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MATHEMATICS AND PHILOSOPHY

Panel discussion*

Dedicated to Professor Đuro KUREPA
on the Occasion of His 70th Birthday

Speech by Kajetan ŠEPER, Zagreb

Ladies and Gentlemen,

In the first place I wish to thank the Organizing Committee of the Symposium for having accepted our proposal to hold this panel discussion.

This discussion being dedicated to Professor Đuro Kurepa on the occasion of his 70th birthday, I am taking this opportunity to say a few words about Professor Kurepa. Please excuse me for the digressions I shall make.

When I was attending the high school at Osijek, somewhere in 1951 or 1952, I came accross Professor Kurepa's work "Teorija skupova", a first text-book on sets in our country. By that time I had read the well-known Moritz Cantor's "Vorlesungen über Geschichte der Mathematik", and a bit of philosophical logic and ordinary mathematics which I found in our libraries. No wonder that the sets were a refreshment for me. Even now I remember the footnote of the text on the null set and the all set. At that time the theory was attractive to me. However, I have never been fully satisfied with it: at the beginning I thought I did not understand what the theory was about, and later on I realized that I had to *accept* the theory in order to be able to *understand* what it was about.

As an undergraduate at the Department of Mathematics of the Faculty of Natural Sciences and Mathematics of the University of Zagreb, I met Professor Kurepa personally, in 1953 or 1954, studied with him and passed through a number of courses and seminars. Mathematical logic did not exist in Zagreb at all, neither did any foundational studies, with the exception of the traditional course in the foundations of geometry, but Professor Kurepa announced a list of various themes, among them the propositional calculus, the predicate calculus, axiomatics of real numbers, and the like. That was crucial for the whole further development of mathematical logic and foundations of mathematics in Zagreb, in Croatia, and perhaps in Yugoslavia, too.

* This panel discussion was organized by the Zagreb section of the Seminar for constructive mathematics and model theory Zagreb—Beograd (of the Mathematical Department of the Faculty of Natural Sciences and Mathematics, Zagreb, and the Mathematical Institute, Beograd).

Once I tried to sketch Prof. Kurepa's influence concerning mathematical logic in Zagreb. Regardless of the interinfluential laterals, I obtained a four-rank tree. I called it Kurepa "small tree". Of course, this tree should be enlarged by taking into the account his influence concerning other mathematical theories — set theory, topology etc., together with his influence in other or bigger regions — Belgrade, Yugoslavia etc.

It is not my intention to give here any account of Professor Kurepa's work, his activity, influence and importance — although I should perhaps apologize for that — but to say — and I feel obliged to do so — that Professor Kurepa has not been just a professional mathematician, a teacher and a pedagogue, but a real scientist and a philosopher, a humanist, and a human in the best sense of the word. He was the father, the originator and the pioneer of mathematical logic and foundational studies in Croatia, and of modern mathematical theories in Croatia and Yugoslavia. Generally speaking, he was catalizer, and initiator, a bringer and a transferer of knowledge.

As a student of his, and an admirer of his personality, with all of its virtues and individualities, qualities and peculiarities, temperament and character, I fullheartedly thank Professor Kurepa, in my own name and in the name of all of my colleagues, for everything he has done both as a scientist and as a man. Happy anniversary and many happy returns of the day!

CONSTRUCTIVE PROCESSES IN MATHEMATICS

Mathlmatical and Philosophical Aspect

SOME THESES CONCERNING THE DEVELOPMENT OF MATHEMATICS

Kajetan ŠEPER, Zagreb

1. Our **introductory general thesis** is that *constructive mathematics, in a broad sense, is a measure for determining the value of mathematics as a positive science in all epochs, and especially at present.* In our current opinion, the development of mathematics can be compared with a two-side balance: one side carries the practical, numerical, computer-computational, concrete, constructive mathematics, and the other — the theoretic, conceptual, abstract, non-constructive, platonistic mathematics. Although this balance has never been balanced, one yet clearly observes in each epoch an overloading of one of its sides. Its balancing by the new, the progressive and the necessary is the golden transition period; this period is the most valuable time interval in the historical development of mathematics both for its fruits and for its influence.

1. At the very beginning of civilization the scales did not actually exist. All mathematics was concrete, practical, inductive; in other words, if we use the comparison mentioned above, the constructive side of the balance overweighed.

2. It was the scientific and philosophical genius of the ancient Greeks that created the balance, i.e. the other side, the abstract, the theoretic, the deductive one.

3. This theoretic side already overweighed at the time of the ancient Greeks, and such a state was transmitted to and prevailed through the Middle Ages.

