not yet been able to verify it). The history of this question is as follows. Liu Huei expressed the volume of a sphere in terms of the volume of a body, contained within the surfaces of two cylinders, inscribed in a cube and having mutually perpendicular axes, but he could not determine the volume of this body. It is very likely that Liu Huei used the so-called principle of Cavalieri, though this principle has not been formulated in any of his texts known to us. We first come across a formulation of this principle in the 5th century, in the writings of Zu Chongzhi. [It is difficult to translate this formulation with exactitude, as the corresponding terms do not have fully determinate mathematical significance in latter Chinese speech; while referring to Anaximander's concept of "apeiron", we have already mentioned the possibility of non-univocal understanding and translation of the ancient terms.] Zu Chongzhi's son Zu Heng applied this principle and found out that the volume of such a body is equal to the 2/3 of the cube, which gives us the cubature of a sphere. It is remarkable that the result of Zu Heng has been formulated in the 2nd proposition of Archimedes' "Epistle to Erastophenes"; in this work the method of indivisibles has been regularly used for heuristic purposes (but not for obtaining any "strict" proof); unfortunately, the conclusion of this proposition, contained at the end of the "Epistle", is not known to us. We do not find a statement of the "Principle of Cavalieri" in the Greek texts known to us, however, Archimedes' quadrature of the ellipse, viewed as the result of compressing a circle, leads to the thought, that in essence, this principle had been used by him intuitively. Personally to me it seems plausible, that there had been a Greek influence upon the infinitesimal methods of the Chinese mathematicians of the 3rd-5th centuries.

On this score Leim and Shen Kanshen display greater restraint (1985), when they say that Liu Huei's text contains no evidence of a Greek influence; such an influence is discernible only later on, in the works of Mei Wendong — a mathematician active in the 17th-18th centuries.

On the whole, however significant may the achievements in the study of Chinese primary sources be, still a very great amount of work remains ahead of us. Perhaps one of our primary tasks is to study all the hitherto known commentaries on the "Nine Books of Mathematics", which shed light upon the methods employed for solving those various problems, which we now classify as algebraic, number-theoretical and geometrical. The Chinese treatises, beginning with the "Nine Books of Mathematics", do not contain any proof, there we find only laconic mention of the methods of solving the problems; the substantiations of these methods are often met with in the commentaries. More than thirty years ago I expressed the opinion, that it would be unjust to judge the mathematics of China on the basis of its collections of exercises, that here and in the case of the mathematics of ancient Orient (and, I add, of India), we must distinguish between the manner of presenting a discipline in the text books, from the creative elaboration of the methods of investigation which preceeds it.Both in China and in Europe, the study of these commentaries are, in essence, in their early stage. Recently, A. K. Volkov examined the commentaries on the rules for calculating some areas (1985) and, therein he noted that in the mathematics of ancient China, the very concept of "proof" and the "systems of proofs", have their own specific characteristics : not an axiomatic theory, but a theory of models happen to be a more adequate analogue of the ancient Chinese logical system, and their criteria for deciding about the correctness of propositions correspond to this; this question deserves a more detailed study. The intensive work that is being carried out in this field in China, USSR (now erstwhile - Tr.), France, FRG (now Germany — Tr.), and in the other countries, will soon yield new results and, one may say, newer postulation of the problems too. The development of mathematics in China beyond the classical period, i.e. in the 14th century and afterwards, has not at all been touched upon here.

India. While dealing with B.L. van der Waerden's hypothesis about Neolithic mathematics, we have already mentioned the latest investigations on the history of mathematics in India. Apart from the more detailed analysis of the works of individual mathematicians like Shridhar or Mahavir (A. I. Volodarsky, 1966, 1969), the reconstruction of the solutions of some problems in the Āpastamba "Śulva-sūtras" (A. I. Raik and V. N. Ilin, 1974), the recent observations of R. Singh (1985) about the so-called Fibonacci numbers of 7th-8th century Indian mathematics or, the works on the history of Indian astronomy by D. Pingri (USA, 1963) and A. K. Bag (India, 1966-), the most interesting attempts in this field were concerned with the exact determination of the connections of Indian science with the science of the other regions and, with its place in the overall progress of mathematics. The aforementioned book by A. I. Volodarsky contains an overall survey of the work done prior to 1977. [Here the reference is to : *Volodarsky A. I.* Ocherki srednevekovoi indiiskoi matematiki (Esays on Medieval Indian Mathematics). M.: Nauka, 1977, 182 pages. — Tr.] It appears that, comparative analysis must be further continued in this direction. It is a fact, that here the investigator has to face a deficiency of exact informations, so much so, that even the emrgence and the earlier stages of development of the now commonly accepted system of decimal positional numeration, remain largely unclear;

Arab Mathematics. During the last quarter century, our ideas about medieval Arab mathematics have changed no less substantially, than those about mathematics in China. The term *Arab Mathematics* has struck roots and comes in handy, though in fact, at issue here is the mathematics of the people of many countries[where at a given period of time Arabic has been the principal language of science — Tr.], stretching from the Pyrenean peninsula, through the northern, Mediterranean regions of Africa, Near East, Central Asia and further ahead, roughly upto the present borders of China and India [in the case of India it should mean upto the India-Burma border of Mughal India and, in case of China — at least upto the eastern borders of Sinkiang or Chinese Turkestan — Tr.]. The sources of Arab mathematics remain largely uninvestigated, they go back to the mathematics of Babylon and Egypt, a large number of Hellenic states, Byzantine, and to the science of ancient Khwarazm, not to speak of the much later influences which came through the contacts with China, India etc. Here one may put forward a number of questions, but in the absence of exact data, it would be more than difficult to answer them. We have left out such questions in the present paper. But one must at once make one observation about the expression "Arab Mathematics".We are unable to find a similarly brief expression, which may be a substitute for it; but one has to stress the fact that even a brief bibliographical survey of the original literature shows the special significance of the contributions of the mathematicians of Central Asia and, this justifies a separate treatment of the history of mathematics of Central Asia of the period under consideration, in a number of books. When one deals with the cultural developments of the Central Asian Republics of the USSR [now CIS — Tr.], which were, quite understandably, closely connected with the culture of those regions, where Arabic or Persian had been the pricipal language of the scholars, then such a separate treatment becomes essential.

A more detailed study of the already known Arab works, and, to a greater extent, an analysis of a large number of mathematical manuscripts preserved in the various libraries and archives, showed that medieval Arab mathematics did attain a scientific level, which is much higher than what it was earlier thought to be. It goes without saying, that the mastery of the Greek scientific heritage, which was one of the consequences of Arab expansion and of the formation of the Arab states in the territories that were earlier under the rule of Rome, was of great significance for the progress of scientific and philosophical ideas in these states, which was often supported (and sometimes opposed) by the rulers of these states as they changed hands.It is enough to mention the scientific school of Bagdad, of the end of the 8th-9th centuries, which blossomed soon after the stabilization of the Caliphate of Bagdad and, the Samarkhand school of the first half of 15th century, during the rule of Ulugbeg. Thanks to the opportunity of quick assimilation of the heritage of Greek ideas and the very turn of thinking, the science of the Caliphate of Bagdad found itself in a situation that was much more favourable than the one in which science found itself in India and, even more compared to the situation in far off China. But an entirely wrong approach to Arab science, including Arab mathematics is prevalent till date; according to this interpretation Arab science is nothing but a transmission point between Greece and Rome on one side and the Europe of the middle ages and of the beginning of the modern times on the other. This conception was clearly formulated by E.Renan, more than hundred years ago, in 1863. It was he who put the expression"the Greek miracle" into circulation and, considered Arab science to be a reflection of Greek science, combined with the influences which came from Persia and India.

In defence of his thesis Renan adduced even philological considerations : he, and not he alone, thought that the Indo-European languages are more suitable for expressing abstract concepts, than the Semitic ones etc. However, Renan was not an historian of science; but his ideas were developed by such important specialists in the field as P.Tanneri and P.Duhem; these opinions are shared by some of the leading scholars even to-day. This Eurocentric conception of history of science, does not correspond to the actual course of scientific progress, and it has been criticised many a times in the Soviet, as well as in the foreign literature. What is special about Arab mathematics is this, that here we find a magnificent development of certain trends which originated in Greek or Indian mathematics and, some important advances in new directions. It would be enough to cite some examples :

The first systematic construction of the decimal positional arithmetic, the principles of which were, however, borrowed from the Hindus. Introduction of decimal fractions and the method of extracting the n-th roots, by using binomial expansions (11th-15th centuries); the different numerical methods for solving algebraic equations, and besides this, an example of approximate solution of a transcendental equation, with the help of successive iterations; extension of Diophantine analysis, solution of indeterminate linear systems, properties of the friendly numbers, Wilson's theorem (9th-10th centuries).

An original theory of ratios and proportions; extension of the concept of number to the positive irrational numbers; arithmetization of the ancient teachings on quadratic and biquadratic irrationalities (9th-13th centuries).

Isolation of numerical algebra together with the algebra of polynomials as an independent science; a developed geometrical theory of cubic equations and, a geometrical theory of the equations of fourth power (15th century, the corresponding treatise is yet to be traced).

Different theories of the parallels, connected with the attempts to prove the 5th postulate of Euclid (9th-13th centuries).

A reconstruction of the 8th book of the "Conic Sections" of Apollonius (9th century).

New quadratures and cubatures (9th-10th centuries).

In this list we have not specially isolated those trends and results, where the Arab mathematicians happened to be pioneers; it is enough to state that where they broke entirely new trails, they went considerably further than their predecessors. We have neither mentioned the names of these mathematicians, nor the names of those historians of science, who have of late elaborated or are continuing to elaborate upon the entirety of this vast complex of disciplines, theories and problems: the lists of either of them would be very large ; one may find these names in the corresponding literature. [See, for example : *Matvievskaya G.P., Rozenfeld B.A.*, Matematiki i astronomy musuilmanskovo srednevekoviya i ikh trudy (VIII-XVII vv.)/ Mathematicians and Astronomers of the Muslim Middle Ages and their works (8th-17th centuries) / ; in 3 volumes (479+650+372 pages), M. : Nauka, 1983. — Tr.]. But in view of the special importance of the question of evaluation of the role of Arab mathematics in the subsequent forward movement of mathematics — a question, which has already been touched upon — it is essential to dwell upon it. As we have noted above, Renan's evaluation of the issue, continues to find its supporters even in our time. B.L.van der Waerden sticks to a clear cut Eurocentric position. He came forward with the following sketch of the emergence of modern science, in a seminar held in Oxford in 1961 [25], while discussing

J. Needham's paper on science in China. He said, Newton's mechanics is the basis of modern science, wherein three threads remain intertwined — each of which has emanated from Greece. The first among them is the planetary astronomy, leading to Copernicus and Kepler, i.e. to the necessary prerequisites of Newton's mechanics. The second thread : Newton's use of Apollonius' conic sections and of the entire structure of the Greek axiomatic geometry, which served as the model for Newton's axiomatic mechanics. The third thread emerges from the Greek mechanics of Archimedes and of some other scholars. There exists no doubt about the enormous significance of the Greek heritage for the sciences of modern Europe, and that includes the work of Newton. But only the Greek heritage was not enough for it. The discoveries of Copernicus, Kepler and Newton are essentially based upon the Arab traditions of a highly developed new algebra and trigonometric system. The intertwining of the Greek and the Arab threads of scientific development and their subsequent creative synthesis in the mathematics, mechanics and astronomy of medieval Europe, had provided the necessary prerequisites here.Not all the attainments of the sciences of the Arab countries, were known in medieval Europe, and a lot of it had to be discovered anew. But often, even a fragmentary introduction of the results of the Arab investigations, served as points of departure for important trends in modern European mathematical thought. Such, for example, has been the case with the Arab theory of parallel lines, an acquaintance with which in the 17th century played an important role in the first stage of evolution of the non-Euclidean geometry.

All the same, if the sources of the sciences of modern Europe go back not only to Ancient Greece, but also to the countries of the East, and the terms "western science" and "eastern science" become admissible only with some stipulations — the mathematics of medieval Europe did have its own specificities, which were only very insignificantly, or not at all, characteristic of the other times or regions. Two of these had a very important or even determining significance for the formation of the mathematics of the modern period.

Here, first of all, we have in view, the creation and systematic perfecting of symbolic algebra, in the 13th-16th centuries. The timid steps taken in this direction, in the Moorish countries, are not going to be taken into account here : thanks to the march of world history, these steps could not be continued, and they failed to exert any influence upon the subsequent progress of mathematics. The formation of symbolic algebra was of immense significance for the entire further development of mathematics, and for developments beyond its boundaries; it was Leibnitz who first evaluated the role of symbolism in human thought. Idea- and time-wise, the progress of symbolic algebra came along with such attainments of the 16th century, as the solution of the equations of 3rd and 4th power into radicals and, the introduction of imaginary numbers. Here the mathematics of Europe broke an unbeaten trail and this led to results of truly universal significance for the entire system of physico-mathematical sciences.

Another characteristic specificity of the mathematics of medieval Europe was connected with the distinctive development of some ancient natural-philosophic and scientific ideas, which, to a significant extent, go back, on the one hand to Aristotle and his school, and on the other — to Pythagoreanism and to Plato. Here, medieval (European) mathematical thought went far beyond the boundaries of that elementary mathematics, which was then known in all the four regions considered in this paper. On the one hand, it was the programme of mathematization of the entire world of knowledge, put forward by the scholar from Oxford R. Grosseteste and his pupil R. Bacon, and together with it the development of experimental method and of the technical means of scientific investigations. On the other hand, it was the first, but already fully perceptible growth of the infinitesimal mathematics of a new type, elaborated first of all in the universities of Oxford and Paris (Sorbonne). An in principle new development of the infinitesimal ideas took place here, and then also in the other universities of Europe. Together with this renewal and the deepening of the ancient discussions about the nature of the infinite in both of its forms, of the potential and the actual infinity, continuity and discreteness etc., within a short while, as has been pointed out by N.Bourbaki, the foundations of a theory of change of magnitudes, viewed as functions of time, and of their graphic representation, were laid down — true, in a rudimentary form. The English (T. Bradwardine, R. Swineshead etc.) and the French (especially, N. Oresme) scholars of the 14th century made a bold attempt to quantify the basically qualitative natural philosophy of the Peripatetics, with the help of infinitesimal ideas. First of all, a new interpretation was given to those sections of Aristotle's physics, wherein the interrelations of force and motion and, force and resistance has been considered — and this turned out to be especially important for further developments; in other words, a reconstruction of the Peripatetic mechanics was undertaken; after that, all kind of changes of the continuous, and partly also of the piece-wise broken, measurable quantities or, in the terminology of the Peripatetics, the intensification — strengthening and remission — weakening of all kinds of "forms" or qualities, like heat, colour etc., as well as of goodness, sin etc. — the varying intensity of which were dependent upon their extensity — the spread of their intensity over finite or infinite intervals in space or time, were subjected to mathematical treatment. Here the simplest of mechanical movement, i.e. spatial displacement too belongs to the category of form. Generally speaking, not theological or ethical, but rather natural-scientific, and in the first place mechanical intensity, is at the centre of interests here.

A quite vast literature has been devoted to these theories of the Oxford and Paris schools, which were extremely close idea-wise, though coloured, so to say, in different tones (the English worked out their "calculations" at a more abstract-quantitative level; and the French "theory of widths and lengths of forms" made wide use of the graphic representations, which, however, were not alien to the "calculators"). The works of P. Duhem, published in the beginning of the 20th century, laid the foundations of the studies in this field; however, V.P. Zubov (1948) has convincingly demonstrated that Duhem was not at all impartial in his judgements. Here, once again, it is not possible to go into the details, and, by way of evaluating the teachings under consideration, it is enough to state that, therein we already find the formation of an idea of variability — flow (fluxus) of magnitudes, of momentary speed and accelaration, for which suitable, even Latin, terms were introduced and, the basic law and the other properties of uniformly accelarating motion were proved at an entirely abstract level, not connected with physics.

The calculations and the theory of widths and lengths of forms became quite widely known in the 15th and 16th centuries, first through manuscripts, and then through printed publications and, in the university-level teaching of a number of countries. During the last quarter of a century, a very large number of investigations have been devoted to this trend of medieval European mathematics, and this includes the publication of many manuscripts; together with this, the infuence exerted by this trend upon the formation of the "mathematics of variable quantities" (A.N. Kolmogorov's term) in the 17th century — on Galileo, Napier, Barrow, Newton and his school, and very likely also on Descartes and Leibnitz — has been studied in greater detail. Of course, there has been no mention of the beginnings of analytical geometry and of infinitesimal analysis, of laying their foundations, in the schools of Oxford and Paris; at issue here are the anticipations of and the ideational preparations for the just mentioned sections of mathematics, the foundations of which were laid in the 17th century. In particular, such ideational connections are evident in the terminology of Newton's" method of fluxions" and, in the fact that till date, in different languages, we use the expression"flowing coordinates" (the terms "variable", "function" and "coordinates" were introduced by Leibnitz). Till date, the works of V.P. Zubov (1962 and 1965) remain the best Russian work in this field (the second book was published posthumously) but, naturally, the results of the later investigations could not be included in them.

Modern Age. In what follows, we shall be providing an even more fragmentary survey of the changes, that our notions about the historical past of mathematics have undergone, and we shall be illustrating it with only a few examples. Choice of the examples will be, to a considerable extent, connected with the recent archival investigations and, they are aimed at showing how these investigations are important for making our knowledge, not only of the medieval mathematics, but also of the mathematics of the modern and recent times, more exact.

One of the greatest events in this area has been the publication of the eight volumes of the mathematical manuscripts of Newton, edited by D.T.Whiteside and his colleagues (1967-1981). This edition contains excellent commentaries. It has fundamentally changed our ideas about the scientific career of Newton and about the chronology of his discoveries. We have also come to know of those of his discoveries, which remained unpublished in his lifetime, owing to various, and not always clear, reasons. Thus, Newton discovered the expansions of Taylor and MacLaurin; he was the first to attempt an axiomatization of the method of fluxions; he proposed remarkable examples of asymptotic serial expansions etc. Almost simultaneously, A.R.Hall brought out a 7-volume edition of the complete correspondence of Newton (1959-1977). Here. V. Boss' book on the spread of the ideas and discoveries of Newton in 18th century Russia (1972), deserves special mention.

The work on the scientific legacy of Leibnitz has been less successful; often his manuscripts are found to be chaotic in character and can be read only with great difficulty. [There are nearly 75000 separate works of Leibnitz, preserved in the Leibnitz Archives of Hanover, and many of them remain unpublished till date. On this see : Katolin L., "Mee byli togda derzhkimi parnyami ...". M., "Znanie", 1979, p. 70. — Tr.] Study of Leibnitz's legacy began long ago, but, in spite of many interesting results obtained so far, the principal work remains ahead of us. Some of the important relevant publications in the field are : the 1st volume of Leibnitz's mathematical, natural scientific and technical correspondence, pertaining to the period 1672-1676, published by I.E.Hofmann (1976) ; the same Hoffmann prepared a detailed name index to the entirety of Leibnitz's correspondence (1977) ; E. Knobloch published a dialogue by Leibnitz, which contained, among other things, the first clear expression of the idea of multidimensional space (1976); three volumes of the publications and investigations of E.Knobloch (1973-1980) are devoted to Leibnitz's works on combinatorics and theory of determinants; the two volumes of the papers read at the Leibnitz- seminars of Hanover, 1966 (1969) and Paris(1978); and the complete chronicle of the life and work of Leibnitz, prepared by K. Müller and G. Krenert (1969). Some of the published works in the field deal with Leibnitz's treatment of the problem of foundations of the differential calculus; here, the emergence and development of non-standard analysis has led to a re-assessment of the earlier evaluations of the relevant contributions of Leibnitz; this holds good for Newton too. Finally, one must mention A.B.Steekan's (1952) investigations on the first analog integrators, inventd by Leibnitz and Newton, as well as by Ch. Huygens and Joh. Bernoulli.

Here one must also mention the deep-going investigations on the history of Jacob Bernoulli's work on the theory of probabilities — based, to a considerable extent upon archival materials — and the publication of his fundamental work in this field of mathematics the "Art of Suppositions"(as well as those related to the early stage of the spread of his ideas), conducted by B.L.van der Waerden, K. Koli and Yu. Henin (1975). Ya. V.Uspensky's Russian translation of the basic theoretical part of this work, which was subjected to deep going analysis by A.A.Markov (1913), has been reissued recently with Yu. V. Prokhorov's and O.V.Sheinin's commentaries (1986); A. Hold (1984) and the present author (1986) too dealt with a number of related questions.

Limiting ourselves only to the archival legacy of the most oustanding mathematicians, we must now go over to L. Euler. Here a great amount of work has been done involving close cooperation among the scholars of USSR, GDR, France and Switzerland. In view of the variety of the work done, we shall have to limit ourselves to listing the basic publications. These are : a complete description of the materials pertaining to Euler preserved in the archives of AS USSR (1962) and a part of his scientific diaries, a complete description of the materials preserved in the archives of AS GDR (1984), 3 volumes of Euler's letters to the Peterburg Academey of Sciences from Berlin (1959-1976), an annotated index of the complete correspondence of Euler — published in Russian (1967) and, a considerably supplemented German edition of it — published as volume I of series IV of the Complete Collected Works of Euler (1975). Volume V of this series contains the correspondence with Clairaut, d'Alembert and Lagrange (1980), and Volume VI — the correspondence with Maupertuis and Freidrich II (1986). So far, volume IV has been issued in another edition, which contains the correspondence with Goldbach (1976). All these volumes are being supplied with a large apparatus of commentaries; the publication of series IV continues. These fuller studies of the archival materials pertaining to Euler and their publication has been connected with some memorial years related to his life — 1957, 1982 and 1983 [L. Euler was born in 1707 and he died in 1783, thus his 250th birth anniversary fell on 1957, 275th birth anniversary — on 1982 and, 200th anniversary of his death — on 1983. — Tr.]and with the holding of various conferences and meetings, as well as with the publication of some collections containing the papers read in these conferences and the papers specially written for these collections. The sum total of all this work has given rise to a much deeper awareness about the works and science-organizational activities of this great mathematician of the 18th century and of a number of his outstanding contemporaries, about the scientific contacts between the Academies of Sciences of Peterburg, Berlin and Paris, as well as those among the other scientific collectives and educational institutions. We should add here, that the publication of some of the volumes of series II of the Complete Collected Works of Euler, in particular of his works on mathematical physics (1960) and, on the theory of ship (1978), have also considerably enriched our knowledge of the mathematics of 18th century.

The number of new books on the history of mathematics of the 19th and 20th centuries considerably surpass all the above mentioned and related publications. In what follows, we shall be limiting ourselves to brief comments on some of the newer publications on the histories of mathematical analysis, theory of sets and, theory of functions of the real variable.

Thus, the manuscript entitled "Dissertations on the Theory of Mathematical Infinity", presented in 1785 by L. Carnot in the competition organised by the Academy of Sciences of Berlin, anounced at the initiative of Lagrange, has been published (1970,1979). At that time Lagrange was the Director of the Mathematics Class of the Academy and, he did not approve Carnot's manuscript, and that is why it remained unpublished. But it is undoubtedly an interesting work. This was the first variant of Carnot's widely known"Meditations on the Metaphysies of Infinitesimal Calculus" (1797, 2nd ed. 1813). Carnot's basic idea — substantiation of analysis upon the principle of compensation of errors, dating back to G. Berkeley — is one and the same in the manuscript and in the published version, but the "Dissertation" contains some important moments anticipating Cauchy's reform: an understanding of the infinitesimal as a variable, of the connection between this concept and the concept of limit, and even an attempt at synthesizing the theory of limits together with its"strictness" and the infinitesimal calculus along with its algorithmic attainments. It is true that Carnot could not succeed here, and all this was successfully done by Cauchy. New light has also been thrown upon the so-called theory of limits of d'Alembert. This name does not quite exactly reflect the role of d'Alembert in the elaboration of the theory of limits : he was rather a successful propagandist of this theory, which was still in need of some important specifications and development; N. Bourbaki's evaluation of d'Alembert's definition of the limit as "very clear", is an overstatement. Lhuilier's book — written under the influence of d'Alembert, but providing a broader treatment of the concept of limit, as has been noted by E.S.Shatunova (1966) — won the prize in the above mentioned competition.

