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LANGUAGE, MIND AND MATHEMATICS

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LEIBNIZ'S MATHEMATICAL AND PHILOSOPHICAL APPROACHES TO ACTUAL INFINITY A CASE OF CULTURAL RESISTANCE

The notion of cultural resistance, as introduced by Raymond L. Wilder in his treatment of the history of mathematics, is a reliable guide in any pursuit of the history of ideas. A trouble which afflicts historians of ideas is that they find inconsistencies where a perfect logic is expected, to wit with great thinkers. However, after a while of reflexion, rather something contrary to that should be expected, if a cultural resistance is taken into account. Let us dwell a while on that phenomenon.

1. Creative thought and cultural resistance

1.1. Greatness of one's creative thought consists in surpassing the bounds of that cultural system in which one happened to be born and to live. But even the greatest mind is no supernatural being that would be able to easily overcome such confines. There must ever arise a tension between the existing paradigm and the drive of new original visions being characteristic of a genuine philosopher. This is a struggle which cannot end without victims, that is, uncertainties, changes of mind, even inconsistencies in the output of any original thinker.

Thus, what a philosopher's contemporaries firmly believe has to affect his mind, even most original and bold. Moreover, not only the beliefs which a philosopher encounters in the time he lives modify his original vision. There is even a more important factor, namely the invincible ignorance shared by him with his contemporaries. When seeing the views of our ancestors from the point of advanced knowledge of ours, we hardly can imagine how much different their way of thinking must have been. Let me mention two historical examples related to Leibniz's intellectual struggles: that of the theory of infinity and that of the idea of cosmic evolution.

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Since Aristotle, people distinguished between actual and potential infinity, and they asked, as Aquinas did, if there might exist infinite actually number of things. Aquina's answer in the negative is characteristic not only of the theological but also of the mathematical mode of thinking since the ancient Greeks up to the end of the 19th century.

Aquinas relies on that maxim of the Book of Wisdom, which impressed also Augustine and Leibniz, that God ordered all the things according to a number: *omnia in numero disposuisti*[1]. It seemed obvious for anybody since Greeks up to the appearing of modern set theory that the term "number" had to denote a *finite* number, so to speak, ex definitione. For, it was rightly believed that numbers are those objects on which operations of addition, multiplication, etc. should be defined. No such definitions were even in a remote field of vision, hence nobody could seriously think of infinite numbers. Only when precise definitions of operations in various domains of infinite numbers have been given in modern set theory, the term "infinite number" started to have a sense.

Thus, when the Holy Scripture declared that the world was created "in number", this must have meant for Aquinas and other heirs of the ancient thought that the world was not infinite. This was the picture of the universe with which Leibniz's vision of the infinite multitude of monads must have clashed. He proved not discouraged by this cultural resistance. However, on the other hand, he had no conceptual devices to incorporate his vision into a reasonable mathematical scheme; in such a sense he incurred losses because of the limitations of the cultural system in which he happened to live. This is why *Monadology*, the main work to develop his idea of the actually infinite universe, does not contain any reference to mathematical approaches to infinity.

1.2. Another conceptual abyss between Leibniz's time and that of ours may be hinted with the following Teilhard de Chardin's remark: *the greatest event in the evolution of human race is that it once learned about its evolution.* This gretest event was among those things in the earth and heaven which were not even dreamt by the philosophers in the 17th century.

I do not mean Darwin's idea of the evolution of plants and animals which was just a small step when compared with what Hubble's discovery and the

[1] See Summa Th., Pars Prima, q. 7, a. 4). The text in question mentions also weight and measure as the principles of ordering (*in pondere et mensura*). However, according to the typical biblic style these terms seem to be added just for emphasis, not as carrying a new content; hence "number" renders the concept in question.

general relativity theory disclosed to the people of the 20th century. The idea of the evolving universe was so puzzling, so unbelievable, that even Einstein rejected it in his first version of general relativity at the cost of distorting the theory. Only after Hubble's empirical discovery of the expansion of the universe, in 1923, Einstein returned to the non-distorted version, ashamed of his previous mistake.