4. The European spirit, commercial and early-industrial, rebalanced the scales, and

5. raised the overloaded side by putting heavy weights onto the neglected side — the infinitesimal calculus has by no means been called a calculus at random, and mathematics and natural sciences became undiscernible.

6. The europeanized Greek genius again loaded the research with axioms and deductions, the actual infinity, and the absolute, and

7. created the Cantorian intellecto-universe. Thus the abstract theoretic side prevailed and its closed empire of ideas got its name: PLATONISM.

8. The force of history, however, is stronger than the ideas; science, and production, and society develop and so does the need for an equilibrium and also the requirement for a new open system, for a constructive universe, for CONSTRUCTIVISM.

9. Perspectives:

a) We conjecture "Periodicity". It should be mentioned that this conjecture concerns the immediate future; otherwise, we do not conjecture anything.

b_1) Goodman and Myhill conjecture "Compatibility and Interaction".

Cf. [1], p.83:

"One can distinguish two traditions in the study of the foundations of mathematics. The non-constructive tradition, represented today by set theory and category theory, . . . (and) the constructive tradition (which) is represented today by intuitionism and much of proof theory. These two tendencies in foundational studies are not incompatible. Rather, it is the interaction between them that is likely to lead to the most fruitful development of foundations as a whole. Current examples include the use of infinite proof-figures in proof theory and the use of elementary, rather than higher order, theories in studying categories. Our subject here is a recent development in constructivity which promises to open new avenues for such interaction."

Cf. [1], p. 94:

"Thus one may hope that the ultimate bastion of classical idealism, set theory, can be made to give way piecemeal to the insights which, in particular cases, it gives into the structure of its own objects."

b_2) Trostnikov conjectures "Quantitative Gnoseology".

Cf. [2], p. 252:

"Возможно, в будущем произойдет следующее: метаматематика вступит в более тесную, чем ныне, связ с определенними разделами материалистической философии и психологии и так образуется область, которую можно назвать "*количественная гносеология*", предметом которой будет проблема согласования различных "языков (каждый из которых опирается на свою специфическую структуру сознания), с помощю которых му конструируем, верифицируем и переконструируем объекты нашего" научного сознания, все полнее и глубже проникая в тайны материи."

2. From this observation it seems to us that *balancing is historically necessary in order for mathematics to be able to enter a new epoch*, and that *preponderance of one scale is a characteristic feature of each epoch*. Therefore it seems to us *that we*

are living in the transition period of balancing by means of historically heavier weights of the constructive, the numerical, the discrete ,the finite. This is our **first conclusive general thesis.**

3. *The process perceived clearly parallels the socio-economic systems in the evolution from the primitive society*, through slavery, feudalism, and early capitalism, *up to the contemporary systems* (highly developed capitalism and socialism). This correspodence suggests to us and substantiates our opinion that *Constructive Mathematics is a Socio-Economo-Political Problem, and not just a Philosophical One*, as it is widely accepted, spread and debated. This is our **second conclusive general thesis.**

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[Discussed by N.A. Shanin (Leningrad), S.R. Zervos (Athens), M. Krasner (Paris), Th. Stavropoulos (Athens), S. Panayiotis (Athens), J. Pelant (Prague), D. Rosenzweig (Zagreb).]

CONTRIBUTION TO THE DISCUSSION

M. KRASNER, Paris

KRASNER: Prof. Shanin said that the constructivism, in caracterizing certain mathematical objects by means of some information (which he compared to the macroscopical information of quantum mechanics), is considering only such objects and reasons only in passing from information to information. Even in supposing such "informational" point of view admitted, I don't believe that the information used by constructivists is the only possible and that the constructivistic way of using it is exhausting.

From another side, Prof. Shanin believes that constructive mathematical objects are more able to imitate (or "model") that of experimental sciences, that do that of classical mathematics and he considers this circumstance as a decisive advantage of the constructivistic point of view. If even it was so, I think that the mathematics, as any other adult science, has its own internal logic, and the existence and the interest if its objects are not determined by their ability of imitation of objects of other sciences or of material world. In particular, many highly interesting objects of algebra and of number theory have, until now, no relations with that of experimental or human sciences, even when they can be described constructivistically.

Let us remind the discussion between Borel, Hadamard and Lebesgue. It is clear that the constructivism is a development (and accomplishment) of Borel's ideas, and that usual naive and axiomatic set theory as basis of all classical mathematics derives from Hadamard's point of view (with some Hilbertian aftertaste). But, there exists a point of view inspired by Lebesgue's ideas, the "definitionism", where only the objects having a definition exist (clearly, the word "definition" has not so a narrow sense as for Lebesgue: in particular, there may exist definitionistic systems, where the definitions may not be finite)' The definitionism uses a wider information than constructivism, and in a wider way, although constructivistic objects are among the definitionistic ones, and the "relative'.

study of only constructivistic objects has (rather mathematical, than logical or philosophica) interest for a definitionist (and ,even, for a platonistic or axiomatic mathematician).