Archival searches have also thrown a new light upon some aspects of the work of Cauchy. The beginning of the proof sheets of the second part of his famous course of lectures in the Polytechnical School has been found, and this has finally enabled us to date the proof of Cauchy's famous theorem about the existence of solutions for the system of first order differential equations, and at the same time to explain the reason impeding the publication of this second part, and namely the differences of Cauchy with the then more influential professors of the Polytechnic, on the question of level of teaching of analysis in the Polytechnic (K. Jilen, 1981). The materials available in the archives of Paris have also enabled us to specify the exact nature of M. V. Ostrogradsky's and V. Ya. Bunyakovsky's participation in the elaboration of Cauchy's theory of residues, at its early stage (1824-1826) and in its first applications in mechanics and in the theory of heat (investigations and publications of the present author and V.A.Antropova, 1965; V.S.Kirsanov, N.S.Ermolaeva, 1985). Here, one of the manuscripts of Ostrogradsky, presented at the Academy of Paris in the beginning of 1826, deserves special mention : herein Ostrogradsky's famous integral formula has been formulated and proved for the first time and thereby his priority has been finally established — it has at times ben subjected to doubt; here we find the first generalisation of the method used by Fourier, during 1807-1822, for solving the problem of propagation of heat, wherein the ideas of D. Bernoulli and Euler were developed. Subsequently, the author of this work presentd it in a somewhat revised form, to the Peterburg Academy of Sciences in 1828 and, it was published in the Transactions of the Academy in 1831. In 1827 Ostrogradsky submitted another memorandum to the Academy of Paris — on the propagation of heat in the right prism, having an isosceles right angled triangle as its base. He conveyed his solution to G. Lame, who published his own enunciation of it in 1861, but the Russian translation of Ostrogradsky's memorandum was published only in 1965. Yet another valuable archival material, to some extent close to the one mentioned above, namely, the notes of Ostrogradsky's lectures on the theory of definite integrals, read in the years 1858-1859, in the hall of the Engineering Academy, has been published by V.I.Antropova (1961). Here many special integrals have been computed with the help of Cauchy's theory of residues and an originally enunciated theory of multiple integrals.

In this connection, here it must be mentioned, that in the 19th and early 20th centuries courses of differing volumes on the theory of definite integrals — computed this way or that, when the corresponding prototypes were not elementary functions or their superpositions — were read in many universities. P.L. Chebyshev read it for a number of years in the University of Peterburg, as an introductory course, together with the calculus of finite differences. Recently, N.S.Ermolaeva found the complete notes of the course on theory of probalilities, read by Chebyshev in the years 1876-1878, including both the introductory parts, and she read a paper on it in the International Congress of the Bernoulli Society. This paper is being published in the proceedings of the Congress, and the manuscripts of the said lectures are being readied for publication.

The place of honour in the elaboration of the foundations of the mathematical analysis of 19th century belongs to B. Bolzano, who largely anticipated Weierstrass, Dedekind and G. Cantor — both in his general conception and in a number of concrete results. His remarkable "Studies on the Functions" remained in manuscript form for nearly a hundred years, and was published by K. Rykhlik only in 1930, and from among the works published in his lifetime, special mention must be made of the brochure, containing a "purely analytical" proof of the theorem about the intermediate values of a continuous function — which too failed to draw the attention of the leading mathematicians immediately. There was a gap in Bolzano's proof : the theory of real numbers was not enough for its completion. It has been found out comparatively recently, that evidently Bolzano himself noticed this gap. In any case the text of his elaboration of the theory of real numbers has been preserved; it predates the constructions proposed later on and independently of each other by Weierstrass(1860), Mere (1869), G. Cantor and Dedekind (1872). K. Rykhlik published this text in 1961; he is of the opinion, that Bolzano's theory, which is not quite clear and complete, may be brought up to the level of modern requirements of strictness, without substantial changes; on this all the specialists are not in agreement with him.

Bolzano was not only a predecessor of Weierstrass and Dedekind in the realm of ideas, but it appears — as has been shown by P. Dugak (1973) — that he influenced both of them.Weierstrass set forth his classical system of mathematical analysis, as well as the theory of analytical functions in his Berlin lectures. Weierstrass' published some isolated new results contained in the lectures, but did not publish the course of lectures, as he was not satisfied with what he attained. Weierstrass' lectures were collected by a diverse international audience, but their contents were spread even wider, thanks to the printed versions prepared by some of his listeners. In 1973 P. Dugak published highly interesting notes of 4 courses of lectures read by Weierstrass during the years 1861-1887, and the corresponding correspondence of Weierstrass with a number of outstanding mathematicians. These materials substantialy helped specifying the stages of development of the foundations of classical analysis of Weierstrass, as well as of his system of theory of analytical functions. Of no less importance is P. Dugak's book on R. Dedekind, published in 1974. Archival materials constitute half of this book. These materials are related to Dedekind, one way or the other; these are : his manuscripts, correspondence (in particular, with G. Cantor) — in part, not contained in the collection of their correspondence published by J. Kavaies and E. Neter, letters of many other leading mathematicians etc. Dugak considerably specified the interrelationship of Canotr's and Dedekind's set theoretic ideas and the significance of their respective contributions to set theory, in the light of the subsequent logico-mathematical investigations right upto K. Gödel and P. Cohen. J. Dieudonné briefly formulated the general conclusion in his forward to this book by Dugak : Cantor's early work on countability, real numbers and topology remain his living and fundamental legacy, and in these fields Dedekind shares with him, in equal measure, the credit for laying the "set theoretic" foundations of the mathematics of our times. Unfortunately, P. Dugak failed to mention F.A.Medvedev's contributions in the elaboration of the history of set theory, who made a detailed and objective study of the connections between G. Cantor's and Dedekind's set theoretic investigations, in a book published in 1965, it is true, however, that he did not have at his disposal all the archival materials, brought into circulation by Dugak, who mentioned Medvedev's book only in passing. It is well known that set theory and theory of functions were developed vigorously in our country. The first shoots of the Moscow school of theory of functions, often called the school of D.F.Egorov and N. N. Luzin, date back to the beginning of this century. Hence, the interest in the life and work of these scholars and of their colleagues and followers, is quite understandable. F.A.Medvedev was the first (1959) to seriously study the given stage of the Moscow school. Subsequently, many archival materials, pertaining to the life and work of the pioneers of this school and of their followers, came to light : Egorov's letters to Klein, those of de la Vallée-Poussin to Luzin, Egorov-Luzin and Luzin-Danjua etc. correspondences, Luzin's preface to Euler-Goldbach correspondence, Luzin's opinions about the work of the famous geometer S.P.Finikov etc. Recently, new and important materials pertaining to the first formative years of the Moscow school of theory of functions have come to light; these are related to : the course of lectures on the theory of functions delivered by V.K.Mlodzievsky in the beginning of the 20th century, the papers read at the students' circle — where P.A.Florensky was one of the most active members and where Luzin took part, the discontinuous functions studied there etc. What is more, now new light has been thrown upon the role of N.V.Bugaev, whose philosophico-mathematical ideas clearly stimulated the interests of the younger scholars and students in the theory of functions. Herein also comes to light the connections with the Moscow school of philosophy. S.S.Demidov's, F.A.Medvedev's, A.N.Parshin's and other publications pertaining to these connections came to light during 1985-1986. In this connection one must mention the analysis made by the Colombian scholar R.K.Arboleda(1980) of the letters to M. Fréchet from P.S.Alexandrov and P.S.Urison — who founded the Moscow school of topology in the begining of the 20s of this century. Reminiscences of P.S. Alexandrov were published during the years 1971-1980. These are not archival materials, but we should not pass them over in silence, since here we find a brilliant description of mathematics in Moscow with the global developments of mathematics in the background, from the middle of the first decade of this century and for a stretch of more than sixty years. D.E.Meinshov's reminiscences of his student days and of the early years of his scientific career (1983), constitute a valuable supplement to the memoirs of Alexandrov.

Scientific correspondences were of first order significance during the 17th-18th centuries, when scientific periodicals were extremely weakly developed, scientists rarely met one another, lived in different towns and, scientific congresses and conferences did not take place at all. These correspondences did retain their place in scientific life even later on, right upto our times, though to a lesser extent and, to the examples just cited, here I shall mention only three : the correspondence of S.V.Kovalevskaya with G. Mittag-Leffler — edited by P. Ya. Kochina and E.P.Ozhegova — important, not only for the biography of this outstanding woman and mathematician, but also for investigations into the life of the international mathematical community during the 80s of the last century; the correspondence between V.A.Steklov and A.Knezer, in the beginning of the 20th century, on questions of mathematical physics and related themes — published by I.I.Markush and others (1980); and the correspondence of A.A.Markov with A.A.Chuprov, 1910-1917, on questions of probability theory and mathematical statistics, prepared for publication by Kh.O.Ondar (1977).

Conclusion. This survey of the historico-mathematical investigations of the last few decades, is far from complete; but even this survey shows, that these investigations have not only considerably supplemented our knowledge of the past of mathematics, but that they also entail considerable changes in our general notions about the characteristic traits of the developments of mathematics at different times and in different directions. This survey was divided into several points; in each of them a corresponding summing up has been provided with and, some of the open problems have been indicated. Now a few words remain to be said about some of the over-all changes in that retrospective, wherein the developments of the last four or four and a half thousand years of developments of mathematics had been presented until recently; it is about these years that we may speak sufficiently confidently. At issue here is the further specification of the periodization proposed by A.N.Kolmogorov [26]. It is true, that the periodization proposed here is global in character and, does not pretend to provide the universal characteristic traits of the objects and methods of mathematics at each of the indicated periods, thanks to the unevenness and inexact synchrony, and sometimes owing simply to the non-synchronous progress (at times regress) of mathematics in the different regions or sub-regions considered [26]; the same is true of the present survey. Having this stipulation in view, such global periodization of mathematics, understood as a single science, without its division into sub-disciplines, appeared to be fully satisfactory for a long time. Briefly speaking, A.N.Kolmogorov, made a distinction among four large periods:

1. Birth of mathematics, as it took place, for example, in Egypt, where, evidently, mathematical theory — in the sense of proofs of general theorems — did not exist at

all, and everything was reduced to the collection of arithmetical and geometrical examples of practical importance, solvable at the level of simplest concepts, according to some prescriptions and rules for computation and measurement; however, perhaps, in Ancient Babylon the prescriptions were not confined to what was of direct practical necessity and, there arose more abstract scientific interests and they evolved some general algebraic methods for solving a number of problems (here, we have, almost word for word, reproduced A.N.Kolmogorov's expressions, from his article in the second edition of GSE, v. 26, 1954).

2. The elementary mathematics of the period 7th century B.C.-17th century A.D., when it became theoretic, having the constant magnitudes of arithmetical or geometrical nature as its main object ; approaches to the ideas of an infinitesimal analysis are observed in Greece and then in medieval Europe, but they were not developed; this second period has been divided into two sub-periods in Kolmogorov's article entitled "Mathematics" — encompassing, correspondingly, the ancient (Greece, Hellenism, Rome) and the medieval (countries of the Orient and of Europe) periods.
3. The period of emergence and development of the mathematics of variables, from the 17th century; it enters into [4.] the period of modern mathematics in the 19th century, in connection with extreme extension of the object of mathematics and the generalisation of its concepts and methods; A.N. Kolomogorov refrained from providing any direct global characteristion of this modern period. At the same time he has provided a fully intelligible account of the transition to modern mathematics, which began in the first decades of the 19th century, straight off in the two fields of geometry and algebra. His article began with Engels' definition of mathematics as the science of the quantitative relations and spatial forms of the real world. In the section devoted to modern mathematics Kolomogorov indicated that, the range of "quantitative relations" and "spatial forms" studied, becomes extremely widened in this stage (this is explained with the help of some examples) and that, when these two expressions are so widely understood, even then, that is even at the present stage of development of mathematics, its initial definition holds good. Here Kolmogorov adds that, when the expression "quantitative relations" is interpreted sufficiently broadly, then"spatial forms" may be considered to be special kinds of "quantitative relations". Here it is not possible to enter into a discussion of the wide range of related methodological questions that arise, for example, of A.D.Alexandrov's treatment of geometry (1952) as the science of spatial relations and forms, as well as about the other relations and forms of reality, which are structurally similar to the spatial ones("spatial-like"). B.A.Rozenfeld's proposal to generally call modern mathematics non-Euclidean, is hardly felicitous : this word is very closely connected with the non-Euclidean geometry. This entire periodization is linked with the periodization of the prevailing social formations.

Many Soviet historians of mathematics accepted A.N.Kolomogorov's periodization with some modifications. Now, let me summarize the specific comments regarding the periodization already made; let me stipulate again, that the terminology used here is conventional and, the chronological and territorial boundaries are diffused.

1. While retaining the term "the period of birth of mathematics", we should stress the fact that not only in Ancient Babylon, but also in Egypt — the prescriptive form of enunciation was followd by works reflecting deductive arithmetical, geometrical and algebraic thought; true, not at the level of construction of systems like the Euclidean geometry. Perhaps, the expression "piecemeal deductive mathematics", more adequately describes the mathematical thought of the aforementioned and similar civilizations.

2. In Greece mathematics was transformed into a deductive science, in the Euclidean — if one may say so — sense, i.e., into a system of disciplines, axiomatically constructed at the level of Aristotelian logic; it was not accidental that the formation of this logic was almost simultaneous with the codification of Euclid's "Elements". However, this axiomatization did not spread to all the branches of mathematical knowledge and, in the 19th-20th centuries it has been substantially completed and developed. But Greek mathematics is different from what preceeded it, also in another, in principle more important, respect — important from the point of view of subsequent development of mathematics : in natural philosophy and in mathematics there arose the idea of infinity and it found application — it became the starting point of infinitesimal mathematics; it is in Greece that the fruitful interaction of philosophy and mathematics was established. Thus, the term "elementary mathematics" is hardly adequate for the content of Greek mathematics. Perhaps, here the more general, though less concrete, name of "the period of emergence of mathematics as a lógico-deductive science", has some advantages.

3. Probably, the term "the period of elementary mathematics" is most appropriate , when one wants to provide a global characterization of the mathematics of the middle ages. But here it is important to have in view the essential traits of mathematics in Europe of 14th-16th centuries and the progress of the non-elementary ideas and methods spoken above.

4. In reality, the 17th-18th centuries are characterised, first of all, by the primacy gained therein by the mathematics of the variable magnitudes. The problem of naming the period of modern mathematics in terms of its contents, happens to be more complex. Perhaps, it would have been proper to borrow a term from modern mathematics, and speak of this period as the period of "non-standard mathematics". However, the question arises : would it be correct to speak of the last two centuries, as one single period of development of mathematics? Does not the mathematics of the recent times of scientific and technological revolution, with its characteristically fast progress of informatics and of the other noticable (discrete,finitary etc.) tendencies, constitute the first stage of a new period of development of this most ancient science ? Personally for me, it is very difficult to provide an answer to-day.

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NON-STANDARD ANALYSIS AND THE HISTORY OF CLASSICAL ANALYSIS

FEODOR ANDREIVICH MEDVEDEV

The history of classical analysis is perhaps the most investigated part of the history of mathematics, and this is quite natural. Mathematical analysis has been viewed as the "simplest and most universal language ... most suitable for expressing the invariable relations of natural phenomena" [1, p. XXIII]. That is why the greatest representatives of mathematical thought, and the most modest workers in the field of mathematics, applied their strength to its construction, from the 17th to the 19th century, as well as, during the greater part of the present century. This new discipline, in the process of its construction, not only served as a felicitous means for describing the phenomena of the external world, but also provided a possibility for advancing profound philosophical reflections about the differential picture of the world, about the causal connections in it, about the laws of nature and thought. That is why, historians of science have paid the greatest attention, namely, to the history of analysis.

A major specificity of the approach of the historians to the study of the formation of this branch of mathematics has been, and to a considerable extent still is, to view it as a single integral theoretical discipline. To a certain extent this view reflects an aspect of a more general notion, according to which "mathematics grew as a single whole" [2, p. 13].

However, of late such a view about mathematics has been shaken or has at least been questioned. "The basic conclusion that may be drawn from the presence of several conflicting approaches to mathematics is as follows : there exists not one mathematics, but many mathematicses" [3, p. 358] ; not only some individual mathematicians and historians of mathematics, but even some philosophers share this conclusion [4, pp. 186-187]. An analogous hypothesis suggests itself also in respect of mathematical analysis : it began to take shape after the construction of the intuitionist and constructivist analyses, and became especially clear after the creation of non-standard analysis, towards the middle of this century. Each of the systems mentioned, is sufficiently independent of, and definitely different from, any other of them — in terms of the composition of basic concepts, modes of advancing arguments about them and, computational procedures ; and it is hardly probable, what is more, impossible (and even if possible, then not necessary) that they be united in a single theoretical construction. This especially comes to the fore, when we view analysis, not so much as a theoretical doctrine about its basic concepts, but rather as calculus, as formal system. [Such an outlook on mathematical analysis has been considerably developed, at the level of historical studies, in a book [5] by C.H. Edwards.] In so far as, "it is possible to propose a large number of formal systems, for describing one and the same fragment of reality" [6, p. 87], there is no ground for preferring one of them in advance : it would be expedient to use different calculi in different situations.

A distinctive and even strange specificity of theoretical constructions — and not merely of the mathematical ones — is this, that at a definite stage of their construction, and at times from almost the very beginning, the adherents of the corresponding theoretical schemas become tempted to view them as the universal and the only possible schema and to declare all the others as false. Such, in particular, was the situation in the history of mathematical analysis, in the second half of the 19th century, when this mathematical discipline attained the highest level of its development and when the gloss of Cauchy-Weierstrassian rigour was put on it. The analysis of Cauchy-Weierstrass was considered to be the only legitimate theoretico-analytical construction, and all that preceded it came to be regarded as mere approximations to it, and largely mistaken at that : "... in the text-books of Cauchy at long last we are on firm ground" [7, p. 207].

The works on the history of analysis come out mainly as descriptions of the development of some integral, one and only possible thing, which served as a kind of ideal for a single science, to which the constructions of the previous epochs approximated. From the point of view of this ideal science (which is in practice reducible to a construction, put forward in some standard text book, followed by the author of the corresponding "history of analysis" or part thereof), the problems that emerged in the course of history had singular correct solutions; and it is these that were mainly of interest to the historians. Indeed the approaches to them could be different: to a large extent these are unsatisfactory and even patently wrong, and then, in the course of further searches they are usually rejected, or at best they are used as auxiliary devices, serving as the raw material, from which the ideal science was built, or they provide heuristic indications, which lead to the discovery of the absolute truths of the ideal science.

In particular, the "evolution of "rigour" in analysis can be summed up as a continuous ascent from unclear and vague concepts, their gradual elucidation, and then the arrival of a stage of stability — after which it was impossible to have *any* dispute regarding what constitutes a correct proof in analysis" [8, p. 50]. In the opinion of J. Dieudonné, the analysis of Cauchy-Weierstrass happened to be this stage of stability.

The analysis of Cauchy-Weierstrass took shape in the 19th century. However, before that mathematical analysis went through more than two thousand years of development — from the first quadratures and cubatures of the ancient Greeks to the analysis of Newton-Leibnitz, crowned with the works of J. L. Lagrange and L. Euler. Here we shall mention only some of the landmarks along this road.

It is well known, that in ancient Greece the first quadratures and cubatures were carried out with the help of some infinitesimal procedures [9]. These procedures were so fruitful, that even after the elaboration of the method of exhaustion by Eudoxus, which tended to exclude the infinitesimals from mathematical arguments and soon became the official doctrine on the corresponding questions, infinitesimal considerations continued to be in wide use, and among the users there were adherents of the method of exhaustion; the assertion of S. Ya. Lureo, that if the followers of the method of exhaustion "did not get hold of ready-made solutions, already discovered by the atomists, then they themselves preliminarily found them out — by stealthily applying [the method of] atomistic decomposition" [9, p.159], is best illustrated by the example of Archimedes.

With the renewal of interest in the problems of analysis in the 17th century, infinitesimal considerations again came to the fore (in the works of J. Kepler, B. Cavalieri, J.-P. Roberval and of many others), though the spell of the method of exhaustion also continued. It was considered to be an irreproachable, totally rigorous mode of mathematical reasoning in respect of the problems of quadrature, cubature, centres of gravity, maximums and minimums, and tangents; and mathematicians did not see in it those serious logical flaws, which may be observed today; for example, this, that therein, neither were the magnitudes under consideration defined, nor were their existence proved. Though this spell was retained for a long time, right upto A. Cauchy and even later, nevertheless the method of exhaustion more and more retreated to a secondary position. It was substituted by the *infinitesimal calculus*. Namely, thus was created the grandiose house of mathematical analysis — the most extensive and the most fruitful mathematical discipline.

Almost from the very beginning of its construction, strange reproaches began to be sounded regarding the illogicality of the arguments, involving the infinitesimals, provided in this calculus — these reproaches continue even to-day [3, pp. 151 - 177]. However, much more illogical was the very demand, that the modes of reasoning used in analysis are to be subordinated to the norms of existing logic. Mathematical analysis is based on the concept of function — which is a special instance of relation (and for an extended understanding of function, they are one and the same). Logic of relations did not exist earlier and it was constructed only in the second half of the 19th century. [It is true that individual advances were made in that direction. Such attempts date back to Aristotle, * G.W. Leibnitz, I.H. Lambert and to some others, but the construction of the theory of relations as a section of logic began only with A. De Morgan, or even later — with C.S. Peirce.] That is why, the arguments employed by the mathematicians in analysis, naturally, did not fit into the frame-work of the arguments conducted in accordance with the canons of older thought. Mathematicians did not like to wait till the emergence of the corresponding logic, in fact they paved the path for it with their new calculus, having constructed a formal system, which is a special instance of a large fragment of the logic of relations (this was done by Euler, Lagrange and their followers).

In the final analysis this formal system was infinitesimal, and Lagrange's heroic attempt at liberating mathematical analysis from the infinitesimals turned out ot be totally unsuccessful, in spite of his indisputable achievements. [In J. Grabiner [10] one finds a good description of many achievements of Lagrange.] What is more, even Cauchy's reconstruction of analysis carried the mark of that very infinitesimal. On this, one must dwell in greater detail.

In correspondence with the established tradition, earlier we have united the constructions of Cauchy and Weierstrrass into an aggregate. Indeed, there is much in common in their approaches, which provides a definite basis for such unification. But in essence the constructions of Cauchy and Weierstrass, differ by so many fundamental parameters, that it would have been more correct to speak of two different systems of analysis, created by them. This difference has been quite clearly outlined by N.N. Luzin [11, pp. 305-312], and in his words, generally speaking, this difference lies in the fact that, Cauchy "in principle, introduces variables, and, thus, since then analysis stands enriched by these new magnitudes, used equally rightfully with the constants, just as the imaginary numbers are used on a par with the real ones" [11, p. 305]; Weierstrass, however, "first of all removes all the variables from analysis, any change, motion and everything is reduced to the stationary conditions and to that alone, i.e., to the constant magnitudes" [11, p. 307]. These systems are also different in

* (to Gangesopadhyaya in India — Ed.)

terms of the explications of the concepts of infinity, which are foundational to them. Cauchy based himself upon the concept of potential infinity and, the concept of potentially infinitely small magnitudes are central to his system of analysis, whereas Weierstrass leaned on the concept of actual infinity, and he in fact drove away the infinitesimals from analysis. This is not the place for citing the other differences, many of them are yet to be brought to light; here we shall only mention the fact that Cauchy's conception completed the stage of infinitesimal analysis, whereas in Weierstrass' system, analysis was restructured as a set theoretic discipline; its contours have been quite clearly outlined by Luzin [11, pp. 307-312].

Infinitesimal considerations were put aside in the classical set theory, and the actual infinitesimals were denied every right to exist ; G. Cantor fought against them energetically — on this, see, for example, [12, pp. 294-296]. At the end of the 19th and during the first half of the 20th century, the Weierstrassian system of analysis, based on the theory of sets, became the ideal of analytical construction — it was considered to be singularly legitimate and absolutely true. Armed with it, even the historians of analysis started viewing the previous developments through the prism of such construction. Naturally, there arose all possible distortions. They are quite numerous, and there is no scope here, even for providing a simple list of them. We shall cite only a few examples.

After a very brief and not entirely objective description of B. Cavalieri's methods of quadrature and cubature, O. Teoplitz termed his conclusions — "clever" [13, pp. 55-58], but there itself he characterized them as "untrustworthy to the highest degree". After that he admitted, that in the 17th century the doctrine of indivisibles was "lauded to the skies" and was employed in various forms by P.Guldin, B. Pascal and J. Kepler and, that in the works of G.W. Leibnitz the various trends in the use of the infinitesimal methods came together, "as rays converge in the focus of a lens" and illumined the entire 18th century. Teopliz concludes that L.Euler, D. Bernoulli, B. Taylor and others "constructed the new edifice of mathematics basing themselves upon such non-strict and heuristic methods " [13, p. 59]. Thus, it appears that a large mathematical theory was built upon the precarious foundations of doubtful heuristic methods, and only a "clever instinct" saved Cavalieri and, of course, the other mathematicians from making false moves.

While describing the mathematical analysis of the 18th century, J. Dieudonné, on the one hand, calls Euler one of the two "giants of that century" [14, p. 20] (the other one being Lagrange), and on the other — presents him as nearly the greatest fumbler : he did not define the concept of a "continuous" or "regular" function "more precisely"; did not give an "exact definition" of his "mechanical functions"; his conception of numerical serieses was "shaky and imprecise"; he "could not formulate" the definition of the concept of the sum of a series, basing it on the concept of limit, and though "he knew well, that when the general term of the series

$$a_0 + a_1 + \cdots + a_n + \cdots,$$

does not tend to zero, then one should not speak of the "convergence" of this series in the usual sense, nevertheless he thought that it was often possible to calculate the "sum" of this series" [14, pp. 21-22]; Euler "did not make a clear statement" about one of the divergent serieses; "Euler's interpretations of the different meanings of the word "sum" of a series appear to be very confused, leading to contradictions", "Euler did not at all understand the difficulty inherent in his definition of the "sum" of a series" [14, p. 23]; Euler "did not always remember what he wrote a few years back", "he did not elucidate the phenomenon of non-uniform convergence, when he ran into it" [14, p. 30], etc.