While Einstein was not able to free himself from the century long habit of conceiving the universe as eternally stable, should we wonder that Leibniz did not manage it? Though his metaphysical vision included a presentiment of the eternally evolving universe as due to God's eternal activity, there was no remotest idea of that in the world picture of his time. Here we encounter another case of resistance; a fruitful idea did not bring fruits which it would offer in more favourable cultural circumstances.

Leibniz's vague intuition of the infinitely developing universe is implicit in his idea that the world is incessantly becoming — due to God's computing, and setting his thoughts in motion: *cum Deus calculat and cogitationes exercet, fit mundus.* Now, one faces the question, whether God may stop his computing and thinking. Provided the answer in the negative, and provided that God's intellectual activity makes the world ever better (and not ever worse), one has to endorse the idea of the universe ever getting better. Thus *the best of possible worlds*, as Leibniz used to call the existing one, in not the one in which we presently live but the one to evolve from that of ours in an infinitely remote future (this would make Voltaire's known satire rather pointless).

However, while so expressing his most intimate vision, Leibniz was not capable of working it out towards an idea of cosmic evolution. Among the reasons of that inability there was that he had no conceptual means to guess what kind of numbers might be involved in God's eternal computing.

2. Uneasiness about mathematical infinity

2.1. Georg Cantor used the phrase 'horror infiniti' coined on the pattern of 'horror vacui'. The latter was to be a property of Nature, while the former was to mean one's being afraid to face the abysmal infinity of infinite collections. The word 'fear' may be too dramatic to call Leibniz's attitude, but such words as 'disquiet' or 'uneasiness' truly render the state of mind both of him and his contemporaries.

The first well known sign of such a disquiet, extensively referred to by Leibniz in his *Accessio ad arithmeticam infinitorum* appears in Galileo's

Discorsi [...] (*Dialogues Concerning Two New Sciences*). The passage in *Discorsi* is worth quoting as a historical landmark in the human way to apprehending infinity. The text runs as follows (p. 32 in Engl. version).

> If I should ask how many squares there are one might reply truly that there are as many as the corresponding number of roots, since every square has its own root and every root its own square, while no square has more than one root and no root more than one square.
>
> But if I inquire how many roots there are, it cannot be denied that there are as many as there are numbers because every number is a root of some square. This being granted, we must say that there are as many squares as there are numbers because they are just as numerous as their roots, and all the numbers are roots.
>
> What then must we conclude under such circumstances? We can only infer that the totality of all numbers is infinite, and that the number of their roots is infinite; neither is the number of squares less than the totality of all numbers, nor the latter greater than the former; and finally *the attributes "equal", "greater", and "less", are not applicable to infinite but only to finite, quantities.* — [Italics W.M.]

The last sentence (italicized) makes evident the enormous distance between mathematical thinking in those times and in the period after the establishing of set theory. Until the power set axiom and the diagonal reasoning were introduced, nobody could reasonably speak of greater and smaller infinite totalities. Thus, in a sense, Galileo was right when he restricted applicability of these predicates to finite numbers; their meaning had not been defined for infinite numbers, hence their scope must have been restricted to the domains in which they originated, that is, the finite ones.

2.2. Leibniz's approach was more ambitious. He tried to handle the problem within a research programme concerning mathematical methods, and in connexion with some mathematical tasks which he was occupied with in 1672. It was the year which Leibniz spent in Paris waiting for an opportunity to carry out a diplomatic mission. The opportunity delayed (with no final success), hence he got a fair amount of time to engage himself into various research projects.

One of them started from a talk with Christian Huygens whom Leibniz regarded as his master in mathematics. This meeting is by Leibniz reported in an extensive letter to Gallois written by the end 1972, entitled *Accessio ad arithmeticam infinitorum.*

According to that report, Huygens suggested that Leibniz should try to solve a demanding (in that time) mathematical problem, namely to find the sum of a series of rational numbers. This series is listed as item [3] below. The remaining items exemplify akin results achieved by him after he generalized Huygen's problem. To wit, he defined a whole class of arithmetic series, the class being constructed in a systematic way to lead to the solution being looked for. It is that construction which Leibniz combined with the problem of infinity in arithmetic.