[In translating in Russian, I added: "Prof. Shanin gave the impression, by what he said, that every problem about constructivistic objects is soluble. That is certainly wrong."].

SHANIN: But, when we prove the existence of a solution of some problems, this proof gives, in the same time, a construction of some such solutions.

KRASNER: Yes, but there are constructivistically formulable problems, for which the constructivism, exactly as the ordinary mathematics, can give no answer, for example that of the validity of the Fermat's last theorem.

So, I recognize the interest of the constructivistic point of view, but I consider it as too narrow for me.

SHANIN: How too narrow? And all the hierarchy of the constructivistic types? For every part of Analysis a constructivistic analogue could be built.

KRASNER: For example, in constructivism do not exist the property of being an object or, also, properties opposite in absolute sense (I must say that they, also, don't really exist in the naive and in ZF-axiomatic set theory).

SHANIN: The arguments of Prof. Krasner about the autonomy of mathematics in respect to other sciences are a typical example of what happens when a constructivist and a classical mathematician meet ..... etc, ...

KRASNER: But I am not a classical mathematician from point of view of Foundations.

DIOPHANTINE EQUATIONS AND CONSISTENCY OF FORMAL THEORIES*

Mirko MIHALJINEC, Zagreb

For any recursively-enumerably axiomatizable first order formal theory, the set of Gödel numbers of its theorems is recursively enumerable. Of this kind are for instance the theory P (formalized Peano's arithmetics, see [1], pp.43, 300—301, it might be better to speak about Peano-arithmetics because the axiom of induction is expressed for formulas with one free variable in the language of the signature $\langle O, S, +, \cdot, < \rangle$), the theory S (formalized second order arithmetics, [1], pp.334—335), the theory ZFC (formalized set theory with the axiom of choice, [2], pp.507—508). If f is a recursive function which enumerates such a set of Gödel numbers, and if a is the Gödel number of a false formula (e.g. $0=s(0)$ in the language of P), then the consistency of the theory can be expressed in the following way: $\neg(\exists x) f(x)=a$. As the set of values of f (range, codomain of f) is recursively enumerable, according to the Matijasevič's theorem it is diophantine (see [4]), and there is a polynomial p (see [8]) in 14 variables with integral ccefficients such that consistency of the theory in question is equivalent to the formula: $\neg(\exists x_1)\ldots(\exists x_{13})\, p(a, x_1, \ldots, x_{13})=0$ (the coefficients of that polynomial can be effectively calculated as soon as the theory is specified, although it is practically impossible because of the size of the numbers involved). Even more, in order to check whether a formula of the language of such a theory is a theorem, one should calculate its Gödel number b and check if the equation $p(b, x_1, \ldots, x_{13})=0$ has a solution in nonnegative integers (although the corresponding algorithm, for instance for above mentioned theories, does not exist — that is connected with the

* Translated from the Serbo-Croatian by D. Rosenweig.

negative answer to the Hilbert's tenth problem). An important consequence is that provability of a statement can be reduced to solvability of a completely specified diophantine equation. From the viewpoint that the theory ZFC contains (almost) all of contemporary mathematics, it might be said that all mathematical problems can be reduced to solvability of corresponding diophantine equations. A word of caution is however necessary in this place, as such a view about ZFC certainly is exaggerated, because a formal system, however rich, cannot contain all of mathematics. Clear interpretability of a system is as important as its consistency.

If we compare the Gödel's second theorem about unprovability of consistency of a formal system P within the system itself ([1], pp.307—315) with the Gentzen's proof (which is finitary-constructive) of consistency of P ([1], pp.315—327), we can see that it has undoubtfully been proved in an arithmetically clear way that the equation $p(a, x_1, \ldots, x_{13})=0$ has no solution in nonnegative integers $x_1, \ldots, x_{13}$ and that this statement is not provable in the system P.

It is hence an enrichment of Peano-arithmetics and the theory of diophantine equations. Although consistency of P can be proved in the system S ([1], pp.338—339) that proof is (unlike the Getzen's one) not finitary-constructive, as such a proof for consistency of S is not known ([1], p.342) even after the results of Spector and Tait (see [7], p.7), and the possibility of such a proof is highly doubtful. This certainly holds for ZFC too, so we cannot be convinced about unsolvability of the diophantine equation derived from the statement "ZFC is consistent".