In short, it appears, that only with Cauchy did the mathematicians — to use an expression of Bourbaki, indicated above — find themselves "on a firm ground", and till then they wallowed "in the quagmire of mathematical analysis" of the Newtonian- Leibnitzian-Eulerian kind [3, p.151], and Cauchy too, quite often fell into it.

Demolition of such mistaken ideas about the development of mathematical analysis began after the construction of the so-called non-standard analysis, in the middle of the present century. [The construction of this analysis is to a great extent connected with the name of the American mathematician A.Robinson [1918-1975], and his work published in 1961 [15] is considered to be the first work in the field. This is not entirely correct. Even if we refrain from mentioning its pre-history, which dates back to the 19th and the first half of the 20th century, we may mention, for instance, the work of C. Schmieden and D.Laugwitz, published in 1958 [16] and the papers of the latter : [17] , [18]. A.F. Monna began working in this direction [19, p. 4], since 1950.] The sole fact, that the infinitesimally small magnitudes and numbers, that were assiduously driven away to build the Weierstrassian analysis, again got back their right to citizenship in mathematics, itself compels us to re-examine the ideas so far formed, about the course of development of analysis upto Cauchy and Weierstrass, about the character of its infinitesimal apparatus and, about the modes of reasoning employed therein. Now the infinitesimal procedures are no more considered to be "non-strict, heuristic methods", but are rather viewed as quite honest means of advancing mathematical reasoning, which not only enable us to re-state the well-known results with great simplicity and elegance, but also help us discover new results; as an example of the latter, one may mention the so-called solution-configurations for a determinate class of ordinary differential equations [20].

This re-examination still continues. It began with the efforts of the founders of non-standard analysis — Laugwitz (1965) and Robinson (1966) [21 & 22]. Now the front in favour of such re-examination is quite broad, and the mere listing of the works in the already indicated direction, would take a lot of space. In particular, many results of Euler. so far considered to be "non-strict" and to have been obtained by doubtful means, turned out to be rationally interpretable within the frame-work of these new ideas [23 & 24]. Now they are no more the products of some special and secret intuition, but are provided with a rational reconstruction in a different system of analytical thought. Now there has even arisen a danger, of too direct an interpretation of some isolated notions of Leibnitz, Euler and of some other mathematicians, of their unnecessary modernization in the spirit of non-standard analysis. W. Felsher has characterized this danger quite clearly [25, pp. 179-180]. However, one may state — by generalizing, to some extent, Laugwitz's understanding of some of the results of Euler [23, p. 4] — that, we must examine the attainments of our predecessors from the modern points of view, while keeping in mind the fact that they could not have thought, namely in the way we do; all the same, we can rationally interpret what they did, namely, with the help of modern ideas.

Further, one of the basic principles of non-standard analysis is the principle of transfer / translation, according to which every true statement of the classical analysis (the classical theory of sets) is true in the non-standard analysis (non-standard theory of sets) too, and conversely [see : for example, [20, p.81]; and see : [26, pp. 202-203] for a more detailed formulation]. Thus, non-standard analysis appears to be yet another model for that very class of analytical phenomena and relations among them, for which the model of classical analysis was built. It is understandable, that in an atmosphere of supremacy of the classical model, the latter played the role of a paradigm in the sense of T. Kuhn or, that of a research programme in the sense of I. Lakatos, and the historians of analysis examined the results obtained by their predecessors through the prism of the basic propositions of this paradigm or programme. In particular, since in it [the classical model] there was no room for the actually infinitesimally small, so all manipulations with them, met with by the historians, were viewed with suspicion, and the data obtained with their help, were interpreted as to have been found non-rationally — intuitively or through the mediation of unreliable heuristic methods. And this led to the, not quite correct, prevalent interpretation of historico-scientific facts, herein indicated (and largely, not even indicated).

After the construction of non-standard analysis, classical analysis of the 19th century ceased to be the only correct analytical construction, its universalization and absolutization appeared to be illegitimate forms of activities. At least two formal systems possessing equal rights turned out to be equally suitable "for describing one and the same fragment of reality". In its turn, the notions about some mythical "absolute strictness" — turned out to be a myth. That is why there arose the necessity of substantial corrections in the historiography of mathematical analysis — corrections, that have been made complicated owing to the presence of intuitionist and constructivist analyses.

Everything said and done, non-standard analysis permits us to give an answer to a ticklish question, which arose in connection with the earlier approaches to the history of classical analysis : if we admit that the infinitely small and infinitely large magnitudes are contradictory concepts, then how could the grandiose edifice of one of the most importanmt mathematical disciplines be built upon them? And H.J.M. Bos — one of the keenest of modern historians of analysis — has been compelled, inspite of certain prejudices against the non-standard analysis, to declare that "the recently constructed non-standard analysis provides an explanation, as to why analysis could develop upon the precarious foundations of the infinitely small and infinitely large magnitudes" [27, p. 13].

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THE NEW STRUCTURAL APPROACH IN MATHEMATICS AND SOME OF ITS METHODOLOGICAL PROBLEMS

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The structural approach in mathematics has found its most complete development in the works of a group of French mathematicians writing under the pseudonym of Bourbaki, and of late in the works of MacLane and, of the other scholars engaged in the elaboration of the algebraic theory of categories. This approach offers an opportunity to take a new look at many major problems of philosophy and methodology of mathematics. The most important among these problems are : the specificity of the object and method of mathematics, the place of mathematics in the system of scientific knowledge and, the relation of mathematical structures with objective reality.

The general problems of the application of mathematics in the other sciences and in practical activities become more clear, when they are veiwed from the point of view of abstract structures. The question of the interrelationship of "pure" (theoretical) mathematics with applied mathematics simultaneously finds a more thoroughgoing solution.

1. FORMATION OF THE CONCEPT OF ABSTRACT STRUCTURE AND THE EMERGENCE OF A NEW APPROACH TO MATHEMATICS

Progress in mathematics has always been connected with the growth in the abstractness of its concepts and theories. Modern mathematics uses ever deeper abstractions to study, not only the quantitative, but also the more complex structural relations; the traditional quantitative relations among magnitudes happen to be a constituent part of these more complex relations.

The beginning of this new approach to the subject-matter of mathematical investigations is, to a considerable extent, connected with the discovery of non-Euclidean geometry by N. I. Lobachevsky and J. Bolyai. It is difficult to overestimate the general scientific and theoretico-cognitive significance of this discovery. It not only undermined the centuries old belief in the possibility of one and only one geometry of Euclid, but also fundamentally changed our earlier notions about geometry, about mathematics as a whole. Above all, the theoretico-cognitive lesson offered by the discovery of the non-Euclidean geometries consisted in the following : it convincingly demonstrated that the axioms of geometry are neither empirical descriptions and inductive generalizations of the properties and relations of the real physical space, nor are they *a priori* synthetic judgements — as I. Kant thought them to be.

Mathematicians, following B. Riemann, began to view these axioms as hypotheses of a kind, whose applicability to the study of the surrounding space must be established after providing suitable interpretations for the basic concepts of a geometry. Within the framework of mathematical investigations these concepts (of a point, straight line and plane) themselves remain as abstract as, say, the algebraic formulae. No one doubted the fact that the symbols of these formulae may indicate any number, and subsequently any vector, matrix, function or other object. However, for a long time the statements of geometry remained associated only with the properties and relations of physical space. It is that is why, namely, that the discovery of the non-Euclidean geometries had such a decisive significance for the emergence of a new approach to the subject-matter of geometrical investigations.

In so far as axioms can describe the properties and relations of objects having the most diverse concrete contents, we cannot pass judgements about their truth or falsity, basing ourselves upon any one system of objects, serving as their interpretation. Besides, intuitive self-evidence also cannot serve as a criterion for truth, since that which is self-evident to one, may not appear to be self-evident to another. That is why, the demand that the axioms should be intuitively self-evident is not a mathematical, but rather a psychological demand. From the logico-mathematical point of view, the most important criterion, which must be satisfied by any system of axioms, is the simultaneity or formal non-contradictority of the system of axioms.

If a system of axioms is contradictory, it will not have any interpertation and, consequently, will have no scientific worth whatsoever. The demands for completeness and independence of a system of axioms are not that obligatory, if only owing to the fact that a dependent axiom can always be translated into a class of theorems, and the criterion of completeness is applicable only to the comparatively simple axiomatic systems.

The transition from the concrete, contentful axiomatics like, for example, the axiomatics of the Euclidean elementary geometry, to the abstract axiomatics like the axiomatics of Hilbert, and then on to those fully formalized axiomatics, wherein symbols substitute terms, and propositions are transformed into folmulae — is quite a new step in the development of the axiomatic method; it is sometimes called a revolution in this method. As we have noted above, it is, namely, this approach which provides an opportunity for viewing axioms as abstract forms ; these forms may be used for investigating the properties and relations of various things that differ in their concrete contents.

In the formation of the ideas about abstract structures, a significant role has been played by the set theory, which emerged towards the end of the 70s of the last century. This theory was crafted, in the main, in the works of the great German mathematician G. Cantor, directed at providing a satisfactory foundation to classical mathematics. This theory views the objects of all mathematical theories like the number, function, vector, matrix etc., in isolation from their mathematical contents. For the set theory these are but elements of such infinite sets, which may be handled with definite rules. Such an extremely general and abstract approach provided an opportunity for viewing the subject-matter of the most diverse mathematical disciplines from a single point of view. Namely, that is why, with the passage of time the set theory came to be looked upon as the foundation of the entire house of classical mathematics.

In the end, a synthesis of the set-theoretic ideas and the axiomatic method led to a new conception of the abstract mathematical structure. This conception was floated towards the beginning of this century. "One is tempted to acknowledge that the modern concept of "structure" was in the main formed around the year 1900; actually, it took another thirty years of study to make it fully clear" — wrote N. Bourbaki [5, s. 33]. This work was done by that talented team of French mathematicians, which uses the collective pseudonym of N. Bourbaki. They characterize a structure as follows: " In order to define a structure, one or more relations, containing their elements are at first specified... then it is postulated that the given relation or relations satisfy certain conditions (these are listed and they constitute the axioms about the structure under consideration). To construct an axiomatic theory of a given structure, is to deduce logical conclusions from the axioms about that structure, without admitting any other presupposition in respect of the elements under consideration (in particular, staying clear of any kind of hypothesis regarding their "nature" [5, s. 25].

From this, it is evident that concrete interpretations of the objects of mathematical investigations are quite unimportant for the said investigations. One can view these objects as elements of abstract mathematical structures, all the essential properties of which are specified with the help of axioms. N. Bourbaki have classified the mathematical structures into three basic types, in accordance with the character of these properties; these are: the algebraic, order, and topological structures. More complex or multiple structures are formed by combining these initial structures. The structure of various mathematical theories may be studied and thereby they may be classified, with the help of these more complex structures. If earlier mathematical theories were simply elaborated one beside the other, in the course of their historical emergence, then now it is possible to reveal their deeper resemblance. Thus, for example, the set of natural numbers, which serves as the basis for the construction of mathematical analysis, contains all the three generative structures and hence, the earlier isolation of algebra, geometry and analysis turns out to be groundless.

Of late the algebraic theory of categories is drawing ever greater attention of the specialists. Before the emergence of this theory, the set was considered to be the most general concept of mathematics. That is why, while substantiating mathematics, all other mathematical concepts were sought to be defined by using the terminology of set theory. Even the conception of N. Bourbaki was no exception to this, based as it is, in the final count, on an axiomatic theory of sets. The concept of category is not only an alternative to the concept of set, it is also the further concretization and development of the idea of mathematical structure.

A category is made up of a certain class of objects and a definite class of morphisms, wherein each ordered pair of objects is contrasted with a corresponding set of morphisms. An operation with the morphisms, with the help of which from two given morphisms of a category, a third unique element is found out from among the set of morphisms, is called the composition or production of morphisms. It must satisfy the conditions of associativity and identity [4, s.9-11]. Simply speaking, in the theory of categories the sum total of objects are considered together with their structure and with some representations among them, retaining the given structure. If sets are viewed as the objects of a category, and representations among them serve as morphisms, then we get the category of sets. Thus, the concept of set turns out to be a special case of the more general concept of category. In contrast to the static concept of a set, here the principal attention is turned towards the character of representations, which retain the definite structural specificity of the objects, and thereby the active, constructive aspect of mathematical knowledge is underlined.

2. THE ROLE OF ABSTRACT SRUCTURES AND CATEGORIES IN THE MODERN UNDERSTANDING OF THE SUBJECT MATTER AND METHOD OF MATHEMATICS

The ideas and methods of the theories of structure and category provide a better opportunity to understand the qualitative change, which is taking place in the very subject-matter of mathematics, as well as in the application of its methods in the other sciences.

Upto almost the middle of the last century mathematics was viewed as a science of magnitudes and spatial figures. Of course, therein not the concrete physical, chemical etc. properties of the magnitudes, but the properties and relations common to all of them were taken into consideration. Since every magnitude can be expressed numerically with the help of a suitably chosen unit of measurement, in the past, often the essence of mathematics was considered to be located in the investigations regarding the properties of and dependencies among numbers [7, s. 15]. The study of spatial figures in geometry was also, in the main, limited to their metric properties. And though by the middle of the last century, there did exist in mathematics such theories and separate disciplines, wherein the questions of measurement did not play any important role (for example, in the projective geometry, group theory etc.), nevertheless, the view that mathematics is a science about the metric properties of and relations among magnitudes, was dominant among mathematicians.

With the emergence of the new abstract divisions of mathematics, a structural approach towards the objects of mathematical investigations took shape; it became ever more clear that the subject-matter of mathematics is not limited to the study of the properties and relations obtainable among magnitudes and spatial figures. One of the important methodological conclusions, emanating from these latest results of mathematics, is this that notwithstanding the practical significance of the metric relations among magnitudes and their representations in numbers and functions, in the theorerical sense, they constitute only a part of the more extensive and deep-going teachings about mathematical structures and categories.

In their attempts at underlining the difference between the modern and the classical mathematics, some scholars often view the modern as the "qualitative" and the classical as the "quantitative" mathematics. However, such a contraposition is, in essence, based on an identification of the concepts of magnitude and number with quantity, and of the abstract structures and categories — with quality. One cannot agree with this position. It is understandable that nobody will object to the position that the concepts of structure and category are qualitatively different from the magnitudes or figures of the three dimensional space. There is, also, no denying the fact that modern mathematics has raised the investigations about the real world to a qualitatively new height. But this does not mean that now mathematics has gone over to the study of the qualitative specificities of objects and processes. It is evident that by contrapositing modern mathematics to the classical,the deeper and more abstract character of the concepts and theorics of modern mathematics are sought to be highlighted, the broadening of its scope and sphere of application is underlined — but by no means a transition to the qualitative methods of investigation is indicated. In contrast to the methods of the concrete natural and social sciences, the methods of the theories of structures and categories are mathematical methods, and not the methods of special sciences, which include obsrvation and experiment. Had the converse been true, mathematics would have been then turned into a branch of the natural sciences and thereby it would have lost its character as a "generally significant and abstract science", to which F.Engels drew our attention.

The concepts and methods of the theories of structures and categories have made the process of application of mathematics in the other sciences, technology and practical activities considerably easy. Actually, having proposed the suitable abstact structures, the scholar or the practical worker can limit oneself only to verifying whether the objects under investigation satisfy the axioms of the structures under consideration or not.The entire further tedious and difficult work of deducing the conclusions from them becomes unnecessary, since one can straight off use all those theorems which were obtained while studying the mathematical structure considered. Thus, the abstract structures and categories of mathematics may be compared with ready-made forms, that may be used while investigating the phenomena and processes having the most diverse contents.

Abstract structures can be successfully used for constructing mathematical models; we may especially use those among them, which aim at revealing not only th nu eri adsstrk c) dependencies among magnitudes , but also the re atitj of a n n-mete c char cter.The study of uch non-metr c relations is f considerable sig ificance f r thos sciences, where owing to the complexity of the object under investigation, and sometimes also owing to the unelaborated stage of a theory, it is imnt ible to presen the results umerically.That is why, there one is often required to turn to the abstract s ructures of order. In their investigations about the dif erent types of rel tio s obta nable mong individuals an groups in social coll ctives, psychologists an sociologists have egu to app y t ori ndsstrk ory of g aphs, whic constitutes the simplest formdstrnslebrdssclategory.

The experience of applying the latest structural methods in the exact natural sciences convincingly shows the future possibilities open for th s line of mathematdsat on f the sciences. n fact, the use of the abstract structures of mathematics in such branches of the exact natural sciences as the theory of relativity and the quantum mechanics, theory of elementary particles and cosmology, quantum chemistry and molecular biology etc., is dictated by the very level of development of these disciplines. The concepts and theories of these disciplines, not only very often do not permit visual representations, but also do not admit of their description in the language of classical mathematics. That is why, there one is required to turn to the ideas and methods of the abstract structures and categories of modern mathematics.Thus, these abstract structures go to highlight the remarkable idea of V.I. Lenin regarding the fact that scientific abstractions,laws and theories do not push us away from objective truth, but rather take us closer to it. "Thought proceeding from the concrete to the abstract — provided it is *correct*... does not get away *f r o m* the truth but comes closer to it. The abstraction of *matter*, of a *law* of nature, the abstraction of *value* etc., in short *all* scientific (correct, serious, not absurd) abstractions reflect nature more deeply, truly and *c o m p l e t e l y*" [3, s. 152 ; Eng. ed., v. 38, P. 171].

Yet another important role of the mathematical structures lies in the fact that they serve as an exact language for the abstract description of the most diverse phenomena and processes.

It is enough to note the fruitfulness of the method of mathematical hypothesis in the process of formation of the quantum theory in physics. In this connection Dyson wrote : "For physics, mathematics is not only an instrument, with the help of which it can quantitatively describe any phenomenon, but is also an important source of such ideas and principles, on the basis of which new theories are generated" [6, s. 112]. This specificity of modern mathematics, as an exact language, for abstractly describing the interconnections among phenomena, chracterizes its role as a synthesizer in the general process of development of scientific knowledge.

While discussing the question of inter-object interconnections, it is necessary, in the first place, to turn our attention, to the role of mathematics, namely, as an exact language for expressing the dependencies, that we come across in physics, astronomy, chemistry and in the other branches of natural science. The mathematical language of formulae, equations and fuctions allows us to express the interrelations and laws of the phenomena investigated in every special science, in the most exact and general form. But for finding out the adequate mathematical language one must take the specific, qualitative character of these phenomena into consideration. All these go to show, that in the real practice of scientific cognition, there exists a dialectical interconnection and reciprocity between the quantitative mathematical methods and the qualitative methods characteristic of every spcecial science. The better we know of the qualitative specificities of the phenomena, the more successful we become in using the quantitative and mathematical methods, for analysing them. The establishment of the quantitative regularity of phenomena, is always based upon the ability to reveal that which is similar and common in what is inherent to the qualitatively different phenomena. And this is possible only by studying the phenomena within the frame-work of the special disciplines.The entire powerful apparatus of mathematics turns out to be effective only in that case, when that which is similar and common in the phenomena under investigation, is preliminarily discovered and formulated in the form of sufficiently deep going general concepts and qualitative dependencies. In fact, if this or that science proposes only the simplest of inductive generalizations of facts and empirical laws, wherein the connections among those magnitudes are established, that are immediately observable in the experiments, then it is impossible to count on the application of the latest methods of mathematics, for their quantitative analysis. The history of physics, chemistry, astronomy and of the other sciences clearly testify to the fact that the progress of theoretical investigations in them was accompanied by an extensive application of the mathematical methods. Often the demand for elaboration of these theories promoted the emergence of new mathematical methods, a clear example of this is the emergence of the infinitesimal analysis.

3. THE MATHEMATICAL STRUCTURES AND THE REAL WORLD

Just as the question of the relation of consciousness and being, is basic for philosophy as a whole, likewise the question of the relation of the mathematical structures and the real world happen to be central for the philosophy of mathematics. In contrast to the positivist approach, the school of N. Bourbaki, not only does not ignore this problem, but, on the contrary underlines the fact that, "the interrelationship of the universe of experiment with that of mathematics" is the basic philosophical problem [5, s. 258].This school also does not deny the existence of close ties among the structures of mathematics and empirical reality, though it considers the reasons of their existence to be entirely unexplainable.

The difficulties connected with the understanding of the objective nature of the abstract structures of mathematics, are rooted in the very specificities of mathematical knowledge, which bases all its propositions upon the laws and principles of logic and not on experiment. It was not accidental, that is why, for G.Frege, B. Russell and their followers to have attempted to seek the foundations of the entire pure mathematics in logic. But therein they ignored the doubtless fact that mathematics, as an independent science, needs its own initial concepts and postulates. Otherwise, as has been correctly pointed out by A. Poincaré, it would turn into a grandiose tautology. It is also important to pay attention to the fact that many western scholars consider logic itself to be an a-priori-science about the forms of thought. And in so far as logic plays the most important role in the formation of abstract structures, often these structures are themselves also viewed as a-priori-forms.

The strictly logical and deductive character of the constructions and substantiations of mathematics are, ultimately, determined by the specificities inherent to the processes of abstraction and idealization in mathematics. Firstly, in mathematics abstraction proceeds significantly further than, say, in the natural sciences. In the concepts of the geometrical point, line, the variable, the function and in the case of all the other mathematical concepts in general, we abstract from the concrete contents and qualitative specificities of objects and processes. Secondly, many abstractions of modern mathematics emerge through a series of successive stages of abstraction and subsequent generalization. It is, namely, thus that all the mathematical structures have been formed. Thirdly, the relative independence of the purely theoretical development is, perhaps, more characteristic of mathematics, than of any other science. In contrast to the experimental sciences, mathematics does not contain any empirical terms or experimental methods for verifying its propositions. These propositions must be proved, i.e., logically deduced from a small number of axioms, accepted without proof. All the hereinmentioned specificities of mathematical knowledge especially clearly came to the fore with the transition in mathematics from the study of the quantitative relations among magnitudes and figures to the investigations into structures of the most diverse kind, which often have only a distant similarity with the traditional objects of classical mathematics. In connection with this, the most widespread notions about the nature of mathematical knowledge and the relation of mathematical objects and structures with the real world, were subjected to criticism.

The empirical notions about mathematics — according to which mathematical knowledge is essentially identical with the natural-scientific knowledge — were the first to be criticised. Moreover, the empiricists misinterpreted natural-scientific knowledge itself. For instance, the followers of classical empiricism viewed its theory to be inductive generalisation of experience. The defenders of similar inductive-empirical notions wanted, according to N. Bourbaki, "to compel mathematics to arise from the experimental truths". Evidently, the new stage of development of mathematics refutes these notions. However, N. Bourbaki so strongly stress the dominant role of rational-theoretic thought in this process, that they entirely forget about the objective source of emergence of mathematical ideas and theories. They do agree that, "of course, on cannot deny that most of these forms had a completely determinate inductive content when they emerged; but once they were consciously deprived of this content, it was possible to give them all their efficacy, which constitutes their strength, and it was made possible for them to acquire new interpretations and to fulfil their own role in data processing" [5, s. 259]. Clearly, all this is true, but from this it at all does not follow that experimental reality goes into the mathematical structures as a result of some predetermination or pre-established harmony.

Exactly in the same way, these structures should neither be considered to be *a priori* constructions of the human mind, nor conventions or argeements devised for ordering empirical data or for "economy of thought" *a la* the subjective-idealist philosophy of E. Mach. One cannot deny the existence of elements of convention, agreement and even of quite understandable economy of thought in these structures, in so far as any abstraction, as pointed out by Engels, is a shortening, and thereby it rids us of a mass of details. But these elements do not play a self-sufficient role and can be correctly understood only in that case, when a structure is considered in the process of its historical emergence and development, wherein the empirical and the theoretical, the contentful and the formal and, the concrete and the abstract factors dialectically interact with each other.

When we deal with the ready-made mathematical structures, then at the first glance they do indeed appear to be *a priori* forms of knowledge, which turn out to be applicable to the study of very different contents. But why does it so happen ? Even the school of N. Bourbaki does not deny the fact that the initial concepts and structures of mathematics do have a fully determinate intuitive content; and this is evident from the aforementioned quotation. Many western philosophers of science stress the priority of form over content in all kinds of ways and that is why they view the mathematical structures as pure forms. One has only to attentively follow the genesis of these forms historically and logically, for the said illusion to vanish.

In fact, isn't there any connection and continuity between the primary structures of mathematics, which have an entirely determinate intuitive content, say the three dimensional space of Euclid, and the many dimensional or even infinite-dimensional abstract spaces ? Don't these structures emerge thanks to the singling out of the deeper and more important properties and relations of the mathematical objects and structures under investigation ? Considering the point of a many-dimensional space to be a vector, for which the ordered sequence of real numbers serve as co-ordinates, we postulate that for them the basic laws of operations over vectors, hold good. Owing to this it becomes possible to establish the connection and to differentiate between the three-dimensional and the many-dimensional spaces: all the laws of operations involving the vectors of the ordinary three-dimensional space do not hold good in the many-dimensional space. No less important is the fact that owing to this, the continuity in the development of mathematical knowledge, and the possibility of testing the conclusions of the many-dimensional space with the help of those of the three-dimensional, are established, since in the limiting case such conclusions must correspond to the results of ordinary geometry.