Leaving aside that method of construction (which would require a rather comprehensive exposition), let us just notice that in every next series the diferences between denominators of any adjacent terms become greater. Here are examples of the series (starting from [2], not from [1], for a reason to be seen later).

[2] $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\frac{1}{5}+\frac{1}{6}+\frac{1}{7}+$ *etc.* $=\frac{1}{0}$

[3] $\frac{1}{1}+\frac{1}{3}+\frac{1}{6}+\frac{1}{10}+\frac{1}{15}+\frac{1}{21}+\frac{1}{28}+$ *etc.* $=\frac{2}{1}$

[4] $\frac{1}{1}+\frac{1}{4}+\frac{1}{10}+\frac{1}{20}+\frac{1}{35}+\frac{1}{56}+\frac{1}{84}+$ *etc.* $=\frac{3}{2}$

Note that in [2] the difference equals one in any case; in [3] it equals two between the first two terms, three between the 2nd and the 3rd term, and so on. In [4] such differences are still greater than in the preceding series. Leibniz lists, moreover, series [5], [6], [7] as examples, each obeying the same law of increasing (with each next series) the differences between adjacent denominators.

At the same time, in the fractions being the sums of series, numerators and denominators increase in such a way the they form the sequence (from [2] to [7], respectively):

$$\frac{1}{0},\ \frac{2}{1},\ \frac{3}{2},\ \frac{4}{3},\ \frac{5}{4},\ \frac{6}{5},\ etc.$$

Now we come to the point in which Leibniz's argument concerning infinity can be traced. We should complete the above list of series with the lacking item [1] in which denominator differences would be lesser than 1 (as occurring in [2]). This should equal zero, while the sum should equal the fraction $\frac{0}{0}$ (for it should have the numerator less than that in [2]). Thus we obtain the following series.

[1] $\frac{1}{1}+\frac{1}{1}+\frac{1}{1}+$ *etc.* $=\frac{0}{0}=0$

Leibniz's comment concerning this series requires a bit of discussion since its meaning does not seem clear to a modern reader. The comment runs as follows (p. 15 in the edition mentioned in References).

> [...] audacter concludo numerum istum infinitum sive numerum maximum seu omnium unitatum possibilium summam, quam et infinitissimum appellare possis, sive numerum omnium numerorum esse 0 seu nihil. Et demonstratio nova vel ex eo suppetit, quod numerus maximus est summa omnium unitatum sive numerus omnium numerorum. At summa numerorum necessario major est numero numerorum (ut 1+2+3 etc. majus quam 1+1+1 etc.). Ergo numerus maximus non est numerus maximus seu numerus maximus est 0, *etsi non ideo infinitas partes continuo aut infinitam magnitudinem tempori ac spatio protinus negem.* [Italics – W.M.]

The passage italicized by myself is worth utmost attention as it displays that uneasiness which I hint at in the title of this section. The scientists and rationalist philosophers in the 17th century claimed that the new science must be entirely mathematical if it is to succeed in explaining the world. Leibniz belonged to most ardent followers of that programme. However, he must have admitted that mathematics does not fully reflect the structure of reality. While time and space possess infinite magnitudes, these magnitudes, unfortunately, cannot be rendered with mathematical concepts.

Before trying to find out a possible source of that failure of Leibnizian mathematics, some comments are in order to interpret the previous part of the quotation. Leibniz makes use there of the equality listed above as [1], where the left side represents an infinite magnitude, as the ones are being added and added without stopping, while the right side amounts to zero (that it is obtained with dividing zero by zero may be here disregarded as a minor point). Thus Leibniz feels entitled to emphatically conclude that *the infinite number of all numbers amounts to zero, or nothing.*

It does not seem clear how to understand the identifying of zero with nothing. As for the series [1], '0' denotes a mathematical entity in it, but no mathematical entity deserves to be called nothing. Moreover, in the next argument ('demonstratio nova') being like a reductio ad absurdum, Leibniz seems to blame the concept of an infinite number as lacking consistency. For, he argues, such a number (as the sum of all terms in series [1]) would be both the greatest one, as being infinite, and not the greatest one, since the series of all natural numbers (as 1+2+3, etc.) would be greater yet. According to Leibniz, the self-contradictory phrase 'the greatest number is not the greatest one' denotes zero, and here again he identifies zero with nothing; however, zero is an object undoubtedly free from being self-contradictory while nothing is defined by an inconsistent expression, indeed.