Solvability of diophantine equations has been object of research for a long time ([9], [10], [6], [3], pp.176—195, [14]). The methods of contemporary algebraic geometry and model theory do enrich our knowlegde about diophantine equations ([11], [12], [13], [15]). The question is, are the results so obtained provable as theorems in P, are there among them some theorems which are provable in a finitary-constructive way and which are not theorems of P? Is there a statement about unsolvability of some diophantine equation which is provable in a finitary-constructive way and which is not a theorem of S (may be even not of ZFC)?

"The study of diophantine equations, that is the solution of equations in integers, or, alternatively, in rationals, is as old as mathematics itself. It has exercised a fascination throughout the centuries and the number of isolated results is immense [as it is witnessed, for example, by Dickson's three tomes]. Some more-or-less general techniques and theories have been developed and there are some grandiose conjectures, but the body of knowledge is less systematic than that in more recently established branches of mathematics because here we are concerned with the most basic and intractable mathematical material: the rational integers." ([13], pp.193—194).

"I wish to note expressly that Theorem XI (and the corresponding results for M and A) do not contradict Hilbert's formalistic viewpoint. For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that c a n n o t be expressed in the formalism of P (or M or A)." (K. Gödel, [5], p.106.)

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[Discussed by A.N. Šanin (Leningrad), S.P. Zervos (Athens), Ž. Mijajlović (Belgrade).]

ON MARKOV'S PRINCIPLE*

N.A. SHANIN, Leningrad

Professor Shanin kindly conformed to the request of the organizer of the panel discussion to give a special lecture on Markov's principle, to speak especially in behalf of it, and to present the related point of view of those constructivists, primarily of Markov and of Shanin himself, who express their opinion about the *consistency with the idealizations and intuitive notions accepted in constructive mathematics* of that principle.

During the discussion we brought out our objections to the application and the plausibility of the principle in constructive mathematics.

At the end of the discussion we came to a *terminological* agreement only: according to the term 'constructive' in (the algorithmic foundations of) 'constructive mathematics' one has to distinguish *at least* two levels of abstraction and security. Markov's principle is concerned with the higher level i.e. with constructive mathematics in a *wide* (or *wider*) *sense*.

[Discussed by K. Šeper (Zagreb), D. Rosenzweig (Zagreb), M. Mihaljinec (Zagreb).]

* Summarized by K. Šeper

1. Contribution to the Discussion of Markov's Principle.

Kajetan ŠEPER, Zagreb

Constructive mathematics (CM) is the science of *constructive processes* (CP's) and *constructive objects* (CO's) — the results of such processes in case the processes terminate, and of our abilities of realizing these processes. More precisely, CP's are defined in terms of *algorithms* of various kind and CO's in terms of *words* in specific alphabets. The *abstraction of potential realizability* (APR), and the related idea of *potential infinity* based on it, is a characteristic feature of CM. Constructive mathematical logic is formed on the basis of CM, and depends upon CM; it models one's intuitive constructive thinking formally by means of syntactical and semantical systems. Our discussion is carried through on an intuitive ground, and is concerned with the phrase 'the process of applying an algorithm to an admissible input value *terminates*', or synonimously, 'the algorithmic process terminates in a *finite* number of steps'. Our initial attitude is that this phrase should be 'immediately clear' by our constructive point of view; in other words, that the phrase means that "we are able to indicate, *actually* or *potentially under* APR, the *number of steps* needed for terminating the application *of the process*, or equivalently, *one of its upper bounds*'. That number will be called here the *halting characteristic* of the process. During the discussion *Markov's principle* (MP) will be mentioned frequently.

We are discussing here the following problem: *Is the acceptance and use of MP in CM legitimate i.e. consistent with the idealizations and intuitive notions accepted in CM, and with APR, especially*?

We have objections to the acceptance and use of MP in CM. One applies it only when one does not have such a good insight in the algorithmic process under consideration that allows him to infer termination of the process, or, we hope seldom, if one does not care about it. In such a case, however, one is very often able to infer 'the impossibility of nontermination of the process' ('A') i.e. the impossibility of continuation of the development of the process after each step. Then, by use of MP, one is allowed to infer 'termination of the process' ('B*'), and, as a consequence, to treat the result of the process as being a CO. Of course, in order to find the result actually one is allowed to develop the process as long as he wants. Such a procedure is just suggested by the constructivists who accept MP and who believe that the process will finally stop. If one succeeds to compute the result, the application of MP becomes superfluous. Otherwise, generally one is in essentially the same position as if he did not have the information A — it does not indicate anything about termination, and it is left to one's decision of how long will he compute. So, we consider B* as not established by A, but rather as an *open* problem.