The situation is quite analogous with any abstract structure in general. The deepest properties and relations of the abstract mathematical objects are formulated in the axioms of these structures. It is, namely, that is why that these structures turn out to be applicable to the investigations of the most diverse phenomena and processes of the real world, and not only of those which served the emergence of the primary structures.

Having begun the work with such ready-made structures, a mathematician usually forgets those intuitive prototypes, which provided the impetus for the formation of the entire chain of successive abstractions and generalizations. And of course he is not bound to remember them, since that would complicate his work. Having proposed an abstrct structure, he deduces the logical conclusions from the axioms, seeks various interpretations for them, establishes its connections with the other structures etc. In other words, the modes of application and elaboration of the mathematical structures are directly opposite to the historical path of their formation. Historically speaking, mathematical knowledge went from the separate, concrete system of objects, or interpretations, towards revealing their general structures,i.e. from the particular and the separate to the general, whereas in the process of further investigations it moves from the ready-made abstract structures towards the observation of various other interpretations.

Marx termed this process of development of mathematical knowledge, wherein its final point is taken as the beginning of the further movement of thought, the "inversion of method".He has illustrated it in detail in the "Mathematical Manuscripts", in the light of the emergence and use of the basic concepts of differential calculus. In the article entitled "On the Differential", he has shown, how, in course of the elaboration of the calculus, the symbolic differential co-efficient becomes an independent point of departure, but "with this, the differential calculus too appears as a specific kind of calculus, already operating independently upon its own ground...." [2, s. 55-57].

These ideas of Karl Marx about the "inversion of method" in the course of mathematical cognition, provides us with an opportunity to proceed correctly towards the solution of the problem of objective contents of the abstract structures of modern mathematics. At the first glance, their emergence appears to be *a priori*, but in reality it is the product of a protracted development, in course of which, the deepest and the most important properties of the structures are gradually brought to light. But as soon as this cycle of development comes to an end and leads to the formation of the corresponding mathematical structure, this final point becomes the beginning of a new stage of mathematical cognition, connected with the elaboration of the theory of the given structure and with its application in the other sciences.

In so far as the entire historical process of emergence of the structures, usually goes on outside the field of vision of the modern mathematician, it is easy for him to have an illusion about the *a priori* character of the abstract structures or about some pre-established harmony among them and the empirical reality.

Roots of the idealist notions about the nature of mathematical cognition consist in this that, therein those connections between the abstractions and reality are ignored, which are realized in the process of application of mathematics in the natural sciences, technology and in the social-human sciences. Here, the contra-positing of "pure" mathematics and its application, is also to be largely blamed. This has promoted the cultivation of the idealist notions to the effect that, it is not the case that the abstractions and structures of mathematics are in agreement with the real world, but, conversely, it is the world and its regularities that come to conform to these abstractions and structures of mathematics. Criticising similar idealistic views about mathematics F. Engels pointed out that here,"as in every department of thought, at a certain stage of development the laws which were abstracted from the real world, become divorced from the real world, and are set up against it as something independent, as laws coming from outside, to which the world has to conform" [1, s. 38].

Another widespread point of view on the mathematical structures is connected with their conventionalist treatment. The famous French mathematician A. Poincaré is the founder of this approach; under the influence of the discovery of the new, non-Euclidean geometries he began to think that the axioms of geometry are conventional agreements and, while choosing them the mathematician is guided exclusively by the demands of convenience. We have seen that the abstract structures are defined by their axioms and, to that extent, here the mathematician enjoys considerable freedom, of choice, and of mutual combinations of the axioms and, that is why, in these structures the conventional moment is clearer than in the ordinary geometrical systems.

Understandably, one can not deny the fact, that in the formation of the abstract structures, as in that of any other mathematical concept, the elements of choice and agreement do have a place. Without such choice, mathematical creativity would become meaningless, but freedom of choice does not signify a rule of arbitrariness. It is confined within the framework of necessity and, in mathematics in particular — constrained by the demands of logical non-contradictority of the axioms of the structure. But how can we be sure of their non-contradictority? In the light of the example of the geometry of Lobachevsky we have seen that the non-contradictoriness of its aximos can be proved by constructing its model with the help of the geometry of Euclid. In its turn, the non-contradictoriness of the geometry of Euclid can be proved with the help of an arithmetical model.

This process of proving the relative non-contradictoriness of the more abstract and new theories with the help of the old theories, with which we are more accustomed, is very characteristic of mathematical cognition. It is testified, firstly, by the fact that there exists a continuity and close inter-connection among the new and the old mathematical theories. Secondly, the freedom in the process of creation of the new mathematical structures, constrained by the demands of logical non-contradictoriness of the system of axioms, in essence signifies this, that the conventional elements play a subordinate role in mathematical cognition. The mathematician may substitute some axioms by others or seek out more general premises for his conclusions, but ultimately the correctness of his results are controlled by logic and by such well substantiated and corroborated theories, as the elementary geometry of Euclid and arithmetic, the truth of which have been tested in the centuries old practice of mankind.

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REFLECTIONS
ON SEVEN THEMES OF PHILOSOPHY OF MATHEMATICS

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PREFACE

An All-Union symposium was organised in the town of Obninsk, from the 26th to the 29th of September 1985, on the theme "The Regularities and Modern Tendencies of the Development of Mathematics". I took part in it. I was invited by Vladimir Invanovich Kuptsov. To him, to a large extent, goes the credit for the relaxed, creative and business-like atmosphere of this symposium. The papers were followed by intensive discussions, which continued in the so-called "round table" meetings. I did not read any paper, but took part in the discussions several times. Mikhail Ivanovich Panov thought that what I said was good enough for publication and, it is he who gave them the shape of papers, to be included in a collection [of some of the papers read at the symposium] edited by him. It is thus that these "Seven Reflections" came into being. Here are the themes :

1. Is it true that in mathematics everything is defined and proved?
2. Is it possible to define the concept of natural number?
3. Is it possible to define the Series of Natural Numbers (written with capital letters)?
4. Is it possible to axiomatically define the concept of a series of natural numbers (written with small letters)?
5. Is it possible to prove, that Fermat's Great Theorem can neither be proved nor disproved?
6. What is a proof?
7. Can mathematics be made understandable?

1. Is it true that in mathematics everything is defined and proved?

Mathematicians are, as a rule, proud of the fact that they are mathematicians. For them, the source of their pride lies in their discipline — not so much in the usefulness of mathematics, as in the fact , that it is an unique field of knowledge, not resembling any other. And even the non-mathematicians are in agreement about its exclusiveness (thus not only the mathematicians themselves, but to their satisfaction, even those around them, recognise the greatness of the mathematicians). Indeed, it is considered to be generally acknowledged, that at least the three following traits belong to mathematics, and to it alone. Firstly, in mathematics, unlike in the other disciplines, all the concepts are strictly defined. Secondly, in mathematics — and again unlike in the other disciplines — everything is strictly proved from axioms. Thirdly, no other discipline has attained that level of respectful trepidation, at which mathematics remains not understood. Tutors of mathematics are hardly more in number, than those of all the other school-level subjects taken together, and yet there is nothing that one (of them)can really say about modern "higher" mathematics: it would be enough to open any monograph, or better still a paper in any journal.(Please note, that it often goes unnoticed, that the third trait indicated above clearly contradicts the first two).

When something becomes very well-known, then a suspicion creeps in : isn't this "something" a myth (well-known ideas do indeed possess an autonomous self-support mechanism). Let us attempt an, as far as possible, unbiased critical examination of the three , just indicated, well-known traits of mathematics.

We notice, firstly, that it is not possible to define all the concepts of mathematics. One is defined through the other, this other through a third etc.; we must stop at some place. (Mrs Prostakova rightly observed — "A tailor learned his trade from another, that one from a third, but from whom did the first tailor learn it?") A story goes, that once the famous mathematician from Odessa S.I. Shatunovsky was introducing ever newer concepts in a lecture and, while so doing he was repeatedly being asked : "And what is this and what is that ? "; he lost his patience at long last, and asked in retort: "And what is 'what is'? ".

Let us consider the structure of a defining dictionary in any language — Russian, English or any other. In it, one word is defined through another, that one through a third etc. But since the words of a language are finite in number, emergence of a circularity is unavoidable (i.e., there emerges a situation, when, in the final count, a word is defined through and by itself). [Here it would be useful to think of a graph, in which the words are placed at the apexes and, when in the dictionary entry defining the word X one meets the word Y, then in that case an arrow goes from the apex X to the apex Y.] There is only one way of getting rid of such a circle: some words are to be left undefined. And that is what is done in some of the dictionaries.[For instance, the words " thing" (in its principal meaning) and "all" have been left undefined in the defining dictionary of English language compiled by Hornby and Parnwell [8]. Unfortunately, such a dictionary has not yet been compiled for the Russian language .] Clearly, such is also the case with the concepts of mathematics. And if, namely, one does not wish to permit a vicious circle, then one has to leave some of the concepts undefined. The question arises — how are these concepts going to be assimilated? Answer : through immediate observation, from experience, from intuition. There is no need to remind the reader, that the formation of general, abstract concepts in the human brain, is a complex process, which belongs more to the realm of psychology, than to that of logic. These concepts, which are assimilated not from verbal definitions, but rather from immediate personal experiences, are naturally called the *primary concepts* or categories of mathematics. The concepts of point, straight line, set, natural number etc. are examples of such categories.

One is certainly required to be careful while preparing a list of the categories (primary concepts) of mathematics (such a list can hardly be made fully precise). Otherwise the number of primary concepts will become unjustifiably large and, the principle of "Occam's razor" will be violated. Here let us consider, for example, the concept of a sphere. It is well-known that a sphere is the locus of points in space having a given fixed distance from a given point — which is the centre of the sphere. However, we can hardly find anyone, who came to know what is a sphere, first of all, from this definition. We must concede that a person assimilates the concept of a sphere in childhood — from the examples of a ball, a globe, a ball-bearing and a billiard ball. One learns the aforementioned definition of a sphere only in the class-room. And there, the teacher does not always find the time to explain it to the learners that the sphere known to them since the early childhood and, the sphere about which they were taught at school —happens to be one and the same sphere. In consequence there grows the notion that : "everything is upside down in their physics and mathematics; perhaps even their sphere too goes upwards". The words just quoted were uttered by a "quite intelligent student ", in justification of a statement made during a lesson, to the effect that a sphere put on an inclined plane starts rolling upward. This remarkable episode has been described in [10]. But, from what has been said above does it follow that since one comes to know about the concept of a sphere from experience, and not from a verbal formulation, the concept of a sphere should be considered a primary concept, one of the categories of mathematics?

It would appear, that the situation becomes more clear in the case of the more complex concepts of mathematics, which are further removed from experience,like for example, the concept of a group — surely, one would not regard the concept of a group to be a primary concept. However, the process of formation of the concept of a group in the brains of professional mathematicians is, perhaps, not very different from the process of formation of the concept of a sphere in the brains of people in general (which includes the mathematicians and the non-mathematicians) : just as the concept of a sphere emerges as a result of numerous observations of various spheres, so did the concept of a group emerge as a result of examination of concrete groups — and only then was this concept fixated in a verbal formulation (obviously, at issue here is the emergence of the concept of a group in the collective experience of mathematicians, and not in the experience of an individual mathematician). That is why not the mode of emergence of a concept,but rather the mode of transmission of informations about it within a system of knowledge, which should be considered to be the characteristic indicator of its primacy (categoricity). For elucidating what has been stated above, we shall imagine a situation where the carrier of a system of knowledge — in our case knowledge of mathematics — has to transmit his knowledge to others. Then he can tell others, what is a sphere or what is a group, by using the verbal definition of the corresponding concept. And that is why these concepts are not categories. If, however, one is required to communicate, what is a set, or what is a straight line or what is a natural number, then that is done differently. For example, it is said that : all the chairs in this room constitute a set, and all the Ostriches beyond the Polar Circle constitute a set (academician P.S. Alexandrov's example), and all the irrational numbers within the interval [0,1]constitute a set; and later on, after having provided sufficient number of examples, it is said that : "these are all sets" — and thus there emerges the general concept of a set. Analogously: zero, one, two, three, four, five etc. are all natural numbers, and thus emerges the general concept of natural number.[It is high time for putting an end to the anachronism of beginning the series of natural numbers with one. In a pencil-box there are always some natural number of pencils — perhaps zero. A natural number is the cardinality (of the number of elements) of a finite set, in particular — that of an empty set.] (We see that while explaining the concept of natural number, there appears the word "etc." — implicitly or explicitly, and it could not have been otherwise in the case of primary concepts: first, a sufficient quantity of examples are indicated and, then we have the word — "etc.")

Thus, the first among the myths about mathematics — that, "in mathematics everything is defined" — collapses. Let us proceed to the second : "in mathematics everything is proved from axioms". In order to get convinced that such is not the case and, to thus blast this second myth too, it would be enough to open the classic text-book of school geometry by A.P. Kisvpfx.psr any text-book of mathematical analysis for the technical colleges, or any university level text-book of the theory of numbers. In all these text-books we come across theorems being proved, but hardly any axioms (save the axiom about the parallel lines — the fifth postulate of Euclid). The situation is somewhat enigmatic. Indeed, if there are no axioms, then on the basis of what re the proofs — say, of the theorems of the theory of numbers — put forward ? Evidently, on the basis of common sense and some notions about the basic properties of the natural numbers; though these notions are identical for all persons, they have not been explicitly formulated in the form of a list of axioms. (How far they may be so formulated! Well, that is the theme of our next reflection).

It must be stated in all honesty that, in reality, in mathematics one pretty often comes across theorems, which are proved without basing the proofs upon any kind of axioms. The situation with the third trait of mathematics indicated by us — namely, with its non-understandability — happens to be more complex. It would be very easy to say that it is a myth; but if in respect of the first two traits it was enough to ask mathematics itself — one asks and gets a negative answer — then here, of course, it is pointless to turn to mathematics with the question: whether or not it is understood. A survey of social opinion indisputably situates mathematics at a prized spot in terms of the level of non-understandability. The reasons behind such an opinion happen to be the theme of a separate large-scale investigation. Any explanation of this phenomenon, it must be admitted, can only be that much objective, as much it is possible, in general, to be objective about the issues of social psychology. We shall not plunge into such a discourse here. In our last reflection we shall be making some remarks on this theme.

2. Is it possible to define the concept of natural number?

It can certainly be said that a *natural number* is the quantity of items in a finite totality. Evidently this formulation is compatible with the meaning (to be more precise, with one of the meanings) of the verb"to define" according to the "Defining Dictionary of the Russian Language " edited by D.N. Ushakov [5] (" to give a scientific, logical characterization, a formulation of any concept, to lay bare its (scientific)content"), as well as with the formulation found in the Philosophical Encyclopaedia [11] (the "definition" of an object — the results of the investigations about which are reflected in the corresponding concepts — "may be viewed as an (explicit and concise) formulation of the contents of these concepts"). Let us proceed now to the concepts behind the verb "to define" and the word "definition", from the position of a mathematician. And then , we demand that a definition should contain exhaustive information about the concept defined — it must be so exhaustive as to permit a person having no earlier knowledge about a given concept, to form a correct understanding of it, solely on the basis of the definition provided. In that case can we assume, that a person who does not know anything at all as to what a natural number is all about (we are not talking here about the term, but, namely, about the concept), will be able to assimilate this concept from the first sentence of the present paragraph ? That is very very doubtful : when one really does not know what is a number, then it is quite likely that he may take the words "quantity of items" to signify, say, their total weight and, the very concept of finite totality of items gets diffused when one considers very large totalities. Probably everyone would agree that trillion to the power trillion is a natural number, however, it remains a fact that this number is greater than the number of atoms in the whole Universe. It is not clear, as to how far appropriate it would be to talk about a finite totality of trillion to the power trillion quantity of items[16].

Thus, we shall be captiously demanding an exhaustive completeness from a definition, i.e. we shall be demanding that the concept being defined be expressed with the help of generally accepted syntactical constructions through other concepts, which serve as starting points for the definition under consideration. Taking into consideration what has been said above, let us attempt the following formulation : a natural number is the cardinality of a finite set. Three basic concepts are operative in this definition : 1) set, 2) cardinality and, 3) finite. The just mentioned formulation indeed appears to be the definition of a natural number, within the frame-work of those theories wherein these concepts are already somehow interpreted (in particular, declared to be uninterpretable or primary). Namely such a definition — such, in the ideational sense, right upto the inessential details — has been accepted in the "Éléments de mathématique" of Nikolai Bourbaki. (In this connection I would like to remind the reader that in Bourbaki's theory the full name of one [unity] requires tens of thousands of symbols, for the purpose of being written down) [6,p.188]. Common sense, however, refuses to accept the concepts of set, cardinality and finite to be simpler than the concept of a natural number. Here we have the typical example of a definition of the simple through the complex.

One should not take the above-mentioned statements to be a criticism of N. Bourbaki and of the other authors, who put forward analogous formulations. Evidently, they, like other people, have some *a priori* notion of a natural number (apparently, *a priori* in respect of the definitions they propose, and not in respect of experience). They do not intend to give an explaining definition of the concept of natural number (i.e., a definition that could be used to teach a novice). Their aim is more modest and technical: to give a definition of this concept within the frame-work of an expounded axiomatic set theory. The concept of a function can be defined through the concept of a pair, and the concept of a pair can be defined through the concept of a function. It is clear that these intellectual constructions have hardly anything in common with the problem of explaining to the uninitiated, what is a pair and what is a function. The aim of the entire foregoing discussion is to lead the reader to the following almost self-evident idea. Let us set aside the mathematical and logical problematique, connected with the search for a definition of (it would be more correct to say — the attempts at representing, modelling) the natural number, within the framework of this or that axiomatic theory. Let us take up instead, the attempts at providing a "naive" explanation of the concept of a natural number — an explanation, that would enable one who does not know, to know what it is all about. Very soon we shall get convinced that such attempts are fruitless. We must admit that the natural number is a primary, undefinable concept — it is one of the categories of mathematics.

3. Is it possible to define the Series of Natural Numbers (written with capital letters)?

Having failed in our attempts at defining the natural number (or, on the contrary, having succeded in finding out that this concept is an undefinable category), let us now turn to the concept of the Series of Natural Numbers. When written with the big or capital letters, the Series of Natural Numbers is the totality of all the natural numbers. If we know what is a natural number and understand the words "totality of all", then we know what is the Series of Natural Numbers. Conversely, if we know the Series of Natural Numbers, then we can easily define a natural number as one of its elements. That is why the concept of the Series of Natural Numbers is as undefinable as the concept of natural number. (However, the sentence "The Series of Natural Numbers is the set of all natural numbers" may be viewed as a legitimate definition of the concept of the Series of Natural Numbers through the primary, undefinable concepts of a "natural number" and the "set of all".)

The reader would exclaim — "How come? And what about the axioms of Peano? Don't they define the Series of Natural Numbers ?" Of course they don't, and what is more, if one understands the Series of Natural Numbers as we do — i.e., as the unique totality of some univocally understood essences called the natural numbers — then the axioms of Peano do not even pretend to do that. Indeed, let us see, how the axioms of Peano look: "Zero is a natural number, and zero is not the successor of any number, etc." Thus, these axioms are based on the concepts "zero" and "successor of" (here, immediate succession is at issue). But they do not explain, and they can not explain, what these concepts signify (i.e., what is "zero" and what is "successor of"), they only indicate the connections between these concepts. However, these axioms have been so formulated, that if the zero of these axioms is the ordinary Zero of the Series of Natural Numbers, and if the words "successor of " signify the immediate succession of one number after another in the Series of Natural Numbers (such that Zero is succeded by Unity, and after Unity comes the figure Two etc.), then all these connections will be satisfied in the Series of Natural Numbers.[In order to stress the uniqueness, i.e., the absolute singularity of the terms of the Series of Natural Numbers — Zero,One (Unity), Two (the figure Two) etc. — we write them with the capital letters. The words " Zero", "One" (or "Unity"), "Two" (or the figure "Two") etc. are proper nouns in the absolute sense (just like the words "Sun", "Moon" and "Earth"), each of them has unique meaning — the quantity of elements of the empty, unit, two-element etc. sets. And the "zero" axiom of Peano indicates a proper noun only relatively, within the limits of the given context, to be more precise — in the context of that structure, which has been described by these axioms. There are many such structures, and each one of them has its own zero.] In other words, Peano's axioms turn out to be true, correct statements upon their natural interpretation in the Series of Natural Numbers. But evidently, they will be true, not only in the Series of Natural Numbers, but also in all the other structures isomorphic [on the concepts " isomorphism" and "isomorphic" we request the reader to go through the second of the two articles entitled "Isomorphism" in the 3rd edition of the Great Soviet Encyclopaedia [14] — V.A. Uspensky; readers of the present translation will find this article on "Isomorphism" at the end of the present theme **3.** — Tr.] to the Series of Natural Numbers. For example, if the term "zero" in the axioms of Peano is interpreted as the smallest prime number, and the words "successor of"— as the [result of] a transition from one prime number to another immediately next to it, then under such an interpretation all the axioms of Peano turn out to be true. It appears that these axioms do not even permit us to distinguish the Series of Natural Numbers from the totality of all the prime numbers. I repeat, they do not pretend to do so. They claim to, it is said," define the Series of Natural Numbers right upto isomorphism". To be more precise, the axioms of Peano define not one, but at once many mathematical structures, moreover they are all isomorphic to the Series of Natural Numbers and, consequently, isomorphic to each other. To be more precise, the axioms of Peano define the entire class of such structures. We shall call any such structure a series of natural numbers (written with small, or lowercase letters!). Thus, the Series of Natural Numbers is one of the serieses of natural numbers.

Briefly speaking, isomorphism of two mathematical structures is the mutually-univocal correspondence among the totalities of elements of the first and the second structure, retaining the operations and relations defined on these structures. In our example the isomorphism between the structure N (the Series of Natural Numbers with the operation "to follow") and the structure P (prime numbers with the operation "to follow") provides the following endless table:

0	1	2	3	4	5	6	...
2	3	5	7	11	13	17	...

In this correspondence the operation "to follow" is indeed retained: 6 follows 5, and simultaneously 17 follows 13, and in general in the upper row y follows x if and only if the corresponding terms of the lower row p_y and p_x (namely, in this order!) follow one after the other (follow in the sense defined for P).

It is sometimes said that the Series of Natural Numbers is the series

0, 1, 2, 3, 4, 5, 6 ..., 126, ...

but likewise it can be said that the Series of Natural Numbers is the series
zero, one, two, three, four, five, six... , one hundred and twenty six,....
or the series

$\underline{\overline{0}}$, I, II, III, IV, V, VI..., CXXVI,....

[Isn't the persistent exclusion of zero from the series of natural numbers explained by the absence of the symbol $\overline{0}$ in the traditional collection of symbols? Briefly speaking, aren't we situated at the level of the Latins on this question?]

Evidently, none of these serieses happens to be the Series of Natural Numbers (which consists of abstract quantitative categories and can not be depicted), these are but the serieses of names designated for its terms, i.e. for the natural numbers. At the same time each of these serieses of names may be viewed as one of the serieses of natural numbers, written with small letters.

The situation with the Series of Natural Numbers is universal in character. For example, we have an analogous situation with the three dimensional Euclidian space, in which we live. Let us digress from the fact that most probably we live in non-Euclidian space, and generally speaking, we live not in the mathematical, but in the physical space — and these are different objects. [In this connection we must mention the fact that, most probably, the "physical" Series of Natural Numbers is something different from its mathematical model — the "mathematical" Series of Natural Numbers. On this issue see the deep-going but insufficiently appreciated essay [16] by P.K. Rashevsky.] Let us abstract from reality and imagine that we live in an entirely concrete three-dimensional Euclidian Space (we are again using capital letters, as we wish to stress the uniqueness of this space). However, it can not be defined with the help of any number of axioms, it may only be "indicated with a finger". On the other hand, there are numerous systems of axioms (the most famous among them belongs to Hilbert) [3], defining this space "right upto isomorphism". The phrase within quotation marks indicates the fact that the given system of axioms defines an entire class of mutually isomorphic spaces, and that our "real" Euclidian Space happens to be one of them.

In general, no system of mathematical axioms can ever define any structure univocally, in the best of the cases they define it right upto isomorphism. (We speak here of " the best of the cases" as there are very important systems of axioms, which define the class of non-isomorphic structures. For instance, the axioms of group theory define the mathematical structures called the groups, but all of them are not mutually isomorphic.)

Let us sum up. It is not possible to axiomatically define the Series of Natural Numbers. We may try to axiomatically define the concept of a series of natural numbers — i.e., the concept of any arbitrary structure, isomorphic to the Series of Natural Numbers. We shall be discussing these attempts in our next reflection.

ISOMORPHISM

It is one of the fundamental concepts of modern mathematics. It initially arose in algebra in connection with the algebraic systems, such as groups, rings and fields, but proved to be extremely significant for the understanding of the structure and domain of possible applications of every branch of mathematics.