2.3. Why did Leibniz prefer to deprive his philosophy of mathematical support than to admit infinite numbers? An explanation can be found in the same *Accessio*, where he sketches a project for what would be nowadays called 'foundations of knowledge'. This project included derivation of Euclidean axioms from mere definitions of the terms involved. This seemed to him the safest way of ensuring the truth of the first premisses of mathematics. When announcing the subject of *Accessio* in its introductory passage, he puts on the same footing arguments against infinity and arguments for the possibility of proving mathematical axioms. This declaration, serving both as a title and an abstract, runs as follows.

> Accessio ad arithmeticam infinitorum, ubi et ostenditur numerum maximum seu numerum infinitum omnium numerorum impossibilem esse sive nullum; item ea, quae pro axiomatis habentur, demonstrabilia esse evincitur exemplis.

Among those most venerable axioms whose proofs, as Leibniz believed, were supplied by him on the basis of definitions alone, there was that a whole is greater than any part of it:

Omne totum est majus sua parte.

At the last pages of *Accessio* Leibniz offers examples of such proofs, including a demonstration of the above principle (the course of reasoning is not relevant to the present subject). Leibniz was so earnestly engaged in that methodological project that he most appreciated what he regarded as its results. This should explain why he was so sensitive to anything what seemed to endanger the whole-part principle; and the idea of a set of numbers whose part equals the whole appeared to him destructive.

There is a moral to be drawn from this story, which may be instructive for students of the history of ideas. Let me express this lesson in the familiar metaphor of hardaware and software. Imagine, you have to choose between (1) a computer which due to the hardware has enormous computational power (as consisting in speed, memory size, etc.), but no software is supplied with it, and (2) a computer with less giant hardware parameters but richly endowed with useful software.

Now compare (A) a genius of old times, enjoing a wonderfull brain but devoid of knowledge and skills which came later in the historical development, and (B) a less gifted brain but equipped with advanced knowledge and sophisticated problem-solving methods. Obviously, A is the counterpart of 1, while B is the countepart of 2 in the hardware-software parable.

The moral is as follows. When admiring the intellectual power of people like Leibniz, we should be prepared to take into account their limitations of knowledge, methods, and conceptual equippment ('software') which are overcome with later achievements, those inherited by our generation. It is historian's task to sharply analyze our ancestors' failures. Thus he wins a starting point to trace the progress owed to next generations.

3. Is it possible for a modern mind to understand *Monadology*?

3.1. One's understanding of other one's view involves either agreeing or disegreeing with it, or else refraining from both with being aware of why one refrains. Does it often happen that this criterion is met by Leibniz scholars with respect to strange ideas of *Monadology*?

There are at least two prerequisities for Leibniz scholars to realize these ideas. (1) A scholar is bound either to recognize an infinite set of monads, at least as being possible in one of scenarios admissible for nowadays science, or to state that there is no chance of such a scientific exemplification. (2) He should decide whether he admits the view of the universe as having infinitely many levels of complexity. The dealing with these questions should be aided by an awareness which infinity is at stake: that of denumerable sets (aleph zero) alone, that of continuum, or else a higher one. The innocent ignorance of our ancestors who did not distinguish among infinities is no longer available to a modern researcher; he may refuse, like Leibniz, to connect metaphysics with set-theoretical notions but, unlike Leibniz, he would be obliged to account for the disregarding of set theory.

In what follows, I shall attempt at a rational reconstruction of Monadology in terms of modern science, treating that procedure as a means to understand Leibniz's thought; when suggesting one from among many possible intepretations, one approaches to understanding. One should notice that such reconstruction may involve counterfactual assumptions — in order to free oneself from accidental historical facts.