MARKOV himself, in his papers written before 1967, clarifies the principle by saying that he does not see any reason of *knowing* in advance exactly the halting characteristic of the process as being a necessary condition for asserting termination of the process. As a matter of fact, a number of great theorems in all branches of CM are obtained by using MP. In ŠANIN's well-known papers on constructive mathematical logic, MP is accepted and widely incorporated in the whole body of his semantical anlysis of the propositions of current CM i.e. CM+MP. (We wish to notice here that we got a feeling, after reading MARKOV's papers published after 1967, that even MARKOV would not treat the principle in such a generality any more.)

In our opinion, however, the acceptance of MP alters the intuitive notions of our constructive universe, and the entire motivation for CM, radically. The notion of 'finiteness' (*effective*, static, determined, bounded, actual or obtaining possibly under APR), which is essential and primary in our understanding of CP's, CO's and the idea of potential infinity, becomes altered into another weakened notion of '*floating* finiteness' (noneffective, dynamic, nondetermined, nonbounded, obtaining via potential infinity), or '*potential noninfinity*' i.e. *non-*' potential infinity'. So, the notions of 'termination' and 'CO' become altered, too; they become a kind of '*floating* termination' and '*floating* CO', respectively. The difference between the two notions of 'finiteness', or 'termination', or 'CO', the one being 'effective' and the other 'floating', can be characterized in the following way. The former is persistently *actual* or *potential under* APR, and so *based on* APR *directly*, and is defined by an *existential quantifier* (which in turn has to be interpreted by means of some contensive arguments), The latter, however, is unsteadily *procedure-like*, and *based on* the idea of *potential infinity*, and so *based on* APR, too, but *indirectly*, and is defined by a *negated universal quantifier*. In CM the primary notion is that of effective finiteness (effective termination, effective CO), directly established under APR, and that of potential infinity being secondary and defined by it. In CM+MP the primary notion is that of potential infinity, based on APR, and that of floating finiteness (floating termination, floating CO) being secondary and defined by it.

We do not say that MP is inconsistent with APR and the like. In any way, APR does not imply the idea of floating finiteness (floating termination, floating CO). We just say that this idea is based on the idea of potential infinity, and so on APR indirectly.

We do not say that MP is an *additional idealization* to APR and the like, either. (Cf. also ROSENZWEIG's discussion in this symposium.) Although we could say so, *if we have in mind our understanding of constructiveness* i.e. the essential and primary notions of finiteness, termination, CO etc., and, in addition, if we have in mind that, if we are working in CM+MP, we indeed *abstract from our actual knowing* of termination and argue as if such knowing is present, we yet avoid to say so. By saying that the acceptance of MP introduces an additional idealization into the body of CM, we could not abstain from saying that the acceptance *extends* the limited computational and combinatorial power of *homo sapiens from outside*, and, consequently, — we are firmly convinced — that it *extends* the class of constructively true propositions, too, and so, that it is *not consistent* with APR and the like, and that it *contradicts* to the essential and primary constructiveness in its whole, as well.

Exactly in the same sense as BROUWER abstracts from *laws* determining the components of sequences one after the other, and introduces in this way so-called 'choice sequences' (or synonimously, 'infinitely proceeding sequences'), so does MARKOV abstract from *halting characteristics* determining terminations and the corresponding results of algorithmic processes, and introduces in this way what we are calling here, 'floating termination' and 'floating CO'.

According to HEYTING ([1], p.71), "the only essential feature" of the components of a choice sequence "is that it does not matter by which means they are determined one after the other", and so, choice sequences "are not constructible objects in the strict sense".

How could the components of a sequence (termination of an algorithmic process and the corresponding result) be *determined*, if not by a law (halting characteristic)?

How could we *know* they are *determined*, if not by *knowing* a law (halting characteristic, respectively)?

We do say, however, that CM+MP, in relation to CM, deals with another weaker conceptual subject, and that it does not treat the fundamental constructive notions, such as finiteness, termination, CO etc., *adequately*. We consider CM+MP as the science of *floating CO's*. In such a theory CO's get mixed among all the weaker and weaker floating CO's. We do not feel any scientific, or philosophical, or practical reason to accept such a weak form of CO's, and in the same time not to accept for instance infinitely proceeding sequences or the like. We do not feel any need for a 'closure' of 'all' the — wider and wider classes of — *total* functions i.e. *total* algorithms, which in a definite sense MP implies. (We mean by that, that MP eliminates the known troubles with the existential quantifier in the definition of these functions i.e. the *circulus vitiosus* in the definition. See also *addenda* below.) We consider MP just as an *approximate guiding principle*, *heuristic* in nature, and its application as an *ephemeral quasi-constructive guiding argumentation*. Theoretically the subject is much more interesting, and the relation of CM to CM+MP should be examined formally in more detail. Nevertheless, we prefer any modeling of the notions of the wider and weaker theory CM+MP in the frame of the (narrower and stronger) theory CM i.e. in the frame of the strict CM. For instance, instead of having the notions of total functions, decidable set etc. in CM+MP, we prefer to manage them in CM by the notions of *weakly total* function, *weakly decidable* set etc., respectively. In fact, we feel and believe that *ultraconstructivistic tendency* in one or another form — we are considering complexity theory as one of the various aspects of the tendency — will play a role *sine qua non* in the development of mathematics in the future.