The concept of isomorphism applies to systems of objects on which operations or relations are defined. As a simple example of two isomorphic systems consider the system R of all real numbers under the operation of addition $x = x_1 + x_2$ and the system P of positive real numbers under the operation of multiplication $y = y_1 y_2$. It turns out that the internal "lay out" of these two systems of numbers is identical. To show this we map the system R onto the system P by associating to the number $y = a^x$ $(a > 1)$ in P, the number x in R. Then the product $y = y_1 y_2$ of the numbers $y_1 = a^{x_1}$ and $y_2 = a^{x_2}$ which correspond to x_1 and x_2, will correspond to the sum $x = x_1 + x_2$. The inverse mapping of P onto R is given by $x = \log_a y$. From any proposition concerning the addition of numbers in the system R we can obtain a corresponding proposition concerning the multiplication of numbers in the system P. For example, since in R the sum

$$S_n = x_1 + x_2 + \cdots + x_n$$

of the terms of an arithmetic progression is given by the formula

$$S_n = \frac{n(x_1 + x_n)}{2} ,$$

it follows that in P the product

$$P_n = y_1 y_2 \cdots y_n$$

of the terms of the corresponding geometric progression is given by the formula

$$P_n = \sqrt{(y_1\ y_n)^n}$$

(raising to the n-th power in P corresponds to multiplication by n in R and extraction of the square root in P corresponds to division by two in R).

As regards their properties, isomorphic systems are essentially the same. From an abstract mathematical standpoint, such systems are indistinguishable. Any system of objects S' that is isomorphic to the system S may be regarded as a "model" of S (modeling a system S by means of a system S'), and the study of the properties of S may be reduced to the study of the properties of the "model" S' of S.

The following is a general definition of the isomorphic system of objects such that each system has a number of relations and each relation involves a fixed number of objects. Let S and S be two given systems of objects. Let

$$F_k(x_1, x_2, \cdots), \quad k = 1, 2, \cdots, n$$

be the relations on S and let

$$F'_k(x'_1, x'_2, \cdots), \quad k = 1, 2, \cdots, n$$

be the relations on S'. The systems S and S', with their respective relations, are said to be isomorphic if there exists a one-to-one correspondence

$$x' = \Phi(x) \qquad x = \psi(x')$$

between the elements of S and S' such that

$$F_k(x_1, x_2, \cdots)$$

implies

$$F'_k(x'_1, x'_2, \cdots)$$

and vice versa. The correspondence is said to be an isomorphic map, or an isomorphism. [In the example cited above, the relation $F(x, x_1, x_2)$, where $x = x_1 + x_2$, is defined on the system R, and the relation $F'(y, y_1, y_2)$ where $y = y_1 y_2$, is defined on the system P, a one-to-one correspondence is given by the formulas $y = a^x$ and $x = \log_a y$.]

The concept of isomorphism arose in group theory where the fact that the study of the internal structure of two isomorphic systems of objects represents one rather than two problems was first understood.

The axioms of any mathematical theory determine the system of objects studied by the theory only upto isomorphism: a mathematical theory based on axioms, that is applicable to one system of objects is always fully applicable to another. Therefore, every axiomatic mathematical theory allows not one but many "interpretations" or "models".

The concept of isomorphism includes, as a particular case, the concept of homeomorphism, which plays a fundamental role in topology.

A particular case of an isomorphism is an automorphism, which is a one-to-one mapping

$$x' = \Phi(x) \qquad x = \psi(x')$$

of a system of objects with given relations $F_k(x_1, x_2, \cdots)$ onto itself such that $F_k(x_1, x_2)$, implies $F_k(x'_1, x'_2, \cdots)$ and vice versa. This concept also arose in group theory but later proved significant in most disparate branches of mathematics.

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[Source: *The Great Soviet Encyclopedia*. A Translation of the Third Edition. Vol. 10, p. 465. Macmillan, 1976.]

4. Is it possible to axiomatically define the concept of a series of natural numbers (written with small letters) ?

So then,we get down to the attempts at axiomatically defining the concept of a series of natural numbers, which is a structure isomorphic to the Series of Natural Numbers. As soon as we utter the word "isomorphism", already thereby it is proposed, that the relations and operations to be retained under this isomorphism have been indicated. Consequently, first of all we must indicate precisely, the relations and operations we wish to examine in the Series of Natural Numbers and in the serieses of natural numbers isomorphic to it. Among these operations we may include zero-place operations(i.e., individual constants ; for example, the individual constant "zero" may be viewed as a zero-place operation) and one-place relations (i.e., properties). These earmarked operations and relations are to a significant extent arbitrarily indicated. For example, the Series of Natural Numbers (and thereby any series of natural numbers isomorphic to it) may be viewed :1) as a structure only with the order relation "< ", or 2) as a structure with an earmarked element "zero" and the operation "transition to the next ", or 3) as a structure, wherein, apart from the relations and operations already mentioned, the operations of addition and multiplication have also been earmarked.

For our purposes it would be most graphic not to indicate any operation, but only to stipulate the order relation "$<$ ".Thus we shall be viewing every series of natural numbers as a set, on which the binary order relation "$<$ "has been defined. We shall be investigating, namely, the properties of such a mathematical structure.

Let us enumerate these properties. When the relation "$<$" is understood as an ordinary relation of order among natural numbers, then every property of the relation "$<$ " in an arbitrary series of natural numbers must (on the strength of the presence of isomorphism) hold good also in the usual Series of Natural Numbers. After this remark, let us now formulate some of these properties.

1. The relation "$<$ " is transitive . Symbolically :

$$\forall x \forall y \forall z (x<y \wedge y<z \Rightarrow x<z)$$

2. The relation "$<$ " is anti-reflexive. Symbolically:

$$\forall x \urcorner (x<x) .$$

3. The relation "$<$ " is symmetric. Symbolically:

$$\forall x \forall y (x<y \vee y<x) .$$

The totalilty of these three properties simply affirms that "$<$ " is a relation of strict linear order.

Before going ahead further, let us stop and think: strictly speaking, why are we listing these properties ? Here's why. We hope that having listed a number of properties, we shall be able to axiomatically define a series of natural numbers . In greater detail, our plan is as follows. At first we write out some of the properties characteristic of the Series of Natural Numbers. Then we declare these properties to be axioms and define a series of natural numbers as an arbitrary mathematical structure, satisfying the listed axioms. We do not exactly claim that a set defined with the given binary relation "$<$ " satisfies our axioms (such a claim would be quite unrealistic), but we do claim that all such sets (with the given relation) turn out to be mutually isomorphic. In so far as the Series of Natural Numbers will satisfy our axioms (we shall be so chosing the axioms), the Series of Natural Numbers will be one of the pairwise isomorphic structures, satisfying these axioms, and this means that all these mutually isomorphic structures will also be isomorphic to the Series of Natural Numbers .If we succeed in attaining the goal just enunciated, then we should think that we have been able to axiomatically define a series of natural numbers.

Keeping in view the aim that we have put forward , can we remain satisfied with the three properties-axioms listed out ? Evidently, no. All linearly ordered sets satisfy these axioms, among them many are non-isomorphic and, consequently, wittingly non-isomorphic to the Series of Natural Numbers N . For example, the set of all real numbers R with the usual order relation will satisfy the three listed axioms. By comparing N and R we note that N has at least two such properties, which are absent in R .

These are :

4. N contains the smallest element. In symbols:

$$\exists x \forall y (x=y \vee x<y).$$

5. In , N after every element x immediately follows some y. ("Immediately" means there is no third element between x and y.) In symbols :

Fig.1

$$\forall x \exists y \, (x < y \wedge \neg \exists z \, (x < z \wedge z < y)).$$

These five axioms significantly narrow down the range of linearly ordered sets satisfying them. The Series of Natural Numbers, as well as the set of real numbers

$$0, \frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{5}{6}, \frac{6}{7}, \cdots \qquad (*)$$

(considered in the usual order), comply with these axioms. The existence of this, different from N, structure (*), satisfying the axioms 1-5, still does not constitute an hindrance to the view that these axioms provide an axiomatic definition of a series of natural numbers : since this structure is isomorphic to N (and, thus it can be identified as a series of natural numbers).

Figure 1 provides a graphic depiction of the order in (*) (and in N). However, it is easily noted that the structure (i.e., the set plus the order relation):

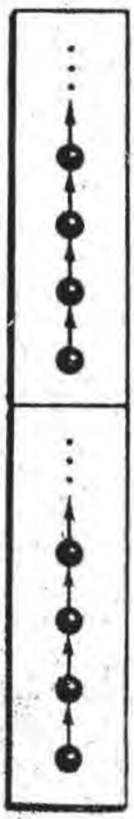

Fig.2

$$0, \frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{5}{6}, \frac{6}{7}, \cdots 10, 10\frac{1}{2}, 10\frac{2}{3}, 10\frac{3}{4} \cdots \qquad (**)$$

too satisfies the axioms 1-5 . Figure 2 gives a graphic depiction of this ordered structure. In this structure two elements (0 and 10) do not have any immediate predecessors. Let us fixate this situation in the following axiom 6 .

6. If two elements x_1 and x_2 do not have any immediate predecessor, then they are equal. In symbols:

$$\forall x_1 \; \forall x_2 \quad \{ [\neg \exists y_1 \, (y_1 < x_1 \wedge \neg \exists z_1 \, (y_1 < z_1 \wedge z_1 < x_1))] \wedge$$

$$\wedge [\neg \exists y_2 \, (y_2 < x_2 \wedge \neg \exists z_2 \, (y_2 < z_2 \wedge z_2 < x_2))] \Rightarrow x_1 = x_2 \}.$$

Axiom 6 eliminates the structure (**),but does not eliminate the structure

$$0, \frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \cdots, \cdots$$

$$\cdots 9 + \frac{1}{m}, 9 + \frac{1}{m-1}, \cdots 9\frac{1}{4},$$

$$9\frac{1}{3}, 9\frac{1}{2}, 10, 10\frac{1}{2}, 10\frac{2}{3}, \cdots, 10 + \frac{n-1}{n}. \qquad (***)$$

It is evident that the structure (***) is not isomorphic to a series of natural numbers .

Like the horizon, our goal is moving further and further ... It appears to be unattainable. The following noteworthy fact appears to be the case: however many axioms may we write out, using the logical symbols, the symbol "$<$ " and the variables, covering the elements of the structure being defined — there will always be a model of the totality of listed axioms, non-isomorphic to a series of the natural numbers . In view of the fundamental importance of this fact (signifying the impossibility of axiomatically defining a series of natural numbers by using the means indicated), let us describe it in greater detail.

The following symbols belong to the alphabet of the formalized symbolic language,which we are using for listing the axioms:

1) the symbols of punctuation: the left hand side bracket "(" and the right hand side bracket ")",

2) the logical symbols " $\neg$ ", "$\wedge$" , "$\vee$" , "$\Rightarrow$" , " $\forall$ " , " $\exists$ " , " $=$ ",

3) the individual variables x , y , z , u , v , w , x_1 , y_1 , z_1 , u_1 , v_1 , w_1 , $\cdots$,

4) the symbol "$<$ ".

Formulas are composed with the help of these letters, according to the natural , but easily formulated syntactical rules. Simplest examples of such formulas are :

$$x<y \vee y<x ;$$
$$\forall x\,(x<x) ;$$
$$\exists x \exists y\,(y<x \Rightarrow y<<x) ;$$
$$\exists y\,(x<y) ;$$
$$\forall x \exists y\ (x< y) .$$

Now, let us take any set, with any binary relation denoted by "$<$ " defined on it (it is not obligatory for the relation to be one of strict order). We shall be calling all such sets with the relation "$<$ ",a *structure with the label* $<$. Thus, a structure with the label $<$ consists of a set (called the *carrier* of the structure) and the relation "$<$ " . Let us fix a carrier of the structure as the domain of change for each individual variable. Then every formula becomes either a sentence , as the second, third and the fifth formulas of the just mentioned list are, or sentential forms, like the first and the fourth formulas of the same list. Those formulas which turn into sentences are called *closed* ; we shall be considering only them in future. It is not difficult to notice, that the property of "being closed" in respect of a formula, does not depend on the structure , wherein we are examining the said formula; this property can be defined purely syntactically, according to the external form of the formula.(Closure consists of this that all the variables must be bound by quantifiers.) It is said in respect of a (closed) formula — when considered in a given structure — which becomes a true sentence, that it is *true in the given structure* or that it is *satisfied in the given structure* ; and about the structure it is said, that it is *satisfied by the given formula.*

The structure N — our usual Series of Natural Numbers with the usual order relation — may be singled out from among the structures with the label $<$. we shall call any closed formula, turning into a true sentence when interpreted in the structure N — an axiom. Thus, however many — finite or infinite number of — axioms may we write out, there will always be such a structure with the label < , which, firstly, satisfies all the listed axioms, and is secondly, isomorphic to N.

Thus , it so turns up, that a series of natural numbers can not be defined axiomatically; since to define N axiomatically is to list such a system of axioms, as would define N upto isomorphism (this, in its turn, means, that any two structures satisfying all the listed axioms, are isomorphic).

"But excuse me "— the reader will again object — "the axioms of Peano do define the Series of Natural Numbers upto isomorphism. Peano's system of axioms is categorical, and this signifies, that all the models of it are isomorphic". [Any structure satisfying each of the axioms of a system is called the *model* of that system or list of axioms.] A little patience ! We shall look into the axioms of Peano too.

But now we shall discuss another question. Not merely the order relation "< " , but a numerous set of other relations and operations are defined on the Series of Natural Numbers. Among them there are the two-place (or binary) relation of divisibility of two numbers; the three-place (or ternary) relation "$x + y = z$ " ; the one- place (or singular) relation of "being a prime number" (let us recall that we treat properties as one-place relations), [here we are using the etymologically more correct term "singular", following W.V.O. Quine, instead of the now widely used term "unary"; see: 7, note 29]; the two-place operation of addition; the two-place operation of multiplication; the two-place operation of involution ($0^0 = 1$); the one-place operation of following immediately (as is customary, we shall indicate it with the prime symbol, such that, $0' = 1$; $13' = 14$); the constants $0, 1, 2, 3, 4, \cdots$ (let us recall that we treat the constants as zero-place operations) ; the four-place operation $[\log_{u+1} (z\,! + y^{x \cdot z + u})]$ (here, as usual, through $[a]$ we indicate the integral part of the number a); and many others. We have adduced only a few examples, and altogether a countless number of operations and relations are defined on N. In order to define the concepts of a structure isomorphic to N, we must at first separate out some operations and relations from among them (theoretically it is possible to take into consideration all of them) and examine the isomorphism, namely, in respect of these isolated operations and relations. Indeed , that is why there does not exist the concept of a series of natural numbers as such, but only the concept of a series of natural numbers in respect of a given list of operations and relations.Earlier we have examined the concept of a series of natural numbers in respect of a list, wherein there were no operations at all, and there was but one relation — the relation of "lesser than".

In the context of our investigations, the operations and relations singled out in the set are called — *labelled*, and the list of such operations and relations — a *label*. To be more precise, not the list of these operations and relations themselves, but the list of their names, is called a *label*, (this distinction is very important in itself) but for our purposes it is not quite essential, and it would be easier for us not to notice it. A set with some singled out operations and relations, forming the list σ, is called the *(mathematical)structure with the label* σ. Now we can say that any series of natural numbers is a structure with this or that label σ. That is why, we should be speaking not about a series of natural numbers in general, but about a series of natural numbers with the label σ. So far we have examined the case, when

$$\sigma = \{<\}.$$

Perhaps the poverty of this label is the reason behind the failure of our attempt at axiomatically defining a series of natural numbers? Let us broaden the label and see what happens. First of all, let us add to "<", the constant "0" (for denoting the smallest element in respect of the order "<") and the prime symbol " ′ " to indicate the operation of immediate succession. In the Series of Natural Numbers N, these objects come under the following axioms (properties) 7 and 8 (compare the properties 4 and 5, which follow from the properties 7 and 8) :

7. $\forall y\,(0 = y \vee 0 < y)$;

8. $\forall x\,(x < x' \wedge \neg \exists z\,(x < z \wedge z < x'))$.

Any series of natural numbers with the label $\{0, {}', <\}$ is by definition isomorphic to N, since isomorphism is considered in respect of $\{0, {}', <\}$. That is why any such series of natural numbers consists of the elements 0, $0'$, $\cdots$, ordered as follows:

$$0 < 0' < 0'' < 0''' < \cdots$$

R e m a r k s. We must be aware of the fact that every series of natural numbers has its own 0, own ′ and own <, i.e., own element indicated by 0, own operation signified through " ′ " and, own relation denoted by "<". Strictly speaking, for every series of natural numbers we shall devise its own symbols for these objects — for example, if we are considering a series of natural numbers M, then it is necessary to add this letter " M " as an index to the symbols "0", " ′ ", and "<". This strictness provides some convenience . However, the absence of strictness also gives rise to some convenience .In the given case, the convenience from lack of strictness is considered to be greater, and that is why one and the same "0" is used to signify various elements (but in every series of natural numbers it denotes one and only one element; in particular, the cardinality of the empty set in the Series of Natural Numbers). Analogously for " ′ " and "<". These remarks are valid not only for a series of natural numbers, but also for any structure with the label $\{0, {}', <\}$, not bindingly isomorphic to N.

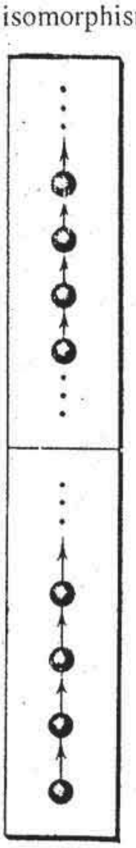

Fig.3

Now we shall see, how an arbitrary structure with the label $\{0, {}', <\}$, subordinated to the axioms 1-8, looks (axioms 4 and 5 follow from the axioms 7 and 8, but that is not a great calamity). Evidently, it is a linearly ordered set, wherein 0 is the least element, $0'$ is the element immediately following 0 (such that there is nothing between 0 and $0'$). $0''$ is the element that immediately follows $0'$ etc. All these elements — 0, $0'$, $0''$, $0'''$ — form the initial cut of our structure. This initial cut is called the *standard* part of the structure, and the remaining part (it may even be empty) is called *non-standard*. The standard part is isomorphic to N. Had there been nothing but this standard part in any structure with the label $\{0, {}', <\}$, subordinate to the axioms 1-8 , then we would have attained our goal:

axioms 1-8 in their totality would have given us the axiomatic definition of a series of natural numbers we are looking for, to be more precise — they would have given us the axiomatic definition of a series of natural numbers having the label $\{0, ', <\}$.

This, however, is not the case. The structure graphically depicted in figure 3 — say, the one like (***), where

$$0' = \frac{1}{2},\ \left(\frac{1}{2}\right)' = \frac{2}{3},\ \left(9\frac{1}{4}\right)' = 9\frac{1}{3} \text{ etc.},$$

satisfies the axioms 1-8, but it is not isomorphic to N : it contains a non-empty non-standard part (in figure 3 this non-standard part has been depicted in the upper rectangle, in (***) this non-standard part consists of the elements of the form $9 + \frac{1}{m}$ and $10 + \frac{n-1}{n}$). What is more, it turns out that no system of axioms can give us a series of natural numbers with the label $\{0, ', <\}$, since the structure depicted in figure 3 will always be a model for such axioms.

Fig. 4

Perhaps it is still a case of poverty of the label ? What will happen if we add addition and multiplication and consider a series of natural numbers not having the label $\{0, ', <\}$, but one with the label $\{0, ', <, +, \cdot\}$?

Would it be possible to make a list of axioms for such a richer label, which would define the concept of a series of natural numbers having this label — i.e., from *all* the structures with this label, would it be possible to single out those structures which are isomorphic to N in respect of $0, ', <, +, \cdot$? It appears that no, it can not be done. Whatever be the totality of axioms — finite or infinite — made out by us, for this totality there will always exist a structure [with the label $\{0, ', <, +, \cdot\}$], non-isomorphic to N. [When we speak of axioms, we have in view a symbolic language, like the one described above for the label $\{<\}$; only now, together with "<", the alphabet contains "0", "′", "+" and "·".]

What is more, *whatever be the label* chosen and *whatever be the system of axioms* chosen for this label, there will always exist a model of this system of axioms, not isomorphic to N. Such, non-isomorphic to N, models are called non-standard, and the axioms listing the properties of a series of natural numbers (especially, when + and · enter into the label), are called the *axioms of arithmetic*. That is why, we may re-state what has been stated above as follows : *there exists a non-standard model for any system of axioms of arithmetic*.

if the axioms 1-8 or something equivalent to them enter into our axioms, then it is possible to single out the standard part 0, $0'$, $0''$, $\cdots$ in any model; in this case, non-standardness of a model signifies the non-emptiness of the non-standard part. This non- standard part may turn out be more complexly constructed , than the case depicted in figure 3. From the standpoint of order, the non-standard part depicted in figure 3 is similar to the set Z of all whole numbers. In the case of natural axioms for a label that includes the operation of addition, the non-standard part of any denumerable structure (i.e., of one containing denumerable number of elements), satisfying these axioms, assumes a look, which we have (not very successfully) attempted to depict in figure 4. In this diagram we have attempted to somehow depict the following idea : a very large (denumerably infinite) number of examples of the sets of whole numbers Z are taken up, and these examples are arranged like the set of all rational numbers Q.

Thus, it is not possible to produce a system of axioms, defining the concept of a series of natural numbers (with any label whatsoever). To our knowledge, a more detailed interpretation of this statement is as follows : whatever operations and relations defined on N, may be chosen, there can not be such a system of axioms, all models of which are isomorphic to N, in respect of these operations and relations.

And now we shall answer the question: "But what about the axioms of Peano ?".

With inessential changes, the classical axioms of Peano are us under. Here, the label $\{0, '\}$ is being considered .Three axioms have been formulated:

I. $\neg\ \exists x\ (x' = 0)$;

II. $\forall x\ \forall y\ (x' = y' \Rightarrow x = y)$;

III. The Axiom of Induction.

We have, till now, only named the third axiom of induction, but have not described it. Now we describe it:

$\forall P\{[P(0) \wedge \forall x\,(P(x) \Rightarrow P(x'))] \Rightarrow \forall x\ P(x)\}$.

When we look at the axiom of induction we notice that together with the usual individual variable it contains yet another variable P. we shall explain the meaning of this variable . First of all, let us recall that the semantics of a formula (i.e., the meaning attached to it) emerges only after the mathematical structure corresponding to the label is produced. In particular, in order to find out the meaning of the axioms of Peano (of the formulas I-III), we must produce some structure with the label $\{0, \ \}$, i.e., a set with a singled out element, indicated by "0 " and a singled out one-place operation, indicated by " ' ". Then the domain of change of the variabale x is at once defined (like that of any individual variable) — it is the set of all elements of the structure under consideration. What is the domain of change of the variable P like ?

The variable P is of a special type, we have not met with the like of it hitherto in our enunciation. Its domain of change consists of all the possible properties (= one-place relations), defined on the structure under consideration, i.e., the properties of the elements of this structure.

The concept of property is a primary concept; it is grasped from examples. The property of being even is defined on the natural numbers — every number may be either even, or odd. It is inessential that there are even as well as odd numbers; we may construct a situation, where all the numbers are even; what is important is that it should make sense to ask in respect of every number, whether it is even or odd. The property of being green is not defined on a series of natural numbers ; for a number the talk of "being green" is pointless. The Series of Natural Numbers possesses the properties formulated above, as a whole. Relations, too, may possess properties: example — the relation of transitivity. But at the given moment we are interested only in the properties of the elements of the structure under consideration (for which the axioms of Peano are satisfied). It is these properties, namely, that can provide the values of the variable P.

The fact that an element a possesses the property Q is described as $Q(a)$. If a property Q is defined on the elements of some set M, then it is possible to introduce for consideration a sub-set of this set, K — consisting of those and only those elements of M, which possess the property Q

$$x \in K \Leftrightarrow Q(x)\,. \tag{+}$$

Conversely, for every sub-set K it is possible to introduce the property Q : of "being an element of K", and again the correspondence (+) will be satisfied.

Thus, a property and a sub-set are almost the same : "the language of properties " and " the language of sub-sets" are trivially inter-translatable. For example, the axiom of induction would look as follows in the language of sub-sets:

$$\forall P\,\{\,[\,0 \in P \wedge \forall\, x\,(\,x \in P \Rightarrow x' \in P\,)\,] \Rightarrow \forall\, x(x \in P\,)\,\}.$$

Thus, in the axiom of induction, the domain of change of P is the totality of all the properties defined on the structure under consideration. We shall now see, how this axiom is utilised to ascertain the fact that a structure satisfying the axioms of Peano, is isomorphic to N. Thus, let a structure with the label $\{\,0\,,\ '\,\}$ satisfy the axioms I-III. Axioms I-II ensure the presence of a standard part $\{\,0\,,\ 0'\,,\ 0''\,,\ 0'''\cdots\,\}$ in the structure. Now let us apply the axiom of induction, having taken as a value of the variable P, the property P_0 of the elements of the structure : "to belong to the standard part". The axiom says that something is true for all P_0, in particular for this P_0. Thus, it occurs that

$$[P_0\,(0) \wedge \forall x\,(P_0\,(x) \Rightarrow P_0\,(x')\,]\Rightarrow \forall x\, P_0(x).$$

The premise enclosed within square brackets is evidently true (0 belongs to the standard part, and if x belongs to the standard part, then x' too belongs to it); that is why $\forall x\, P_0(x)$, i.e., all x (all elements of the structure !) belong to the standard part. We have already noted that the standard part is isomorphic to N. This concludes the proof of this that the structure under consideration is isomorphic to N.