3.2. Let me start from assuming that Leibniz's rejection of infinity in mathematics was just a historical accident. Had he been born, say, in the 20th century, he would have willingly agreed that there are as many even numbers as all natural numbers, and so on. For, owing to the achievements of set theory, he would have accepted the distinction of two kinds of the whole-part relation, one valid for finite collections, the other for infinite collections. Certainly he would have enjoyed Cantor's diagonal argument

to the effect that the set of natural numbers, being a part of the set of reals, has to be smaller than the latter (because of the lack of one-to-one corespondence), in accordance with his favourite principle[2].

When making use of such counterfactual considerations, I claim thereby that Leibniz's rejection of mathematical infinity is logically independent from the rest of his philosophical thinking (depending solely from too narrow interpretation of the whole-part axiom in that time mathematics). Should we reject that rejection, the rest of the intellectual edifice would remain intact. If someone affirms the opposite, it is up to him to demonstrate a logical nexus between the denial of infinite numbers and philosophical principles.

The historical, and not logical, dependence of Leibniz views on infinity, connected with the cultural circumstances of his time, has been noticed by Hans Poser who reports on most influential rationalist thinkers as well as most renowned mathematicians of that time, all of them denying reasonabless of the idea of infinity, and then concludes: "Dies ist die Situation die Leibniz vorfindet". Had he found a different situation, his views on mathematical infinity would have been likely to be different, without any significant change in his philosophical vision.

It should be distinguished between, so to speak, downword and upword understanding of older ideas. The former relies on knowing those historical antecendents which account for the content and the appearance of the idea or theory in question. The latter consists in an attempt to render this idea in modern conceptual framework, to make it reasonable within this framework; this does not mean its acceptance, rather a mere possibility of acceptance if certain conditions prove satisfied. Such interpretational hypothesis in a way resembles an empirical hypothesis in science; even if not accepted in the moment, it may be seriously considered owing to its well-defined content.

3.3. Let me start from a conjecture to interpret the notion of monad. It seems that neither elementary particles of physics nor human minds can pretend to be monads. Though in the moment no commonly acceptable candidate is in view, a situation is better for Monadology now than it was in the framework of classical physics. For in various ways physics becomes to be permeated with the concept of information.

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[2] There is an inspiring Friedman's essay on analogies between Monadology and set theory. He notices Leibniz's refusal to acknowledge actual infinity in mathematics, but a historical explanation of the divergence in question is not intended in his essay. The problem appears more sharply when one takes into account Leibniz's view on mathematics as the most powerful device for philosophy. Then the question arises why did he give up applying this tool to the foundations of his own philosophical system.

The uncertainty principle somehow connects physical processes with activities of the human mind. This was the first departure from the paradigm in which mind (as well as information) and matter were absolutely separated. It is commonly known how far we are from a satisfying interpretation of the uncertainty principle. Hence no reliable path leading from such ideas to monadology can be imagined in the present state of science. Nevertheless, a rift in the old picture has been made, and clima for connecting matter with information gets more favourable. This is why there could appear a popular book on quanta entitled *The Ghost in the Atom: a discussion of the mysteries of quantum physics.* It includes interviews with most prominent representatives of eight, competing with each other, interpretations of quantum physics.

Among these interpretations there is one, developed by David Bohm, having been initiated by Louis de Broglie, which should suit Leibniz in a particular way. Contrary to the mainstream interpretation by Bohr and Heisenberg, which suggests an influence of the mind on physical phenomena, Bohm's view is free from such subjectivist approach. Instead, the famous uncertainty is being explained as resulting from researcher's lack of knowledge as to a deeper, more complex, level of phenomena; because of this emphasis on the objective reality, Bohm's interpretation is called *ontological.*

The whole point of ontological interpretation is to claim that there may be an *infinity of levels* in nature. Ever new kinds of entities and processes may appear at a deeper level. Bohm (1957; 133) characterizes the qualitative infinity of nature in the following way[3].

> A systematic and consistent analysis of what we can actually conclude from experimental and observational data leads us to the notion that nature may have in it an infinity of different kinds of things.