Addenda. Now, we wish to give here some quotations and comments.

After HEYTING's and PÉTER's clarification of the point (see the quotations given below), it is generally supposed that everybody working in this area is familiar with what the problem is about.

"There ought to be distinguished between

a) theories of the constructible;

b) constructive theories." ([1], p.69.)

"The notion of a constructible object must be a primitive notion in this sense that must be clear what it means that a given operation is the construction of a certain object. It has been explained by Miss Péter in her conference in this colloquium that any attempt to define the notion of a constructive theory leads to a vicious circle, because the definition always contains an existential quantifier, which in its turn must be interpreted constructively." ([1], p.70.)

"Als eine Zusammenfassung und Verallgemeinerung der durch diesen speziellen Rekursionsarten definierten Funktionen ist der HERBRAND-GÖDEL-KLEENEsche Begriff der allgemein-rekursiven Funktion entstanden [4]. Das ist ein sehr nützlicher Begriff, da er die einheitliche Behandlung sämtlicher speziellen rekursiven Funktionsarten ermöglicht; bisher ist aber keine allgemein-rekursive Funktion bekannt, die für irgendeine mathematische Unetrsuchung wichtig ist, und nicht unter eine der bekannten speziellen rekursiven Funktionsarten eingereiht werden könnte. Aber der Hauptziel bei der Einführung dieses Begriffes war eben die exakte Fassung des Konstruktivitätsbegriffes. Die sogenannte Churchsche Thesis identifiziert den Begriff der berechenbaren Funktion mit diesem Begriff. Hier möchte ich nicht darauf eingehen, worüber Kalmár sprechen wird, nämlich ob tatsächlich alle berechenbaren Funktionen allgemein-rekursiv sind; ich möchte gerade die entgegengesetzte Frage aufwerfen: können die allgemein-rekursiven Funktionen sämtlich mit Recht "effektiv-berechenbar", d.h. "konstruktiv" genannt werden?

Eine allgemein-rekursive Funktion wird durch ein Gleichungssystem angegeben, wobei vorausgesetzt wird, dass es zu jeder Stelle ein endliches Berechnungsverfahren gibt, welche aus Einsetzungen von Zahlen für Variablen und Ersetzungen von Gleichem durch Gleiches besteht, und den Wert der betrachteten Funktion an der angegebenen Stelle eindeutig liefert. Nun ist aber dieses "es gibt" etwas unsicheres, wie darauf schon der sprachliche Ausdruck hinweist, und zwar in den meisten Sprachen. "Es gibt" — wer denn? "Il y a" d.h. "er hat da" — wer und wo? "There is" d.h. "da ist" — wo denn? Kleene meint, wer das in dieser Allgemeinheit nicht annimmt, mag dieses "es gibt" konstruktiv auffassen. Das ist leicht zu sagen, gerade da bisher keine echt-allgemein-rekursive Funktion bekannt ist, und so kann man nicht wissen, was mit einer solchen Einschränkung verloren geht. So werden eigentlich zwei Begriffe der allgemein-rekursiven Funktion definiert: einer mit klassisch aufgefasstem, und einer mit intuitionistisch aufgefasstem "es gibt". Es wäre interessant durch ein Beispiel zu zeigen, inwiefern der letztere Begriff enger ist, nämlich durch eine

Funktion, welche klassisch allgemein-rekursiv ist und intuitionistisch nicht; das ist aber kaum zu hoffen, da in den bisherigen Betrachtungen noch überhaupt kein Beispiel für eine allgemein — und nicht speziell-rekursive Funktion vorgekommen ist. Nun, der klassische Begriff der allgemein--rekursiven Funktion ist nicht konstruktiv, und die intuitionistische (Definition) enthält ein Circulus vitiosus: hier soll das in der Definition aufretende "es gibt" konstruktiv sein — man wollte aber gerade mit dieser Definition der Allgemein-Rekursivität die Konstruktivität exakt definieren.

Derselbe Circulus vitiosus taucht überall auf, wie man ihn auch umgehen mag." ([2], pp.227 and 228.)

"Es hat den Anschein, dass sich der Konstruktivitätsbegriff überhaupt nicht zirkelfrei erfassen lässt." ([2], p.233.)