Thus, any structure satisfying the axioms of Peano, is isomorphic to N, and consequently, these axioms define the concept of a series of natural numbers of the label $\{\,0\,,\ '\,\}$. Apparently, this situation contradicts our repeated announcement to the effect that it is not possible to formulate a system of axioms with such properties.

There are no contradictions here, and here's the reason why. Earlier we were speaking only about the properties of the Series of Natural Numbers, which could be expressed through a definite linguistic means — in other words, we were talking about some axioms written in a definite language. This language contained only one type of variables — the individual variables $x, y, z, \cdots$. The essence of these individual variables lies in the fact that upon being interpreted on any structure, each one of these variables gets one and the same set as its domain of change — it is the set of all elements of the structure under consideration. Another kind of variable — the variable P — takes part in the axiom of induction. Its values are not the elements of the structure under consideration, but a property of these elements (in other words — the one-place predicates defined on these elements, whence the variable P itself is called a *predicate* , to be more preceise — a *predicate variable of valency* 1). Thus, the axiom of induction is a formula of a n o t h e r, e x t e n d e d l a n g u a g e ; this language is more extended than the narrow language so far considered. (Narrow because it contains only individual variables). And when we said that there is no system of axioms, which fully characterizes a series of natural numbers, then we had this earlier, narrow language in view .

Of course, an explanation has been provided, but it hardly satisfies any one. What if it is not possible to write out a system of axioms for a series of natural numbers, in some language ? It is, as they say, "not a fact from the biography of a series of natural numbers, but rather one from the biography of that language ". Simply put, a narrow language is bad, and look, now we have found a good, extended language, in which it is possible to write the adequate axioms for a series of natural numbers.

Everything, however , is not that simple. Crudely speaking, the situation is just the opposite: a narrow language is "good", an extended one — "bad".

Let us attempt an explanation of the situation. We shall begin with terminology.

The formulas, wherein all the variables are individual are called *elementary* formulas, and the language that permits of only the elementary formulas, is called an *elementary* language. In the given context, the synonym for the term "elementary" is the term "1st order" or "first order". All the axioms considered above, save the axiom of induction (i.e., the axioms 1-8 and I-II) were elementary axioms, i.e., elementary formulas. There exists no (neither finite, nor infinite, and besides of any label) system of elementary axioms, which would satisfy the Series of Natural Numbers N and, all the models of which would be isomorphic to N.

There are non-elementary formulas, but they belong to a non-elementary language. Variables of a more complex nature are permitted in this language — predicate variables of valency 1, properties (= one-place relations) serve as their values; predicate variables of valency 2, binary (= two-place) relations serve as their values etc., and also, functional variables (any one-place operation, like, say, "to follow", may serve as the value of a functional variable of valency 1, and any two-place operation, like, say, addition, may serve as the value of a functional variable of valency 2). The axiom of induction is an example of a non-elementary formula. A more precise non-elementary language, having the possibilities just described, is called a *2nd order language* : this means, that it admits of variables covering relations and operations (what sort of relations and operations, that must be defined on the elements of the structure), but does not consider more complex variables, as the values of which may serve, say, the properties of operations or operations on relations (or the properties of relations such as "transitivity"). The axiom of induction serves as an example of non-elementary formula of a 2nd order language (or simply, as an example of a 2nd order formula).

A second order language is the simplest of all non-elementary languages.

One would think — and the presence of the axioms of Peano somehow confirms this — that it is possible to have a system of non-elementary axioms of 2nd order (i.e., axioms, written in the form of formulas of this non-elementary language), defining the concept of a series of natural numbers, in the following precise sense:

1) N is a model of this system; and

2) any model of this system is isomorphic to N.

However , here arises an unexpected , but quite fundamental semantic (one may even say, epistemological) difficulty. The fact is this, that already in the case of a 2nd order language (not to speak of the more complex non-elementary languages), the very concept of a model loses its essential clarity. The following example, connected with the so-called problem of the continuum, illustrates this situation.

It is well known that the quantity of elements of any set is called the *cardinal number* or the *cardinality* of that set. The concept of a cardinal number or cardinality is a generalisation of the concept of natural numbers, in so far as the natural numbers are the cardinalities of finite sets. From among the infinite cardinalities the following two are singled out: the cardinality of the set of all natural numbers and the cardinality of the set of all real numbers. The first is indicated by $\aleph_0$ (read "aleph- null") and is called the denumerably-infinite (or infinitely denumerable) cardinality; the second is indicated by c (small gothic "c") and is called the cardianality of the continuum. Evidently, $\aleph_0 < c$. The famous *continuum problem* consists of explaining whether or not there exists any intermediate cardinality, i.e., a cardinality satisfying the inequality

$$\aleph_0 < m < c.$$

The famous continuum hypothesis consists of this, that such a cardinality does not exist. (On the strength of the results of K.Gödel and P. Cohen) it is well-known that it is n e i t h e r p o s s i b l e t o p r o v e, n o r d i s p r o v e t h e c o n t i n u u m h y p o t h e s i s. While speaking of "proving" and "disproving", we have in view all the conceivable means permitted in modern mathematics. Thereby, the question of the very meaning of the continuum hypothesis remains unsettled. Indeed, the meaning of such a statement is taken to be vague — its truth or falsity can not be determined in any way. This extraordinary situation is radically different from such situations, often to be met with, when we simply do not know something (though we understand the question very well). [And the security of clarity in understanding a question lies in the clarity of understanding the possible answers.]

It appears that we may write out a formula of the 2nd order, which then and only then has a model (i.e., such a structure , for which it becomes true), when the continuum hypothesis is true.

It is also possible to write such a formula of the 2nd order, the existence of a model of which is equivalent , on the contrary, to the existence of an intermediate cardinality, i.e., to the truth of negation of the continuum hypothesis. [*A note for the specialists*. Examples of such formulas, known to the present author , contain predicate symbols of valency 2. However, if in the axiom of induction we change the prime symbol " ′ " into a predicate symbol, then this axiom too would contain a predicate symbol of valency 2.] Thus, for the formulas of 2nd order, the question of existence of their models may turn out to be as hazy as the continuum hypothesis itself.

That semantically so vague a language would be able to serve as a satisfactory means for axiomatically defining something — in particular , a series of natural numbers — appears to be doubtful.

And indeed, if we analyse the use of the axiom of induction in the process of proving that any model of the axioms I-III is isomorphic to N, then we shall see, that here , in essence, we are using that very concept of a natural number, which we still only intend to define axiomatically. Our property P_0 signifies "to have the form 0 ″. . . ′ ". The dots in the expression "0 ″. . . ′ " are only a substitute for the general notion of a natural number. And it is not possible to depict the property P_0 without an *a priori* notion of the natural numbers or without substituting that notion by dots or by the expression "etc.".

5. Is it possible to prove that Fermat's Great Theorem can neither be proved nor disproved ?

We have mentioned the continuum problem at the end of our last reflection. It is one of the major problems agitating the intellect of mathematicians. In the famous report entitled "Mathematical Problems", read by the great Hilbert in the year 1902, at the International Congress of Mathematicians in Paris, it was mentioned as the first problem. We have already noted that the continuum problem turned out to be unsolvable : it is neither possible to prove, nor disprove the continuum hypothesis.

While enumerating 23 basic problems of mathematics, Hilbert did not mention the problem of proving (or disproving) the Great Theorem of Fermat. Evidently, Hilbert did not consider it to be important enough. At the same time there is no doubt that it is the most famous among the unsolved problems of mathematics. And besides, unfortunately, it is unique among the unsolved problems, known to the wide mass of non-mathematicians. We wrote "unfortunately", since professional mathematicians spend an appreciable percentage of their time, studying and refuting the essays of the Fermatists — the name given to people, who do not have the necessary mathematical preparation, but who think that it is, namely, they who have proved Fermat's theorem.

Strictly speaking, Fermat's theorem should not be called a theorem. The "Matematicheskaya Entsiklopedia" [22] defines a theorem as a "mathematical statement, the truth of which has been established through proof".

A proof has not yet been found for Fermat's "theorem". [However, not every one supports this point of view. Thus, Viktoly Invanovich Budkin states in p.45 of his book "Methodology of Cognition of "Truth". A proof of Fermat's Great Theorem" (Yaroslavl: Upper Volga Publications, 1975. pp. 48): "13 generations have passed, and yet Fermat's Great Theorem still remains unproved. Only in the present work, a complete proof of the theorem is being given in its general form".]

What is more, the same "Matematicheskaya Entsiklopedia" contains an article entitled "Fermat's Theorem", in its 5th volume (the same volume that contains the aforementioned definition of a theorem). We too shall be using this generally accepted but imprecise term — though we admit that it would have been more correct to speak of the hypothesis of Fermat.

Many factors contributed to the popularity of Fermat's theorem among the non-professionals. These include : 1) the authority of its author : it has been stated by one of the originators of the theory of numbers — the famous French mathematician Pierre de Fermat; 2) the respect due to age: it was stated at around 1630; 3) the romantic circumstances of its formulation: Fermat wrote it down on the margins of the 1621 edition of the "Arithmetic" of Diophantus [the eighth problem of the second book of the "Arithmetic " of Diophantus reads — " To decompose a given square into two squares "; Fermat made the following comment on this problem — "On the contrary, it is not possible to decompose any cube into two cubes, any biquadratic into two biquadratics, and in general no power greater than the square, into two powers having the same index. I have discovered a truly wounderful proof of this, but these margins are too narrow for it"; this proof could not be found among the papers of Fermat]; 4) the setting up of a prize of 100,000 German marks for providing a proof of Fermat's theorem, in 1908 by Wolfskel (naturally, the "pleasant" fact of the institution of a big prize became much more well-known, than the "unpleasant" fact of its complete depreciation as a result of the post-first-world-war inflation); and 5) the simplicity of its formulation.

Of course, the first four factors could not have been effective in tandem, had not the formulation of Fermat's theorem been so popular. It is as follows : Whatever be the integer n, greater than 2, the equation

$$x^n + y^n = z^n$$

has no positive integral solution.

We see, that the equation present in the formulation of Fermat's theorem may be viewed as an equation with three unknowns — x, y and z. In sofar as n may assume any of the values 3, 4, 5, 6 etc., here, in fact, we have an infinite series of equations, and it has been stated that none of them has solutions in such integral x, y and z that $x>0$, $y>0$ and $z>0$. From the point of view of logic it would be more natural to consider the equation

$$x^n + y^n = z^n$$

as an equation with four unknowns x, y, z, and n. Then, Fermat's theorem would state that this equation has no integral solutions for $n>2$, $x>0$, $y>0$ and $z>0$.

The search continues for the proof(s) of Fermat's theorem*. Theoretically speaking, the search for its refutation could also have continued, but that is not happening. The situation with the hypothesis called "Fermat's theorem", is significantly different from the situation in respect of the continuum hypothesis : we know, that for the continuum hypothesis it has been proved that it can neither be proved, nor disproved (to be more precise, in 1939 Gödel showed that it can not be disproved, and in 1963 Cohen showed that it cannot be proved). For the hypothesis (theorem) of Fermat, such a proof — the proof that it is neither possible to prove, nor disprove it — does not exist. The question arises : whether this proof does not exist so far (with the

* A more recent example : Andrew Wiles' (June, 1993) claim to have provéd the Taniyama-Weil conjecture, entailing the solution of Fermat's problem. Experts are now examining this proof.—Ed.

hope that it will be obtained in future) or is it in principle impossible ? Had this proof been obtained, it would, undoubtedly, have been of great use for mathematics , as it would have closed, once and for all, the floodgates in the face of the flow of ignorant attempts to prove the theorem of Fermat. Unfortunately, such a proof is not possible. It is true, that there remains a theoretical possibility of proving that Fermat's theorem cannot be proved. The appearance of such a proof too would have closed the aforementioned floodgates — but then, perhaps, there would have emerged a flow of attempts to disprove Fermat's theorem (for example, by way of producing, in an oblique manner, four astronomically large numbers n , x , y , z for which the required equation would be practically unverifiable).

Thus, we are assuming, that (a) there exists a proof to the effect , that Fermat's theorem can not be proved; (b) there exists a proof to the effect, that Fermat's theorem cannot be disproved.

Now, our aim is to show, that (a) and (b) are incompatible, i.e., it is not possible for these two statements to be true at the same time. In fact we find that (b) is incompatible even with the weaker-than-(a)-statement (a_1) : "Fermat's theorem cannot be proved ". We shall show, namely, that from (b) the existence of a proof of Fermat's theorem follows and thereby (a_1) is negated.

First, some preliminary remarks. Let us agree to call, any four natural numbers n, x, y, z such that $n>2$, $x>0$, $y>0$, $z>0$ and $x^n+y^n=z^n$, the *Fermat four*. Fermat's theorem states that the *Fermat four* do not exist. Disproving any theorem is to prove its negation. [Thus] disproving Fermat's theorm would mean proving that the *Fermat four* exist. [As before we are using inexact terms and identifying the word "theorem" with the word "statement" and not with the expression "proved statement".]

Lemma 1. If it cannot be proved that the Fermat four exist, then they do not exist.

Remarks. Let A be a statement. There is no reason for thinking, that if it cannot be proved that A , then A is not true. However — and herein lies the essence of the lemma — that is the case, as soon as A is the statement that "the Fermat four exist".

Proof of lemma 1. We shall proceed from the opposite. Indeed, we shall assume that the Fermat four exist. Let us write out any of them — let it be the four natural numbers a, b, c, d. Let us verify that they really are the Fermat four, i.e. let us verify that the inequalities $a>2$, $b>0$, $c>0$, $d>0$, and the equality $b^a+c^a=d^a$, are satisfied. Presence of the four numbers a, b, c, d together with the indicated verification constitutes the **existence proof for the Fermat four.**

Lemma 2. If Fermat's theorem cannot be disproved, then fermat's theorem is true.

Remark. There is no reason why this must be true of any theorem.

Proof of Lemma 2. Lemma 2 is simply a reformulation of Lemma 1. "To disprove Fermat's theorem" is "to prove that the Fermat four exist", and to assert that "Fermat's theorem is true " is to say that "the Fermat four do not exist".

The lemma 2, which we proved, has the structure " if P then Q". That is why, if P has a proof, then Q too has a proof (the proof of Q consists of joining the proof of the lemma with the proof of P). That is why, we have the following

Corollary of lemma 2. If there exists a proof to the effect, that Fermat's theorem cannot be disproved, then there also exists a proof to the effect, that Fermat's theorem is true, i.e.,simply put, a proof of Fermat's theorem.

In view of the importance of this corollary, let us Formulate it once more : *if there exists a proof to the effect that Fermat's theorem cannot be disproved , then Fermat's theorem can be proved.* Thus, if (b), then Fermat's theorem can be proved, and this is the promised negation of the statement (a_1).

The contradiction thus obtained concludes our arguments to the effect , that (a_1) and (b), and even more so (a). and (b), are incompatible.

Here arises the following natural question: but why these arguments cannot be repeated for the continuum hypothesis ? Indeed, Fermat's hypothesis (theorem) states that the Fermat four do not exist, and the continuum hypothesis states that there exists no set having a cardinality intermediate to $\aleph_0$ and c. Now let us replace the Fermat four by a set of intermediate cardinality, and Fermat's theorem — by the continuum hypothesis and, let us once more adduce the arguments just adduced. We are bound to stumble somewhere , as the statements (a′) and (b′), obtained from (a) and (b), by substituting the words "continuum hypothesis " for the words "Fermat's theorem", are both true. And where shall we stumble ? Here's where — in the proof of lemma 1(evidently, not in the initial formulation, but with the replacement of the words "Fermat four" by the words "set of intermediate cardinality"). The aforementioned proof of lemma 1 was based upon the following idea: that it is in fact possible to produce the four numbers a , b , c , d and to assure oneself that they are the Fermat four. But what does it mean to produce a set ? Objections may be raised, that strictly speaking, we do not produce the numbers as quantitative categories, it is not possible to produce them, we can only write their names (for example, in the form of zero with the prime symbols or in the form of decimal notation). However, the fact remains, that each natural number has a name, but such is not the case with the sets : t h e r e a r e m o r e s e t s, t h a n t h e r e a r e n a m e s (if we understand the latter as finite combinations of the symbols of some alphabet). But even if we limit ourselves to the sets having names, and produce in place of the sets — these names, there remains, all the same, a major difficulty : how to verify that the set produced has an intermediate cardinality ? The verification to the effect that the four numbers are the Fermat four, is not complicated in principle (if we digress from the number of steps and the necessary space) : we just have to put the numbers in the equation and compare the left hand side with the right hand side. But there exists no way of determining the cardinality of the produced set or of determining whether or not this cardinality satisfies the inequality $\aleph_0 < x < c$.

The theme under consideration is most intimately connected with the famous incompleteness theorem of Gödel. This theorem states that *whatever be the proposed concept of a formal proof, there would be such a statement about the natural numbers, that neither it itself , nor its negation may be formally proved within the frame-work of the proposed concept.* We begin with the self-evidence of the fact that it is possible to define formal proof variously. These definitions differ from one another in respect of the collection of permissible axioms and the rules of deduction. It is possible to have such notions about formal proof, wherein there is no use at all, either of the axioms or of the rules of deduction. Briefly speaking, the approaches to the concept of formal proof may be very very different. But all these approaches have a fundamental generality expressed in the following principles:

1) every formal proof is a text — i.e., a finite chain of symbols, chosen from some alphabet;

2) in respect of every text, composed of the letters of an alphabet under consideration, it is possible to algorithmically identify, whether or not it is a formal proof , and if yes, then what, namely, does it state;

3) only true statements can have formal proofs.

On the strength of the third principle, the production of a formal proof of some statement guarantees its truth and, consequently, may be considered to be its proof. The converse, of course, is not being proposed: it is not being proposed that every true or even essentially provable statement has a formal proof, in terms of a pre-given concept of formal proof.

An analysis of Gödel's incompleteness theorem shows, that the statement therein discussed always has the form $\exists x\ u(x)$, where u is some property of the natural number x. This property depends upon the concept of formal proof under consideration, but it is always algorithmically verifiable (just as it is possible to algorithmically verify the property of "being the Fermat four", in respect of four given numbers), [being algorithmically, verifiable means — there exists an algorithm, which verifies for any c, whether or not $u(c)$ is true]. Thus Gödel's theorem states that neither $\exists x\ u(x)$, nor $\neg\exists(x)\ u(x)$ has a formal proof.

Let us make our demands about the notion of formal proof even more strict. Let us demand, namely, that as soon as the statement $\exists x\ u$ turns out to be true for some algorithmically verifiable property u, then and there this statement $\exists x\ u$ possesses a formal proof. This demand is quite natural : it is realized upon formalization of the following steps indicated above: 1) the production of some c ; 2) the verification that this c satisfies the property u ; here it is essential, that c may in fact be produced and, that $u(c)$ may in fact be verified.

Our demand follows from two even more natural demands:

1) if the (algorithmically) verifiable property u is valid for a number c, then $u(c)$ has a formal proof;

2) for any property u whatsoever, if for some c the statement $u(c)$ has a formal proof, then the statement $\exists x\ u(x)$ also has a formal proof.

Now, with the help of arguments analogous to those used in connection with Fermat's theorem, we arrive at the following conclusion: if neither the statement $\exists(x)\ u(x)$, nor its negation $\neg\exists x\ u(x)$ has a formal proof, then from this information alone about the given situation it is possible to find out which of these two statements is true: namely, it is true that $\neg\exists x\ u(x)$.

Indeed, had it been true that $\exists x\ u(x)$, then this statement would have had a formal proof; perhaps it is not true that $\exists x\ u(x)$, and it is true that $\neg\exists x\ u(x)$ [the words "true" and "correct" are synonyms, but the word "provable" has another meaning (even other meanings)].

Let us appreciate the paradoxicality of the situation once more: *from the sole fact that neither A, nor not-A has a formal prof, it is possible to conclude ,which of these two sentences is in fact true.*

6. What is a proof ?

When we read a book written some fifty years ago, then the arguments found there, appear to us to be largely bereft of logical rigour.

Jules Henri Poincare, 1908.

(Nauka i metod , kn. II, gl. 2, § 4; [2,s.356]).

In the previous reflection we came across the terms "proof" and "formal proof". It is sometimes thought that a formal proof is a proof that is formal. We would prefer to take a different look at these concepts.

A formal proof is a mathematical object, like, say , a matrix or a triangle. It is a finite chain of the symbols of some pre-fixated alphabet, i.e., as they say in mathematics, a *word* according to this alphabet. In the given instance, when we speak of a "symbol", we do not have in view the meaningful, contentful side, but only the external, graphic aspect of it is taken into consideration. To stress this circumstance, in mathematics, when the external, graphic aspect is had in view, then they speak not about a "symbol" [or "sign"], but about a "letter"'. Usually, the letters of the alphabets of various (Russian, Latin etc.) languages, numerals and the punctuation marks are considered to be letters. It would be reasonable, to consider the gaps among the words to be letters too (words in the ordinary, and not in the mathematical sense); we may devise some special symbol for it, for example # . This creates a possibility for viewing a text, i.e., a sequence of words, also as a word (in the exact mathematical sense indicated above). Thus, a formal proof is first of all a word in some alphabet — in the alphabet of formal proofs. It is clear, that this does not exhaust the concept of formal proof in the least: we simply wanted to stress that the concept of formal proof belongs to the class of words — just as the concept of triangle belongs to the class of geometrical figures.

What sort of words may be considered to be formal proofs ? That is the theme of a special discourse; it is beyond the cycle of topics we wish to discuss here. We stress here that it is possible to give various definitions of the concept of formal proof, each of which would lead us to its own set of formal proofs. In the previous reflection we have enunciated some general postulates, to which any reasonable definition should be subordinated. It must be mentioned, however, that sometimes yet another step is taken in the side of generality and it is not demanded beforehand , that only true statements should have formal proofs, thereby the concept of formal proof is fully separated from the concept of truth. And afterwards this discarded requirement is introduced in the form of a supplementary property (which a formal proof, generally speaking, may not have): namely, if all statements having a formal proof are true, then the set of formal proofs is called *semantically non–contradictory.* A more precise general notion of formal proof is enunciated with the help of the concept of deduction; see, for example, [21].

We would like to stress once more, that not the contentfully understood statements themselves, but only their representations (i.e., again words) may (or may not) have formal proofs , in some precisely given logico-mathematical language.

The definition of the concept of formal proof — perhaps, it would be better to say : the definition of the sets of formal proofs — within the broad limits (conditioned by the general limiting properties of the sets of formal proofs, indicated above), happens to be arbitrary. Here, we have in view that " juridical" arbitrariness, which distinguishes mathematical definitions in general. For example, we have the "juridical" right to arbitrarily define a class of functions and to call it, " as we wish ", say — continuous.

It is another matter, that any reasonable mathematical definition usually claims to correspond to some intuitive notions, to reflect them. Legitimacy of a definition still does not signify its reasonableness. Thus, the mathematical concept of a continuous curve reflects (with some sort of precision) our intuitive, contentful notions of the trajectory of a moving point. Analogously, the concept of formal proof reflects the intuitive notions of a contentful proof.

It may be said that the concept of formal proof is a mathematical model of the concept of proof — in the same sense , in which the concept of continuous curve is a mathematical model of the concept of trajectory.

It still remains to be explained : what a proof is . We have indicated at the very beginning of the present cycle of reflections, that it would be incorrect to assume that in mathematics everything is proved ; however, there is no doubt about the fact that the concept of proof plays a central role in mathematics . " From the time of the Greeks, to say 'mathematics' is to say 'proof' " — thus begins Nikolai Bourbaki his " Éléments de mathématique " [6, p. 23]. At the same time we have noted that the concept of proof does not belong to mathematics (only its mathematical model — the formal proof, belongs to mathematics). It belongs to logic , to linguistics and, above all — it belongs to psychology .

Thus, one of the most important terms in mathematics, the term "proof", has no precise definition. An approximate definition of it is as follows : *a proof is a persuasive argument, which so persuades us that with the help of it we become capable of persuading others* [12].

Having grasped a proof, we become aggressive to a certain extent, ready to convince others with the help of the arguments which we have grasped . If we are not so ready , then it signifies that we are yet to grasp the presented argument as a proof, and even if we have given it the recognition of a proof, then we have done so simply to brush aside something.

We find that the concepts present in our definition of a proof are either logico-linguistic ("argument"), or psychological ("persuasive strength ", " readiness") in nature. This fully meets the essence of the matter : the very notion of proof is inseparably connected with the linguistic means and with the social psychology of human society. And both of them change in the course of history . Linguistic formulations of proofs change. Our notions of persuation change.