Popper (1977; 33) when approvingly discusses Bohm's ideas, summarizes them as follows in the context of complexity of particles deemed earlier as elementary (here the Leibnizian anti-atomism would be triumphant).

> More recently, the subatomic particles have in their turn been diagnosed as complex structures; and David Bohm (1957) has discussed the possibility that there may be an infinity of such hierarchic layers.

Leibniz in the frequently mentioned passage 64 of *Monadology* speaks of living bodies as being structures *in the least of their parts ad infinitum.*

[3] The quotations below from Bohm's texts are given after Pylkkänen 1992.

Here Bohm and Popper seem to be more monadologist than Leibniz himself as the latter restricted the infinite structural complexity to organic bodies alone. However, would he have sticked to this restricted view, if he had possessed our modern knowledge of matter? Then he should have known that parts of organic bodies may be isolated and exist outside a living body. Should they lose their structure then? If not, Leibniz's notion of infinite structural complexity of living matter, down to ever deeper layers, should be extended to any matter at all. As if continuing this line of thought, Bohm (1990; 283f) claims the following.

> In some sense a rudimentary mind-like quality is present even at the level of particle physics, and as we go to subtler levels, this mind-like quality becomes stronger and more developed. Each kind and level of mind may have a relative autonomy and stability. One may then describe the essential mode of relationship of all these as *participation.* [...] Through enfoldment, each relatively autonomous kind and level of mind to one degree or another partakes of the whole. Through this it partakes of all the others in its "gathering" of information. And through the activity of this information, it similarly takes part in the whole and in every part.

This seems to be akin to Leibniz's idea which in passage 63 is expressed as follows. *Every monad is in its way a mirror of the universe, and since the universe is regulated in a perfect order, there must also be an order in that which represents, that is to say in the perceptions of the soul.* Bohm's notion of gathering information is worth comparing with that of perception, while participation seems to be like Leibnizian mirroring.

These and other analogies do not mean that Bohm's theory is something like Monadology resuscited. There are differences to be discussed, for instance, the notion of substance (which is very rigid in Leibniz while in Bohm is more relative), and the claim of determinism (which with Bohm is combined with a kind of indeterminism). However, what I intend is not to vindicate Monadology within the frame of modern science but just to hint at the possibility of reasonably discussing it in modern terms.

3.4. There may be still another modern approach to Monadology. I mention it very briefly here as the subject requires more size and a separate discussion.

Ever more popular with phycists and information scientist becomes the idea that the universe is like a giant computer. On the other hand, within a view due to Richard Feynmann, even single elementary particles may be viewed as computers. Between these two extremes there are in Nature innumerable systems — included in the universe and including particles — which also may act like computing devices.

If we try to imagine monads on the pattern of computing automata, belonging to Nature, hence *automata naturalia* (as called in *Monadology*, 64), then we obtain a picture not very far from that drawn by Leibniz. If, moreover, we emphasize the role of software, treating hardware as a secondary element which may be produced if a suitable software (to control production) is available, then the analogy with monads gets even closer.

Let us go further. *Essentiae rerum sunt sicut numeri* — says Leibniz in his juvenile dissertation *De principio individui*, and develops this thought throughout later writings. At the same time, each Turing machine, hence each computer, can be defined with a single number, owing to the ingeniuos coding procedure invented by Turing. Analogically, monads might be represented by single numbers. While computers, ex definitione, are coded with computable numbers, there is no obstacle to believe that what Leibniz called divine or natural machines might be coded with non-computable numbers (an idea close to Penrose's contention), and so known to God alone.

After thus arriving at God's calculating powers, we reach a fitting finale to sum up the argument with the saying: *cum Deus calculat, fit mundus*. This seems to disclose the essence of Leibniz's thought. Therefore, contrary to all the arguments he had against infinity of numbers, and in spite of the cultural resistance, there was a moment when he could not help expressing a faith in the infinity of numbers. *Neque enim negari potest, omnium numerorum possibilium naturas revera dari, saltem in divina mente, adeoque numerum multitudinem esse infinitam.* (Letter to Des Bosses, 11 March, 1706.) Let me say it once more, in English. "One cannot deny that the natures of all possible numbers do exist, at least in the mind of God, and this is why the set of numbers is infinite."