However, even after HEYTING's and PÉTER's papers, MENDELSON argues as if he did not know what is the subject about, and moreover, he gives a misleading statement of the subject of PÉTER's discussion. We quote here sec.2 of his paper entirely.

"2. According to the precise mathematical definition, a function $f(x_1, \ldots, x_n)$ is general recursive if there exists a system of equations **E** for computing f, i.e. for any $x_1, \ldots, x_n$, there exists a computation from **E** of the value of $f(x_1, \ldots, x_n)$ (Kleene [5]). Both occurrences of the exitential quantifier "there exists" are meant here in the non-constructive classical sense. To this, Péter ([2], p.229) makes the following objections: (i) The existential quantifier must be interpreted constructively; otherwise, the functions defined in this way cannot be considered constructive. (ii) If the existential quatifiers are meant in the construtive sense, and if the notion of "constructive" is defined in terms of general recursive functions, then this procedure contains a vicious circle.

Both objections seem to be without foundation. " (6. I am assuming that Péter intends "constructive" to have the same meaning as "effectively computable".) In the case of (i), the general recursive functions defined using the non-constructive existential quantifiers are certainly effectively computable in the sense in which this expression is used in Church's Thesis; no bound is set in advance on the number of steps required for computing the value of an effectively computable function, and it is not demanded that the computer know in advance how many steps will be needed. In addition, for a function to be computable by a system of equations it is not necessary that human beings ever know this fact, just as it is not necessary for human beings to prove a given function continuous in order that the function be continuous. Since objection (i) is thus seen to be unjustified, there is no need to assume, as is done in (ii), that the existential quatifiers are interpreted constructively. However, there is another error in (ii); "constructive" (or "effectively computable") is not *defined* in terms of general recursive functions. Church's Thesis is not a definition; rather it states that the class of general recursive functions has the same extension as the class of effectively computable functions; and the latter class has its own independent intuitive meaning. Thus, there is no vicious circle implicit in Church's Thesis". ([3], pp.202 and 203.)

MENDELSON's objections to PÉTER's objections to the definition of general (i.e. total) recursive functions are seen immediately to be without foundation and unjustified. His discussion is carried through in the non-constructive classical sense, in another universe, in a universe of speechifying, and so it has nothing to do with PÉTER's criticism. The discussion failed to hit the point. PÉTER *intends* "constructive" to have the same meaning as "effectively computable"; it *is* demanded that the computer *knows* in advance how many steps will be needed for computing the value of an effectively computable function; and, in addition, for a function to be computable by a system of equations it *is* necessary that human beings *know* this fact, just as it *is* necessary for human beings to *prove* a given function continuous in order that the function be continuous. Otherwise, *human beings* will try to solve all these *open* problems. PÉTER's initial question is:

Can all the general (i.e. total) recursive functions properly be called "effectively computable" i.e. "constructive", ([2], p.228.) Nowhere in PÉTER's paper one can find any mentioning that Church's Thesis is a definition, or that there is a vicious circle implicit in Church's Thesis, but that it seems that the notion of constructiveness (or finite-computability, or constructive theory, or effectively computable total function) cannot be made precise by a net mathematical definition that would be free of a vicious circle ([2], p.233). It seems as MENDELSON did re-discover in his paper that the class of effectively computable functions has its own independent intuitive meaning ([3], p.203).

KLEENE explains the situation very carefully and restrainedly. We give here a quotation of a passage from the fourth paragraph of footnote 171 in his text-book.

"We have been assuming without close examination the CONVERSE OF CHURCH'S THESIS: *If a function is Turing computable (or general recursive, or λ-definable), then it is intuitively computable (or effectively calculable).* In defending this implication to an intuitionist, or to any other kind of constructivist who considers an algorithm to exist only when it is proved by his standards that it always works, we only ask him to accept the following: if the hypothesis that a function is Turing computable holds by his standards, so does the conclusion. Put thus, it is hard to see how it can be questioned. Only if one allows a nonconstructive interpretation of the hypothesis, and yet insists on a constructive interpretation of the conclusion, is the converse of Church's thesis in doubt. "([6], p.241.)

Unfortunately, KLEENE does not discuss the meaning of 'it is proved by *a constructivist's standards* that an algorithm always works (i.e. terminates)'.

Undoubtly, ŠANIN's new 1973 paper is fully influenced by HEYTING's and PÉTER's papers, or at leasts by the facts they discuss. According to ŠANIN ([7], pp.217, 218, 222, and 223), let us consider some propositions with their clarifications, and some definitions.

Let A be any alphabet, $\mathbf{A}$ any algorithm over the alphabet A, and P any A-word (i.e. word in A).