The notion of persuation depends not only on the epoch, but also on the social surroundings. Unfortunately, I am unable to recollect now, where I read a passage on the following theme. The Cardinals of the time of Galileo, were quite intelligent, some of them saw with their own eyes the mountains on the moon through Galileo's telescope, and could follow the logic of Galileo's arguments. However, for them, their own views, based on an a *priori* dogma, were more convincing than any experiment and any logic . [In an article by S.P. Bozhich [13], we find an interesting analysis of how an *a priori*, predetermined notion about the ways of proving things prevents the recognition of certain facts.]

The notion about the persuasiveness of this or that argument depends on many factors. Revealing these factors happens to be an important task of logic and psychology. For example, the division of concepts (to be more precise, of terms) into sensible and senseless ones, happens to be one of these factors. The concepts of phlogiston and thermogen were considered to be meaningful in the 18th century, but they are now considered to be meaningless. Einstein discovered that the concept of simultaneity of two events is meaningless , as an objective concept independent of the observer (to be more precise, he discovered that simultaneity is not a two-place relation between two events, but a three-place relation involving the 1st event, the 2nd event, and the observer, as its terms). On the other hand, such an "evidently meaning-less concept" as the infinitesimal number, now fits into an exact meaning within the framework of a new branch of mathematics — the so-called non-standard analysis. With the changes in the notions about meaningfulness or meaninglessness of concepts, the notion about the very essence of scientific truth also changes. The notion of evidence too changes. Once everyone knew that " higher forces " unleash storms, now everybody knows that storms are caused by atmospheric electricity. In the case of inert gases, their property of not taking part in any chemical union was so evident, that this property was fixated in the very name "inert"; when , in 1962, these gases were for the first time found to take part in chemical unions, then , apparently, the chemists were not ashamed, rather — they happily stated that "for explaining the structure of these unions, we did not need any, in principle, new notion about the nature of chemical bonds " (The Great Soviet Encyclopedia, 3rd ed. the article on the "Inert Gases ").

It is common ground that human knowledge changes with the march of history. Here one would like to stress that not only the facts themselves go into the composition of knowledge, but also the initial positions and presumptions, on the basis of which this or that fact becomes a component of a system of knowledge : these are the notions about meaningfulness and meaninglessness , about obviousness and non-obviousness, of the possible and the impossible, of the part and the whole, persuasiveness and the lack of it, the proved and the unproved and, the authentic and the inauthentic. It is possible , that all these notions change more slowly than the simple notions about facts ,but in essence , they are historically as relative as our notions about facts.

Mathematics is sometimes perceived as a stationary rock towering above the waves of changing notions about the other disciplines. Of course, there are grounds for such a view of mathematics. At the same time, the notion of some absoluteness of mathematics is evidently exaggerated. If mathematics is absolute, then it is so only at the level of everyday experience — just as Newtonian physics is absolute in its application to the phenomena of "medium size" (and yet another — Einsteinian — physics operates at the level of the small and the very big) [see the already mentioned article of P.K. Rashevsky [16] : on "diffusion in the large " notions of the natural number].

In particular, the socio-historical conditionalities of the notions of proofs are on the whole extended upon mathematical proofs.

To illustrate what has been said above, I shall now briefly narrate my understanding of the concept of proof in ancient Egypt, ancient Greece and in India.

We do not have much authentic information as to how mathematical proofs were enunciated and understood in the ancient period. The texts that came down to us are in many cases fragmentary : moreover the terms contained therein often have debatable interpretations ; for example , on the interpretation of ancient Egyptian mathematical texts, see the remarks of the translator in : [4, p. 139]. There is a lot that is conjectural. Every one makes conjectures in the direction s/he wishes, and the present author is no exception. Taking these stipulations into account, the following outline may be proposed.

The proposed outline is based on the conviction that the notion of proof is a product of the history of societies. We are aware of the simplification involved in our historical excursus,as we describe ancient Egypt as a centralized state — since there have been periods of splintering there, or ancient Greece — as a democracy, since there, too, there have been cases of tyranical or oligarchic rule. But then, any outline involves some simplification.

Ancient Egypt. A centralized theocratic state , with an extraordinarily strong discipline. Continuous construction of pyramids — requiring colossal human and material resources and uniting the strength of the entire land — served as an effective instrument for maintaining centralization, discipline and order. Authority of the Pharaoh and of the priests was incontestable. Authority of the written word was also unquestionable. If a priest , scribe or teacher said or wrote something, then that means — such is the case. If something is written on papyrus, then that is the case. Persuasiveness was based on the authority of the source.

Ancient Egyptian mathematical texts contain ready-made recipes without any substantiation. When we speak of absence of substantiation, here we have in view the modern understanding of the word "substantiation". From the point of view of a person of that time a recipe on a papyrus was fully substantiated , as it came from an authoritative source and was drawn up in the authoritative form of a record on papyrus. The fact of being recorded on a papyrus, was in itself the proof. In reality, this fact was enough for convincing others with the help of it. A number of recipes for computing the areas of triangles and quadrangles have been non-univocally interpreted in our time; the disputes about how to understand the terms contained in these recipes, still continue [4, ch.IV, §2, a]. Depending upon these interpretations, these formulae may be taken to be either exact, or approximate, or totally incorrect. When we speak of incorrect formulae, here, we have in view the representation of the area of a tringle through half of the product of the base and a side of it. This is what academician L.S. Pontryagin has to say on this score : " The first mathematical manuscript known to us — is the manuscript of Ahmes, composed some 2000 years before our era. It contains some algebraic and geometrical rules — for example, for computing the area of a triangle ... However, the Ahmes Papyrus contains a mistake. According to him the area of an isosceles triangle is equal to the product of its base and half of a side — but to-day every school-student knows that it is not true" [25]. However, many a researcher thinks that the corresponding ancient Egyptian term should not be translated as a side , it should be taken to mean height (and then the formula contained in the papyrus turns out to be true. However, even if this term did in reality signify, not the height, but a side, the corresponding (according to our modern point of view incorrect) formula should be considered as proved according to the ancient Egyptian understanding of the word "proved ": as this formula is convincingly substantiated by the fact that it (of course, not as a formula, but as a recipe expressed in words) is contained in an authoritative document.

The situation was somewhat different in *ancient Greece.* (In comparison to Egypt) here we have comparatively small state formations together with popular assemblies. The orators, who spoke in these gatherings did not carry any *a priori* authority. They had to convince the listeners by arguments. Formulating correct arguments became an everyday and actual requirement. Hence the birth of logic in the hands of Socrates, and its final shaping as a discipline by Aristotle. Hence also, the beginning of the deductive method in mathematics, approaching the modern notion of proof. Arguments became the basis for mathematical conviction. The concept of the foundations of correct arguments, of axioms and postulates, arose . That which could be obtained through "legitimate arguments" from the initial statements, considered to be valid, was considered to be convincing (and consequently, proved).

Finally, *India*. We intend to refer to some geometrical figures, taken from mediaeval Indian texts, but that does not mean that these figures did not appear in ancient India. In general, the task of dating of Indian mathematical notions gives rise to considerable difficulties, as some texts may be expositions of some other earlier texts. On the other hand , it is not so essential either: mediaeval Egypt and Greece had nothing in common with ancient Egypt and Greece, but mediaeval India remained the custodian of the intellectual heritage of ancient India. An essential trait of this tradition was (and is) the conferring of the status of highest authenticity to the inner light. Immediate inner illumination was considered to be the basic source of knowledge and it had indisputable persuasive power. That which was thus known was considered to be proved. In order to convince others of it, those others must be brought to such a state , that they themselves experience the inner illumination. That is why, a geometrical proof had two parts : a diagram, and below it the inscription — " See !".

We find the examples of such diagrams with the inscriptions "See !" in some texts dating back to the 12th-16th centuries [9, p. 76 and 154]. We reproduce below one of these diagrams; it has also been reproduced in : [15,p.75]. We are of the opinion that it deserves to be included in any modern, secondary school level, text book of geometry : it shows, more graphically than the modern proofs, that the area of a circle is equal to the area of a rectangle , the two sides of which are respectively equal to half of the circumference and half of the diameter of the given circle. See figure 5 below.

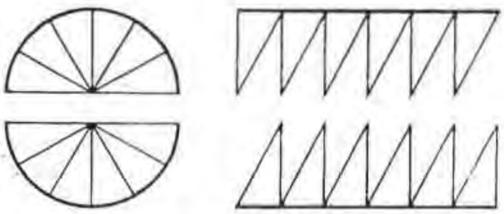

Fig 5.

The present author is aware of the fact that his views on the Indian proofs are different from those of an authority in the field of history of mathematics, like A . P. Yushkevich, who wrote : " The laconism of inferences in the Indian works on mathematics or the presence in them of diagrams together with only the inscription "See !", should not be viewed as the manifestation of some special approach to the problem of proof or of some special movement of thought "[9,p. 155]. We are of the opinion that they should be so viewed. Or else, why do we not come across this kind of "See !" anywhere else ? Why only in India ?

S.S. Demidov has put forward valuable considerations on the evolution of the concept of mathematical proof in [15], where, in particular he states that, "in the final count, the proof (giving power) of mathematical arguments is their persuasive power. What appeared to us to be convincing yesterday, does not appear to be so to- day".

The definition of proofs as convincing / persuasive texts makes the concept of proof very subjective (for some, a text is convincing, for others — not). We do not consider it to be a deficiency of the definition. Such is the state of affairs. Perhaps, the use of the word " makes " above has been unfortunate. Our definition *does not make* the concept of proof subjective , it *only reflects* the subjective character of this concept . Even more interesting is the problem (we are very far from solving it), as to why, nevertheless, the concept of proof has an universal-cultural character in the sense, that within the limits of one and the same culture, though there occur disputes about whether or not this or that statement is true — such disputes are comparatively rare.

While speaking of such disputes, we do not have in view the disagreements among the representatives of various logical trends in mathematics, for example, those among the representatives of the ordinary, classical mathematics and the representatives of the intuitionistic (constructive) mathematics. The latter do not recognise many statements of ordinary mathematics as proved (and, on the contrary, consider them to be untrue). It may be said that the intuitionists and constructivists belong to different mathematical cultures and even the most customary words (like, say, "exists") has a different meaning for them (evidently, the intuitionists and the constructivists think that the representatives of traditional mathematics put different meanings into words , and it is they — the intuitionists — who use these words in their only correct meaning). That is why the intuitionists consider many proofs of traditional mathematics to be invalid.

Here we are talking of something else — not about changes in the semantics of terms, leading to the changes in the truth values of statements, but about the fact that a proof may turn out to be not understood and that is why not convincing (and once not convincing — it is not a proof at all). Modern mathematics has a complex structure, which has almost stopped to be visible. The proofs of some of the theorems turn out to be so cumbersome, that in order to be able to verify them one must have an extraordinarily big desire, patience and time, to say nothing of the fact that one must have special knowledge — for a number of theorems, not only the invention of their proofs , but even the verification of these proofs appear to be accessible, only to a narrow circle of refined specialists.

Sometimes the volume of the proof of this or that theorem becomes an object of interest. Here,we often find that some theorems established earlier — which are no more required to be proved — are permitted to be used as ready-made formulations in a proof. Will such an argument be a proof — i.e., a convincing text — for some one who does not know the proofs of these theorems " established earlier "? We do not intend to give an univocal answer to this question. We would like to mention further , that the very word " earlier " introduces an additional subjective "relativistic" moment (two almost simultaneously proved theorems may be chronologically differently ordered by different observers). If any reference to any theorem whatsoever proved earlier,is forbidden in a proof and if one is required to go back directly to the definitions and primary, undefined concepts (which we have discussed in our first reflection), then such a complete proof, may, in a number of instances, stretch into thousands of pages of mathematical text (and may even be more difficult to perceive, than a proof based upon clearly formulated — though not known to the reader — facts).

The study of difficult mathematical proofs may be compared with a mountain-climber's ascent to the peak. The sea-level corrresponds to the initial concepts. Ascent from the sea-level may take months, and its mathematical analog (understanding a proof) may take years. In both the cases, there are many intermediate stops. First, you go to the common high-altitude base camp, here the climbers going to the various neighbouring peaks gather. This stage corresponds to the stage of serious mathematical preparation, sufficient for acquiring an understanding of the more special themes. Then begins the assault to the chosen peak ; here again, we have intermediate camps and stops. For mathematics, the corresponding theories and theorems play the role of these camps and stops. Just as a mountaineer may make a limited number of ascents in his life time, so may a mathematician — get to know only a limited number of proofs.

The following common trait of the mountaineer and the mathematician is an important one — there is some kind of conventionality involved in the choice of the point of departure. The ascent proper does not begin at the sea-level , but from a point, where the professional mountaineers may be able to gather without difficulty, but it may be a matter of great difficulty for an ordinary person, if s/he wants to arrive there. The proof proper begins from an analogous point : this point is situated at some general-cultural (we have in view the mathematical culture) level. However, at the present stage of mathematics the generality of the prefix "general- " is being lowered continuously, and now many proofs begin form a point, accessible only to the narrow specialists. A second common trait consists of a break up into stages, the presence of sufficient number of intermediate stops (camps).

Where from does one get the conviction in mathematics, that the proved theorems — the proof of which one never gets to know — are indeed proved , i.e., have proofs ? Evidently such conviction is based on trust alone . Seen from outside, such a situation should not appear to be very strange. Indeed how many of the readers of these lines have seen the Easter Island ? For those who have not, the conviction that this island exists, is also based , in the final analysis, on trust. But if a modern proof is based upon trust in authority, then how is it, in principle, different from the ancient Egyptian proof ?

It is not a simple question. Perhaps, the answer to it lies in the fact that proofs are gradually moving over from the ranks of the phenomena of individual experience to those of the phenomena of collective experience. Pushing the collective to the fore is in general characteristic of the history of civilization. It is well known (and widely discussed) that with the development of human society, there arises division and cooperation of labour and, this gets strengthened steadily. Only in the deep antiquity could man himself produce all that he needed: now everybody is required to use the results of the labour of others. It is known (though less discussed) that division and cooperation of scientific knowledge takes place simultaneously. It is difficult to say when — perhaps in the middle ages — one could have found individual scholars, capable of grasping the totality of the knowledge of his time. Now everybody must, this way or that , use the knowledge of others. The situation with proofs is an analogous one : the activities in the sphere of production and consumption of proofs have become as much an object of division and cooperation of labour, as are the activities in the sphere of production and consumption of knowledge. The very concept of conviction has begun to lose its individualized nuance and, is more and more assuming the character of "collective conviction". Evidenty, we must gradually learn to speak of the conviction, of not a separate individual but, of some scientific collective. Here , collective conviction does not in any way signify that it is equal to the "immediate conviction " of each of the individual members of the collective. A collective does not function as a simple totality of its members, but as a single whole . The idea of collective conviction indicates the fact, that for every component part of a proof we have a member of the collective "answerable for it ", who is immediately convinced , namely, about that part (and other members of the collective rely on this member on that given question).

The age of informatics is introducing its own correction, also to the notion of proof. For instance, there arise situations when a proof involves sorting out of such a large number of variants, that it becomes difficult for a human being to do the sorting, but a computer can do it. Let us assume that a computer sorted out all the required variants , and the sorting led to the necessary results. Can we then say, that we have obtained a proof ? And what if the machine "malfunctioned"? (But a person also makes mistakes !) That apart , a guarantee is essential to the effect that the programme was right; correctness of programmes can be ascertained only with special proofs , and the theory of such proofs constitutes a special division of the theory of programming.

In reality, computers were used to solve the four colour problem. [For a formulation of this problem, see: the article "The four colour problem ", in the 3rd edition of the Great Soviet Encyclopedia.] In terms of simplicity of formulation, this problem, consisting of a proof of the four colour hypothesis, is hardly inferior to Fermat's problem (consisting of a proof of Fermat's hypothesis), but in terms of the naturalness of the statement (and the applied significance) it is superior to Fermat's problem . The solution of this problem was announced by Appel and Haken in 1976 [17] and set forth in 1977 [18 and 19]. This solution is based upon a reduction of the solution into a large number of particular instances, the study of which was entrusted to computers. The computers verified all of them, and thereby it was proved that every map is four colourable, as per requirement.

Appel and Haken themselves said about their proof [20]: "The proof involved an unprecedented use of the computers. The calculations used in the proof made it longer than what is traditionally considered to be permissible. In fact, the validity of the proposed proof can never be verified without the help of computers. What is more, some of the decisive ideas of this proof materialized through computer experiments. It is posssible , of course, that one fine morning there would appear a short proof of the four colour theorem ... At the same time, it is also conceivabale that such a short proof is not at all possible. In the latter case, new and interesting types of theorems emerge, for whom traditional types of proofs do not exist ".

Of late , however, the validity of the proof provided by Appel and Haken came to be doubted. The doubt is not about the computer-use part of it, but about the pre-computer, theoretical part — wherein it is sought to be established that the entire problem is really reducible to a consideration of the particular instances.

C o m m e n t a r y . Let us describe the situation involving the proof of Appel and Haken in somewhat greater detail. The basic idea of its authors is connected with the following notions.

First of all, the authors go over from the colouring of the regions in a map to the colouring of the apexes in a planar graph, such that it is a triangulation. Further, they call any sub-graph, forming a cycle, and the interior of that cycle — a configuration. If it can be proved by some standard method, that a configuration can not be immersed in a minimal counter-example of the four colour hypothesis, then it is called — *reducible*. If each planar triangulation contains one of the configuarations of a set as a sub-graph, then that set of configurations is called *unavoidable*. From these definitions it easily follows that for obtaining a (positive) solution of the four colour problem it is enough to produce an unavoidable set of reducible configurations. The authors of the proof produced 1834 explicit, reducible configurations, forming the unavoidable set [19,pp.505-567]. In each of these configurations, the length of a cycle was 14 or less. Computers were used both for finding the unavoidable set and for proving the reducibility of its terms.

If in the first case (construction of the set) the computers were drawn into a helping role , since the very proof of unavoidability of the set obtained (now it is not important, how that was done) is not based on computer-calculations, then in the second case (of verification of reducibility) the use of computers happen to be an essential component of the proof , and each configuration needs some 10 minutes of computer time for such verification. While evaluating the proof of Appel and Haken, some reviewers indicated [23] that the authors of the proof needed four years and 1200 computer hours for its construction and, that the text of the proof takes 139 pages, including 99 pages of drawings, the average size of more than 30 of these drawings being one page. The reviewers commented that the "essentially search type character of the proof makes its verification difficult (according to Appel the verification of all the details requires 300 computer hours)". Evidently, these 300 hours are required for the verification of reducibility. However , as we have already mentioned, the non-computer part of the proof — involving the verification of the unavoidability of the set of configurations produced — gives rise to doubt. The fact remains that the text of the proof [18 and 19] does not present this verification directly and exhaustively. We have been informed through a foot-note on p.460 that the details of the proof of unavoidability of the presented set (to be more precise, details of the proof of the so-called spacing out theorem, which provides the basis for this unavoidability), are contained in the microfiches, supplied as a special supplement to the journal. However, the present author could not go through that supplement.

It seems that with the development of mathematics (and with the appearance of ever more complex and long proofs) the proofs are losing an important trait — that of being convincing. One fails to understand , then what remains of the proof : conviction /persuasiveness enters into the very definition of proofs ! That apart , with the growth in the complexity of proofs, their element of subjectivity grows too. Of course, a f o r m a l proof is objective. But, firstly, not the judgements themselves, rather their expressions, their representations in formal languages, that have formal proofs. Secondly, though the verification of the statement, that a given text is a formal proof, is accomplished algorithmically, it may give rise to considerable difficulties, in the case of a voluminous text.

Large proofs begin to live by some macroscopic rules. Just as the concept of natural number gets diffused in the case of the "large" numbers (once more we refer the reader to P.K. Rashevsky's article) [16], so does our notion of a proof; it gets diffused , when the volume of a proof becomes inordinately large.

It so happenes, that though all proofs should, by definition, be convincing, some proofs are more convincing than the others, i.e., as though, some happen to be, to a greater extent proofs, than are the others. There emerges something like a gradation of proofs according to the degrees of demonstrability — of course, such an idea fundamentally contradicts our primary notions about the identical indisputability of all proofs. But mathematical truths do permit a gradation of that kind. Each of the three following statements — "$2 \cdot 2 = 4$", "$17^{14} > 31^{11}$" and "$300! > 100^{300}$" — are true. However, we say : " True as $2 \cdot 2 = 4$ ", and do not say : " True as $17^{14} > 31^{11}$" or " True as $300! > 100^{300}$ " .

7. Can mathematics be made understandable ?

Why so many people do not understand mathematics? The great Poincare was disturbed by this problem and, he wrote: "How to explain, why many an intellect refuses to understand mathematics? Is it not paradoxical? Indeed.... here we have a problem , that does not lend itself to an easy solution; all those who wish to devote themselves to teaching must take up this problem" [2,p.353] .

Most probably both the sides involved "are to blame". Non-mathematicians are to blame: a bad education has trained them into a non-understanding of and even into taking a hostile attitude towards mathematics (as Poincare has noted " often the intellect of those people who are in need of guide lines, is very lazy to seek them out") [2,p.354]. Mathematicians are to be blamed: they do not wish to waste their strength, explaining their mathematics to the uninitiated (and how many people are astonished to find , that there still remains something to be discovered in mathematics !). Of course, in mathematics there would always remain numerous details inaccessible to the non-professionals (and even to the professionals, of a different field of mathematics). But such is the case everywhere — for example, in chess, even the other grand masters do not understand many a move, when Karpov and Kasparov battle against one another. At the same time, a very large part of mathematics, larger than what is usually thought to be the case, may be explained to a wider circle of well-meaning listeners and readers — of course, not in detail, but at the level of the herart of the matter. Clearly, this would require that the mathematicians engage themselves single-mindedly in this new direction of activity. Perhaps, thereby they would be discharging their moral duty to the humankind.

"But in order to help those who do not understand, first of all, we must know what restrains them" [2,p.345]. It appears, that the complex logical structure of mathematical definitions and statements, in which the logical connectives and the existential and universal quantifiers take turns, happens to be the hindrance in many cases. Every teacher of mathematical analysis knows the difficulty that arises in the course of parallel assimilation of the concept of limiting point of a sequence — the definition of which has the structure

$$\forall \varepsilon \forall k \exists n (A \wedge B),$$

and the concept of limit of a sequence — the definition of which has the structure

$$\forall \varepsilon \exists n \forall k (A \Rightarrow B).$$

However, are these psychological difficulties encountered by the learners, while assimilating these concepts — difficulties pertaining to the heart of the matter or, are these difficulties of linguistic expression? I do not have any final answer to this question. It is connected with an even deeper question: is it possible to separate mathematics from its linguistic formulation? In other words, does mathematics abide exclusively in the mathematical texts or does mathematics have some other essence, differnt from the texts — and the texts serve only as this or that (and perhaps not always felicitous) mode of expression for that essence. It is clear that this question, which we have called a "deeper" question, is appplicable not only to mathematics, but also to any other discipline *. According to a formulation of Engels, mathematics is different from the other disciplines in so far as it is "an abstract science, dealing with intellectual constructs" [1,p.529]. [These intellectual constructs can hardly be understood by the human intellect, if they are not based on ordinary human logic, and consequently — on reality, from the operations with which , this logic has come into being.]

Like all rational concepts, the mathematical concepts too exist in the form of notions, not necessarily connected with texts. The linguistic texts defining these concepts should be recognized as important, but not as the only, means for their assimilation.

It appears, that now we have at our disposal more adequate means of introducing the concepts of limit and limiting point of a sequence, to those learners (who do not have special "mathematical capabilities" — that is, according to modern understanding, to those who do not have a high capability of assimilating, namely, linguistic formulations). Let us imagine a screen, on which we may draw the trajectory of the movement of a point, unboundedly approaching some other stationary point, which is the limit. This has to be repeated a number of times, with changes in the position of the limit (so that the false impression is not created, that every sequence has one and the same limit), as well as in the mode of approach of the moving point to the limit (so that, in particular, the false impression is not created, that the distance between the moving point and its limit changes monotonically).It is possible to present an analogous graphic illustration of the concept of limiting point: when, though the trajectory unboundedly approaches that point at times — at others, it moves away from it by a definite distance. It appears very likely, that any viewer of such pictures would form a correct notion both of the limit and the limiting point.

One is led to believe that with the introduction of computers, teaching will proceed along the path of visualization of concepts, traditionally considered to be entirely abstract.

Had the theme under consideration been one of pedagogical significance alone, then we would not have dwelt upon it so elaborately in an essay of philosophical character. However, this theme exceeds the bounds of pedagogics and, closes up to the question of ontological nature of mathematical concepts. Like all other rational theoretical question, this question too has an applied significance — in the given case, in the order of reverse connection, it is pedagogical. Indeed, if a mathematical concept has an essence, different from its embodiment in a linguistic definition or formula, then one can hope for a better understanding of that essence, by demonstrating its various manifestations (and not only its formulation).