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STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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LOGICAL CONSEQUENCE AND THE LIMITS OF FIRST-ORDER LOGIC

1. Introduction

Logic as a formal mathematical theory is interesting in itself. It poses problems, like any other body of knowledge. Some of them, especially questions concerning axiomatization, consistency, completeness (in various senses of the term), decidability, etc. are more specific with respect to logic (and mathematics to some extent) than in the context of other theoretical systems. These questions are in principle independent of any application of logic to other branches of science. If logic is conceived in this manner, we speak about *logica docens*. On the other hand, logicians always claim that the main task of logic consists in governing intellectual activities. Thus, *logica utens* (logic in use) is deduction, indispensable device of mind, particularly in various reasonings. Leaving aside traditional prescriptions, usually considered as stemming from logic and helping us in processes of defining or classifying, the main aim of logical theory is to codify the rules of deductive proofs. These rules should be stated formally and effectively. In particular, checking whether a proof is correct or not should be subjected to mechanical or algorithmic procedures. However, the rules of deductive proofs must guarantee that they lead from true premises to true conclusions, that is, block deductive derivations of falsehoods from truth. We have here an important difference between deduction and induction. The label "correct deduction" is in fact pleonastic, unless it points out that a given deduction was more complex than the proof required, for instance, that is employs unnecessary premises or proceeds indirectly instead of directly. Yet too complicated deduction is still deduction, if any. Induction may be correct, despite starting with true premises and resulting with false conclusions, if its rules are preserved (of course, I am conscious that speaking about rules of induction is a delicate matter, but we think, for instance, that inductive

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reasoning is correct if it was performed as carefully as possible). Incorrect induction is still induction, although incorrect deduction is not a deduction.

According to the contemporary view about logic, the codification of the rules of proof is a matter of syntax, although semantics investigates what it means that logical rules do not pass from true premises to false conclusions. On the other hand, we expect that syntax and semantics interplay in such a way that syntactically stated principles of inference have their semantic counterparts. To be more explicit, provability as a syntactic phenomenon displays the semantic fact that logical consequence is an operation that preserves truth. The question whether it is so or not, is known as the completeness problem (I will com back to this issue in section 5 below). Roughly speaking, a logic **LOG** is (semantically) complete if and only if the sets of its truths (logically valid sentences, tautologies) coincide with the set of its theorems (provable sentences). More strictly, the completeness problem consists in the question whether all tautologies are provable, and the problem whether provable sentences are tautological constitutes the soundness (adequacy) question. The latter is much easier to be solved, because it is sufficient to check whether axioms are tautologies, but the rules lead from tautologies to tautologies. The real completeness problem is a much more difficult topic. It is clear when we consider another formulation of the completeness theorem (equivalent with the formerly given), namely that every consistent set of sentences has a model. Since constructing models of theories is not a straightforward matter, it shows how complex the completeness problem can be.

It is quite understandable that logicians expected decidability of the methods of deductive proofs. At the first sight, it is even strange that things could be different. The concept of proof as formalized by our familiar logic, is recursive. Since any string of formulas can be mechanically checked whether it is a proof or not, it could well motivate the claim that the property of provability is decidable. Great philosophers or mathematicians, like Lebniz or Hibert were convinced about decidability of provability. This expectation was showed (Church, Turing) to be a dream. Thus even a semantically complete logic does not need to be decidable: although we can know that every tautology is provable, it does not imply that we have a mechanical procedure which tells us whether an arbitrary formula is provable or not. Things look differently in particular systems. Propositional calculus is semantically complete and decidable, but first-order predicate logic is semantically complete but undecidable. Going further, arithmetic is semantically incomplete (unless we employ a primitive trick consisting in taking all arithmetical truths as axioms) and undecidable.