(C_1) [(C_9), (C_{10})] The process of applying algorithm $\mathbf{A}$ to P terminates [is potentially infinite, is not potentially infinite].

(C_2) [(C^-_{11})] For any A-word X, the process of applying $\mathbf{A}$ to X terminates [is not potentially infinite].

(C^-_{12}) For any natural number n, $\neg\, \mathbf{W_A}(P, n)$.

Here $\mathbf{W_A}(X, n)$ stands for the condition "The process of applying algorithm $\mathbf{A}$ to word X terminates after not more than n steps". Obviously, this condition is testable by means of an algorithm applicable to (i.e. total with respect to) every word of the form X, n.

The notion of '*total algorithm with respect to words of a certain type*' is defined in this case by (C^*_2) ([7], p.218), an obvious generalization of (C_2). The sign — in (C^-_{11}) indicates an inessential for our discussion modification of (C_{11}). (C^-_{12}) slightly differs symbolically from (C_{12}).

(C_1) [(C_2)] is said to be *true* if it has a potentially realizable contensive demonstration.

An algorithm $\mathbf{A}$ over the alphabet A is said to be *total* (with respect to all A-words) if proposition (C_2) is true.

(C_9) is clarified (or 'deciphered') by (C^-_{12}).

(C_{10}) and (C^-_{11}) are correspondingly clarified.

We cannot imagine any such potentially realizable contensive demonstration of (C_1) which would not indicate the halting characteristic. On the other hand, if we accept MP, as a contensively conclusive argumentation, as ŠANIN does in the paper, then we do not see why *termination* i.e. (C_1) is not *clarified or defined by* (C_{10}) i.e. *by* $\neg \forall n \neg W_A(P, n)$, and *totalness* i.e. (C_2) *by* $(C^-{}_{11})$ i.e. *by* $\forall X \neg \forall n \neg W_A(X, n)$, where the prefixed quantifier — connective combinations are to be interpreted contensively as usual. By the acceptance of MP, a *pure* contensive demonstration of (C_1), i.e. such that does not make use of MP, get mixed among contensive demonstrations of (C_1) that make use of it.

According to PÉTER ([2], p.228), there are indeed two notions of general (i.e. total) recursive function that depends on the interpretation of 'there is' in the definition. The one is the *classical* notion and the other the *intuitionistical* (i.e. *constructive*) notion. However, the former is not a constructive notion, and the definition of the latter contains always a vicious circle. Nowadays 'there is' is interpreted contensively, or, in other words, is considered as a primitive notion, and is not defined by a net mathematical definition; hence, there is no vicious circle in the'definition' of the constructive notion of total recursive function. If 'there is' is interpreted in the sense of general applicability of MP, one more notion of total recursive function (call it *MP — constructive*, or *floating-constructive*) is introduced that has an intermediate status between those before mentioned.

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2. Contribution to the Discussion of Markov's Principle

Dean ROSENZWEIG, Zagreb

I see Markov's principle (MP) as a way around the difficulties arising in constructive interpretation of the quantifiers occurring in the definition of a total recursive function. A constructivist mathematician could live very well just with and open hierarchy of known total functions, e. g. of Péter-recursions. If one however insists on a closed, general definition, then such a directed application of *reductio ad absurdum* is the only known way to secure it. An attempt to interprete $\forall x \exists y\ T(a, x, y)$ just like any other sentence of the same form falls into an endless loop, while leaving such an interpretation to unspecified intutive arguments makes the demarcation between constructivism and intuitionism seem quite arbitrary and unmotivated.

So I understand MP as an additional idealization, *consistent with* but certainly *not derivede from* the abstraction of potential realizability and constructive interpretation of logical connectives and quantifiers.

Results proved by means of "constructive mathematics in the narrow sense", i.e. without MP, could de described as computations with an *a priori* upper bound on computational complexity. In such a case a programmer could say: "I could compute this if only I had computing apparatus of such and such speed, "where "such and such" means a previously known function. On the other hand, results proved in "extended constructive mathematics", i.e. by MP, represent computations with no *a priori* complexity bound. No real or imaginary programmer, however powerful a computer he had, could risk an uncontrolled run of such a program.

These arguments are of course highly theoretical, as in any presently conceivable situation only first three or four levels of the Grzegorczyk hierarchy are effectively computable (compulable in the sense of German *berechenbar*; more complex functions are effectively *rechenbar* but not *berechenbar* by humans in this time).

Nevertheless, such considerations guide me to distinguish between constructive mathematics without and with MP as different degrees of idealization, hence to try to eliminate MP where possible and to isolate results for which I don't know how to eliminate it.

K. Šeper
Fakultet strojarstva i brodogradnje
Salajeva 5
41000 Zagreb