In order to adduce a proof, we shall consider a fresh example. On pp.71-72 of a recently published text book [24], there is a formula that defines a mathematical concept — the so-called Clark's cone. Having formulated its definition the authors wrote : "However, at the first glance, it is neither possible to understand the properties of Clark's cone, nor the meaning of its formal definition itself". And further on, they have at first put forward some heuristic arguments explainning Clark's cone, and then translated these arguments in the language of non-standard analysis. Here one gets the idea that as though the concept of Clark's cone exists all by itself;

* Enter Jacques Derrida and post-structuralism in Mathematics?—Ed.

its definition in the form of a formula is only oné of the means (and not the most felicitous means) of comprehending this concept, but descriptions like the "results of examination of the set through a microscope" [24.p.86], are useful for a better understanding of it.

Independently of the fact, whether or not such is indeed the case, we may put forward the following fruitful working hypothesis: a truely profound mathematical concept or mathematical statement must in essence be simple. And then there is a hope, that it will be understandable (or better still, understood): it is easy to get used to that which is simple, and we do not know any interpretation of "to understand", other than "to get used to".

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EMERGENCE AND DEVELOPMENT OF THE CONCEPT OF CONSTRUCTIVISABILITY IN MATHEMATICS

NIKOLAI NIKOLAEVICH NEPEIVODA

Constructivisability of a mathematical theory signifies the possibility of isolating the constructions of objects from their existence proofs.

Pre-Greek empirical mathematics was constructive by its very nature. It was preoccupied, namely, with the means of construction of objects, and gave empirical recipes in certain situations. Mathematical reasoning was reduced to one or, in the extreme cases, to several applications of such recipes, and the only descriptive element in it involved judging whether or not the problem at hand or, part thereof, belongs to a certain class. This descriptive element was most often reduced to an appeal to immediate obviousness.

Construction of the object being sought was the only method of proof in the Indian and Chinese mathematicses, and this construction was able to take the place of arguments (we recall the famous Indian diagrams with the word "see"). Arguments could only help in construction, they did not have any independent significance [1].

The concept of proof came to occupy a proper place in Greek mathematics. Classical logic was used with all its might. It has been established in the 20th century, that this logic was suitable, in the first place, for describing the static universe of ideal concepts, and not for carrying out intellectual constructions. Though Aristotle did highlight the special logical status of the rule of contraries : "... one of these [direct proof] proceeds from the previous [knowledge], and the other [from the opposite] from the subsequent" [2, p. 307] — this remark, which astonishingly exactly reflects the semantics of the rule of contraries in Kripke's models, did not exert any influence upon strict mathematical arguments.

Nevertheless the use of classical logic — an instrument, oriented towards descriptive and not constructive applications — did not lead Greek mathematics to non-constructive methods and theorems. As before, existence proofs included (as a rule, geometrical) constructions. Arguments from the contraries were used only to substantiate the constructions already carried out, in the main, for proving the equality or inequality of certain magnitudes.

The deeper reasons behind this phenomenon were revealed only in the last few decades. It is intimately connected with the hold-up of the Greeks in front of the concept of real number, from the point of view of traditional mathematical paradigm — with their strange antipathy to the explicit use of numbers in general, in strict mathematical arguments. Vexations regarding the specificities of Hellenic mathematics — which are indeed not quite understandable from the classical point of view — have been expressed more than once. In particular, the question arises : why the real numbers were used in a masked manner, as proportions, and why acquaintance with incommensurability did not come in its way, and yet in geometry, the natural numbers were avoided in all possible ways, though, one would think, that these are sufficiently intuitively reliable objects? Why, the Hellenic arithmetic remained something like a handicraft or an art,. never entering into the sphere of operation of "pure mathematics", save in the case of a few theorems like the one about the infinite set of prime numbers? What prevented the Greeks from formulating and utilizing such a powerful principle of conducting arguments, as the mathematical intuition ?

It has been proved, namely, that the classical geometry and the elementary theory of real numbers are complete and solvable ; see, for example, [3]. Thus, for every concrete, closed statement in the language of these theories, it is provable in them, that it is either A, or $\neg A$. Consequently, classical logic can not lead us to the non-constructivisability of the theorems proved either in geometry, or in geometry supplemented with algebraic operations on the real numbers, but without the explicit mention of the integers as a set. In any classical proof of these theorems one may mark out the construction and its substantiation, which may be carried out, in particular, also by the method of "indirect proof".

This fact once more confirms the depth of the intuition of the Greeks, which was based upon purely aesthetic and methodological considerations, but which permitted them to stop, namely there, where the rupture between argument and construction, between descriptive and constructive knowledge, became important. Such an exact halt was conducive to the fact that, the distinction between what was constructive and what was descriptive was not realized and, was correspondingly erased out of the world outlook of mathematicians. Perhaps it gave an indirect push to the courageous introduction of numbers and their functions in the mathematics of the modern times : mathematicians were still unaware of the danger of a rupture between the proof and the construction, it was erroneously accepted that [the verbs] "to prove" and "to construct" were always mutually concordant. Consequently, as before, mathematicians assumed — now, without any foundation, simply due to inertia — that a strictly proved statement provided the means for the construction of those objects, whose existence has been affirmed. When the construction was explicitly indicated in a poroof, then that was, of course, rated somewhat higher, but the pride of place was reserved for the other factors, in the first place — for the not explicitly formulated, and that is why constantly implicitly changed, aesthetic ones.

Prior to the formulation of the axiom of choice by G. Cantor and E. Zermelo, mathematicians did not realize that even after the explicit introduction of the totality of natural numbers together with the principle of mathematical induction, there would appear non-constructive theorems of existence, which would not provide the construction sought — even in principle. The axiom of choice is demonstratively ineffective. It states that, it is possible to construct a function, by choosing its elements from among each of the members of the family of non-empty sets, without saying anything about the method of carrying out this choice. The shock generated by the axiom of choice and by the paradoxes of the theory of sets — which appeared practically at the same time, forced the realization that a very large part of mathematics of the period ending in the 19th century was indeed non-constructive. The axiom of choice was magnificently inscribed upon the entirety of the hitherto formed paradigm of classical mathematics.

It should be mentioned here, that even in the 19th century attempts were made to construct some sections of mathematics upon a more constructive foundation — in particular by R. Grassmann [4] and E. Schröder [5] — but these attempts remained on the sidelines, away from the main road, and were forgotten.

Thus, the "crisis of the foundations of mathematics" sharply posed the question about the nature of mathematical constructions and about the interrelationship of mathematical objects and reality. And this gave rise to the necessity of a more exact and, in any case, a more explicit characterization of the class of constructive methods and, if possible, of eliminating the explicitly non-constructive ones. While examining the problem of constructivisability, it is possible to mark out three major trends: pseudo-classical, non-classical and significative.

The aim of the pseudo-classical trend is to single out the constructive sub-languages of the classical theories, wherein the classical logic and the customary concept of truth are left untouched. In modern mathematics this approach begins with A.Poincare — who tried to isolate those results of classical mathematics, which were obtained without the help of the axiom of choice, as he considered them to be more reliable and, with D. Hilbert — who had a more radical programme. For a detailed analysis of Hilbert's programme from a point of view that corresponds to the present state of the investigations in the foundations of mathematics, see : *Ershov. Yu. L.* and *Samokhvalov K.F.*, O novom podkhode k methodologii matematiki // Zakonmernosti razvitiya sovremennoi matematiki ("Nauka", M., 1987), pp. 85-106. Here we shall limit ourselves only to the remark that D. Hilbert unequivocally declared that the majority of mathematical statements do not have any real meaning and, that mathematics is required to give correct results only in respect of a set of comparatively simple real statements.

In the non-classical trend the concept of effective method is considered to be of paramount importance — mathematics is viewed as the science of effective (intellectual) constructions and logic adapts itself to the methods of such constructions, and gets so modified, as to wittingly guarantee the constructivisability of the constructions.

L.E.J. Brouwer was the first to point out that while aiming at attaining constructivisability one must not blindly follow that logic, which is tied to the tradition [6]. The roots of the non-constructive structures, are often not so much mathematical, as logical. For example, in any recursively axiomatizable non-contradictory classical theory containing arithmetic, it is possible — basing oneself upon a theorem of Gödel — to construct a statement of the form $\exists x \in N A(x)$, such that it is not possible to construct even one such number n, that $A(n)$ is provable, but, nevertheless, $\exists x \in N A(x)$ is provable. Indeed, for this it is enough to take the statement A — which is unsolvable in the given theory, and to construct the formula

$$\exists x ((x = 0 \,\&\, A) \vee (x = 1 \,\&\, \neg A)). \tag{1}$$

Brouwer showed, in particular that, the following two logical principles are most open to criticism from the point of view of constructivisability : the law of excluded middle $A \vee \neg A$ and the method of indirect proof $\neg\neg A \Rightarrow A$. Indeed in the constructive substantiation of the law of excluded middle it is demanded that a general method be constructed in respect of every problem, for establishing whether or not a given statement is true, and in the majority of cases such a method does not exist. Thus, the law of excluded middle may be called "the principle of omniscience", and it may be applied only in that situation, where both the language and the interpretation are deliberately so selected as to exclude the possibility of emergence of unsolvable problems.

Analogously, the principle $\neg\neg A \Rightarrow A$ signifies, that there exists a method for transition from the formulation of a solvable problem to its solution, i.e. the existence of the so-called "universal problem solver". This too narrows down the sphere of its constructive applicability.

Brouwer showed, that in principle it is possible to develop mathematics shunning the non-constructive principles, in particular — the principle of omniscience and that of the universal problem solver. The idea of logic as a calculus of problems, and that of the logical rules as transformers of problems into solutions, was made more exact by A.N. Kolmogorov [7]. Of late it has been realized that an enormous advantage of Kolmogorov's interpretation lies in the fact that it has not been specified through to the end, and, thus, it is rather an outline that can be modified. But to begin with this advantage was taken for a deficiency ; both S.C. Kleene and A.A. Markov derived the constructivist (intuitionist) logic from an exact definition of algorithm and of the concept of natural mumber. They showed that the principles against which Brouwer raised objections, are ineffective after a natural algorithm is given for singling out the constructive task from among arithmetical formulae, and an interpretation is provided for the transformations operative in this constructive task, as algorithms (partially — recursive functions). N.A. Shanin carried this construction through to its logical end [8] and, he provided an explicit algorithm for singling out the constructive task, having showed that after this substantiation of the construction carried out, one may go ahead with the methods of classical mathematics.

The third — significative — trend views mathematics as a science of formalisms and of the modes of their transformations. E. Schröder should be considered to be the spiritual father of this trend in many of its aspects, though he did not formulate an explicit "manifesto"of significative mathematics.

Now we shall dwell upon the paths of development, the mutual interactions and, the future perspectives of these three trends.

Gödel's incompleteness theorem turned out to be a turning point in the fate of the pseudo-classical trend. Naive hopes to the effect that it will be possible to get away without any serious reexamination of the paradigm of classical mathematics, having simply "sanctified" it with the Hilbertian incantations (and then one may happily forget about them) — were not vindicated. It became clear that the pseudo-classical trend too demands a serious reconstruction of the entire system of mathematical concepts, and that is why, from the point of view of psychological protection, the easiest thing to do was to simply interpret Gödel's [incompleteness] theorem as the collapse of Hilbert's programme as a whole — to be able to get along with what one was doing, this time, openly refusing to bother about the foundations. A quite frequent methodological error crops up in the interrelationships among the theoreticians and the "practitioners" : the "practitioners" are inclined to demand that the theoreticians should substantiate their [i.e., practitioners'] positions and activities, but only a "theoretician" on the verge of becoming a charlatan can provide such substantiation, in so far as in practice there is always a mix up of the rational, extremely exact activities, not only with the non-optimal, superfluous moves, but also with the plainly bad ones, dictated by tradition. That is why a true theory cannot substantiate practice, but must reconstruct it.

Nevertheless, namely, in this slack period, principled results were obtained by P.S. Novikov [9] : he established that from the classical proof of a formula of the form $\exists x A(x)$ in arithmetic — where $A(x)$ is algorithmically solvable — it is possible to obtain the construction of an n , such that $A(n)$.

During the last few years, the demands of theoretical programming, informatics and those of the so-called "artificial intelligence" have forced the logicians to return to the pseudo-classical trend. In particular, the development of the programming languages like PROLOG [10], has put forward the task of isolating those sub-systems of the classical logic, which retain constructivisability in some sense — as one of the most important tasks in this field. It appeared, in particular, that the system of Horn's formulae of the form

$$\forall x_1, \cdots, x_n (P_1 \& \cdots \& P_k \Rightarrow Q), \tag{2}$$

upon which PROLOG is based, possesses such property.

The classical formulae, which provide an opportunity to describe and elicit constructivist consturctions, have been more systematically described in the latest works of the Novosibirsk school : [11], [12]. Here we find the theoretical foundations for all possible systems of logical programming, based upon the classical logic. In yet another work [13] semantic conditions have been introduced upon the constructivisation of classical theories, without imposing any limit upon the class of formulae used. It appeared that for the fully constructive models (where any set defined by a formula of our label (signature) is recursively solvable), the classical logic is completely constructively interpretable.

The development of constructivist logic has forced us to ear-mark yet another component in the definition of a calculus, besides the axioms and the rules of deduction : it is the global structure of the inferences ; and this has opened up yet another opportunity for the development of the pseudo-classical trend. Even the intuitionist logic can be interpreted as classical logic with a limited global structure of natural deduction ; it has been established [14] that one can obtain the classical logic from intuitionist logic by adding to it one global, structural rule of inference : the rule of accepted unexpectedness.

But all these possibilities, opened up for the use of the pseudo-classical trend do not alter the basic conclusion of Brouwer : while using a logic one must carefully investigate the class of problems and the class of implied interpretations, otherwise reasoning and construction would inevitably drift apart. The ear-marked sub-classes of formulae, theories and inferences are based on such investigations in every case.

The pseudo-classical trend has unexpectedly turned up on the highway of development of classical mathematics itself. Though it has not been explicitly recognized , category theory may be that instrument, wherin again, as in the Hellenic mathematics, the constructive and the descriptive aspects have merged into one — where, proof guarantees construction. But this has now happened owing to a transition to a new level of abstraction, which has again permitted the banning of any explicit reference to the numbers. Further, the theory of categories leads to the necessity of investigating its own inner logic, which is intuitionst in the spaces (toposes) and coherent in the more general case [15]. Thus, here also, the pseudo-classical trend closes in on the non-classical trend.

Now, the non-classical trend has divided itself into two branches : intuitionism and constructivism.

In intuitionism we essentially base ourselves upon the incompleteness of our knowledge. Namely, we do not intend to provide any precise and final definition of the class of effective constructions[16]. What is more, in intuitionism we try to use this indeterminateness, this ignorance, as a positive factor. For example, from the substantiation of the principle of continuity in [16] :

$$\forall\alpha\,\exists n\,A(\alpha, n) \Rightarrow \forall\alpha\,\exists k\forall\beta\,(\forall l\,(l < k \Rightarrow \alpha(l) = \beta(l))\ \&\ A(\beta, n) \Rightarrow A(\alpha, n)) \qquad (3)$$

it is evident, that this principle signifies the absence of any knowledge of the global rules, which would indicate the behaviour of the Brouwerian "sequences of choice" or of the "sequences that have become free". What is more, later on a conception of "lawless sequences" has been worked out, wherein, in general, all accessible information constitues an initial block [17]. Thus, in intuitionism an attempt is made to demonstrate that the knowledge of ignorance happens to be the most valuable form of knowledge. Intuitionism is thus sharply at variance with the entire paradigm of classical mathematics.

Constructivism tries to unite constructivisability with maximum retention of the classical mathematical paradigm. To some extent constructivism is as Platonist, as the classical mathematics. The class of objects under consideration and the methods of their transformation (at this point we have an essential difference with classical mathematics — where one does not even think of the methods of transformation) are formulated precisely, basing the formulations upon an exact concept of algorithm. Knowledge is interpreted as a normal state, and ignorance — as an anomaly, which is inevitably present, but which must be overcome at all cost. Such an interpretation permitted A.A. Markov [18] to clearly ear-mark the system of initial abstractions, which are foundational to constructivist mathematics. This interpretation predetermined the journey of constructivist mathematics to a dead end in the narrow constructivism of N.A. Shanin [19], where an attempt has been made to totally ban ideal sentences from mathematics.

E. Bishop tried to occupy an intermediate position [20], when he tried to get away from an exact fixation of the class of effective methods, as well as from basing oneself upon ignorance. But when his conception was made more precise — see, in particular P. Martin-Leof's book [21] — it was found, that Bishop's conception lies completely within the frame-work of constuctivism. Martin-Leof was the first to make use of the circumstance — though it is true that he did not formulate it explicitly — that the giving of the consrtuctive objects and of an exact description of the methods of transformation of the objects, still does not fully determine the methods of transformation of methods, and that such constructive functionals of the higher type, metaalgorithms, can be varied completely— without touching the algorithms themselves.

During the last few years, the demands of application — in particular, of informatics, — has stimulated a Renaissance of Constructivism, even in our country; but this time it is a broad constructivism, which investigates the most diverse classes of methods and, correspondingly, the most divergent constructivist theories and even the constructivist logics. The very concept of constructivist logic is little by little tearing itself away from its unjustified ties with a single class of problems and effective methods and is becoming a relative concept, which may be varied, depending upon the descriptive language, in which transformable objects are described and problems are posed, and the class of programmes, or methods of transforming concrete and abstract objects are put forward. Viewed thus, intuitionist logic itself appears as the most classical of the constructivist logics — this logic is constuctivist in that situation, where we are faced with the task of pure functional programming [22]. In other words, a constructivist use of intuitionist logic is possible, when we are not very much constrained in terms of time and other resources, when our computations only add new data, new knowledge, over and above what we already have and, when it is possible to use the complex structures of data and the functionals of higher type.

The use of independent constructivist logics, and not of some fragments of classical logic, is expedient, when thereby the expressive power of language is sharply raised, and when the constructivist description is by far shorter and clearer than the corresponding classical one. For example, in [23] a case of pure implicative logic has been considered, wherein the formulae have been constructed with the help of a single logical connective — implication — from amongst the propositional letters, and a translation of the problem described into the traditional languages requires all the finite type of λ–terms, predicates upon them, and quantifiers.

Practically speaking, the third — significative — trend is also highly topical. Formal calculi and transformations of formalisms are widely investigated in mathematical linguistics and theoretical programming. But the methodological and metamathematical aspects of the significative point of view have not been sufficiently analysed . In this connecition see the works of P. Lorenzen [24] and S. Yu. Maslov : [25], [26].

Let us attempt a few conclusions. It is possible that the arrival of the pseudo-classical, "Hellenic" stage — wherein the constructive and the descriptive aspects merge into one — is indicative of the maturity and conceptual unity, of the conceptual completeness, of the system of mathematical concepts. Here, explicit mention of numbers are banned from the theories.

The desire to "carry the results right upto the numbers", to have explicit theories of computational methods and, generally, of the methods of practical constructions, leads to constructivism — in one form or the other. The different problem-oriented constructivist theories must give rise to an unified pseudo-classical theory, describing the effective structures of a given class.

The intuitionist theories are more abstract and they too can give rise to an entire family of more concrete constructivist theories; but they are operative in a different situation : when it is not expedient to assume the completeness of a system of concepts or, when the system of concepts is explicitily non-formalizable and when there is no point in striving at completeness.

Finally, it is necessary to conduct serious and deep-going investigations, to ascertain the emerging shape of significative mathematics.

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C O R R I G E N D A

ABBREVIATIONS : p. page: l. line, f-n. foot note, r. read, f. for.

p.1, l.12, r. *Marksizm*, f. *Markiszm*.

p.8, l.26, r. from x but , f. from x_1 but .

p.14, l. 28, r. Social, f. Soial.

p. 36, l. 19, r. $\frac{dy}{dx}$, f. $\frac{dy}{dy}$.

p.49, l. 21, r. $\frac{d^2u}{dx^2}$, f. $\frac{d^2y}{dx^2}$.

p.60,l.25,r. $[=f'(x)]$, f. $=f'(x)]$.

p. 63, l. 20, r. the, f. that.

p. 73, f-n., r. in, f. is.

p. 85, l. 23, r differential, f. differenctial.

p. 93, l. 11, r. *form*, f. *from*.

p. 94, l. 6, r. practice, f. parctice.

p. 94, l. 10, r. generally, f. genrally.

p. 94, l. 14, r. basis, f. bais.

p. 94, l. 15, r. binomial, f binomimal.

p. 94, l. 18, r. Newtonian, f. Newonian.

p. 98, l. 3, r. equivalent, f. equvalent.

p. 111, l. 5, r. Chios, f. chios.

p. 123, l. 33, r. manuscript, f. manuscipt.

p. 136, l. 21, r. $-\frac{1}{2\cdot 3}f'''(x)h^3$, f. $-\frac{1}{2\cdot 3}f''(x)h^3$.

p. 137, l.25, r. Marksizma, f. marxisma.

p. 177, l. 16, r. *from*, f. *form*.

p. 180, l. 6, r. that, f. t at.

p. 180, l. 29, r. $ma^{m-1}y$, f. $ma^m\ y$.

p.184, l.11, r.-67, f.-27.

p. 199, l. 8, r.

$$f(m)^m = \underbrace{f(m)\cdot f(m)\cdots f(m)}_{m\ times} = \underbrace{f(m+m+\cdots+m)}_{m\ times} = f(m^2),$$ f.

$$f(m)^m = \underbrace{f(m)\cdot f(m)\cdots f(m)}_{m\ times} = \underbrace{f(m+m+\cdots+m)}_{m\ times}$$

p. 202, l. 12, r. what has been, f. what been.

p. 227, l. 27, r. functions, f. function.

p. 237, l. 12, r. again pages, f. again a page.

p. 237, l. 28, r. 19-23, f. 19-22.

p. 275, l. 25, r. $(B)_{1)}$, $(B)_{2)}$, f. $(B)_{1)}$. $(B)_{2)}$.

p. 285, l. 19, r. broken form, so also with, f. generalised from, with.

p. 295, l. 21, r. Cx^2, f. cx^2 .

p. 308, l. 29, r. equalities, f. equlities.

p. 320, l. 32, r. investigating, f. investigations on.

p. 326, l. 19, r. translation, f. transtation.

p. 341 l. 7, r. owing to , f. for.

p. 344, l. 28, r. differentials, f. dif rentials.

p. 379, l. 1 (col. 2), r. ischislienie, f. ischeslienie.

p. 379, l. 4 (col. 2), r. Lausanne, f. Laussane.

p. 380, l. 22 (col. 2), r. estestvoznanie, f. estestvoznania.

p. 386, l. 21, r. graphic, f. graaphic.

p. 386, l. 22, r. introduced, f. ,intoroduced.

p. 391, l. 1, r. Engels, f. Engles.

p. 399, l. 18, r. it, f. them.

p. 400, l. 8, r. by, f. of.

p. 400, l. 28, r. (No., f. No.

p. 412, l. 13, r. *introduced*, f. *introdueed.*

p. 417, l. 11, r. $f''(x)\cdot[\Phi'(t)]^2\cdot\Delta t^2$, f. $f'(x)\cdot[\Phi'(t)]^2\cdot\Delta t^2$.

p. 425, l. 32, r. *Kisileva* , f. *Kiseleva.*

p. 425, l. 36, r. *Rybnikov*, f. *Rybikov*.

p. 427, l. 33, r. endlessly, f. endlessl.

p. 427, l. 34, r. dearest, f. dearst.

p. 438, l. 1, r. *Tattvachintamani,* f. *Tattva Chintamani.*

p. 446, l. 1, r. Engels, f. Engles.

p. 446, l. 10, r. Nauchnykh, f. Naychnykh.

p. 453, l. 41-42, r. 1928, books since 1948 and since..., f. 1948, and since... .

p. 460, l. 24, r. C. Thruesdell, f. K. Trusdell.

p. 466, l. 23-24, r. soon it was extended to the study of the so-called Arab Diophantus by R. Rashid (1974-) and, independently by J. Sesiano, and then, f. (the same words in different order).

p. 470, l. 21, r. happens, f. happen.

p. 478, l. 1, r. been, f. ben.

p. 496, para 3 should read as :

Abstract structures can be successfully used for constructing mathematical models; we may especially use those among them, which aim at revealing not only the numerical(metric) dependencies among magnitudes, but also the relations of a non-metric character.The study of such non-metric relations is of considerable significance for those sciences, where owing to the complexity of the object under investigation, and sometimes also owing to the unelaborated stage of a theory, it is impossible to present the results numerically.That is why, there one is often required to turn to the abstract structures of order. In their investigations about the different types of relations obtainable among individuals and groups in social collectives, psychologists and sociologists have begun to apply the oriented theory of graphs, which constitutes the simplest form of algebraic category.

p. 496, para 4, l. 2, r. this line of mathematisation of, f. th s line of mathematdsat on f.

p. 496, para 4, l.3, r. In fact, f. n fact.

p. 506, l. 7, r. Kisilev or, f. Kisvpfx. psr.

p. 506, l. 11, r. what are the, f. what re the.

p. 519, l. 32, r. $\{0, '\}$, f. $\{0, \}$.

p. 520, l. 5, r. above, f. abo e.

p. 523, l. 13-14, r. $0'' \cdots '$, f. $0'' \cdots '$.

p. 546, l. 8, r. $\ldots k\ \forall \beta (\ \forall\ l(l<k \ldots$, f. $\ldots k\ \forall \beta (V l(l<k \ldots$