Discovering theorems displaying various properties and limitations of formal systems became a great achievement of logicians of 20th century. So-called limitative theorems (Gödel – incompleteness of arithmetic, unprovability of consistency; Tarski – undefinability of truth; Church – undecidability of first-order quantification theory, undecidability of arithmatic; Skolem – the existence of non-standard models of first-order theories) threw a new light at formal systems and their properties. On the other hand, the crystal vision of logic as an instrumentarium of deduction, formerly propagated by people, like Frege, Russell or Hilbert, became obsolete. The question "What is logic?" appears much more complicated than the pioneers of logic were inclined to think. The problem is what we should preserve as essential properties of logic. Completeness?, decidability?, or something else, perhaps a considerable expressibility. It is an interesting philosophical question. I will try to exhibit some of its aspects by focusing on first-order logic and its properties as related to concepts of provability and logical consequence. Of course, it requires a comparison of first-order logic (**FOL**) with higher-order systems. One guiding idea directs my considerations. Although we are free to a great extent in adopting this or convention governing the use of the word "logic", it basically has nothing to do with properties of formal systems. They are complete or incomplete, decidable or undecidable, have a great expressive power or not, etc. entirely independently of our terminological decisions and inclinations concerning the usage of words. It should be remembered in any discussion about the nature of logic.

2. Some remarks about the main foundational projects of 20th century

The standard account of the history of mathematical logic is like that. Leibniz was a great forerunner of it, but his ideas were not understood and probably they could not be properly appreciated at the time. Then, Boole came and began the algebraic tradition in logic which was continued by people, like Peirce and Schröder. The genius of Frege changed everything. Firstly, Frege established the proper succession of logical systems, which starts with propositional calculus and goes to quantification theory. Secondly, he also projected and elaborated logicism in details, a project of *logica magna* (one of Leibniz's dreams), which could cover the whole of mathematics. Unfortunately, Frege's system was damaged by discovery of the Russell antinomy. Russell tried to save logicism by the theory of types.

However, this construction was not satisfactory even for Russell himself, who found some of its elements, like the axiom of reducibility, dubious. On the other hand, the theory of types disappointed other people not because of its details, but quite principally. Other foundational projects arose, namely Brouwer's intuitionism and Hilbert's formalism. All foundational projects had advantages and disadvantages. Logicism was either incomplete or based on artificial assumptions (the mentioned axiom of reducibility). Intuitionism cut classical mathematics too much. Formalism was promising but Gödel's and Church's results devastated it considerably.

I do not want to suggest that these facts did not happen. Also I do not underestimate works of Gödel, Tarski or Church, which brought the real revolution and done in the frameworks of *Principia Mathematica*. Yet the history of mathematical logic and the foundations of mathematics the above outlined scenario suggests, does not exhibit the whole truth. First of all, it overestimates the relevance of antinomies. What matters here is not only that some antinomies (the paradox of Burali-Forti) were earlier discovered than Russell's, but that the problem of the set of all set that are not elements of themselves was known to Zermelo and probably Hilbert, and did not alarm them. Zermelo's way out consisted in an axiomatization of set theory, which precluded dangerous sets. However, one can argue that, due to the common practice of axiomatizing of mathematical theories at the break of 19^{th} and 20^{th} century, set theory would be captured by an axiomatic system, even if no antinomies were discovered. Hilbert's case was even more explicit. His demand that consistency of mathematics should be effectively proved was explicitly articulated before Russell announced his famous paradox. The same concerns Hilbert's slogan that in mathematics there is no room for *non ignorabimus*. Finally, Brouwer's protest against epistemology of classical mathematics is also conceivable independently of antinomies. Of course, since antinomies appeared, all three great foundational projects (logicism, intuitionism an formalism) had to propose devices to avoid them, but, with exception of logicism, it was their secondary task.

Also, I think, that the consequences of Gödel's incompleteness theorems did not concern formalism only. Look at the definition of logic in Frege and Russell. Roughly speaking, it says that theorems of logic, including mathematics as reducible to logic, are provable by purely logical methods plus definitions in terms of a few very primitive concepts (in particular, the membership relation). Since the logicist identifies logical truths and logical theorems, the definition of logic says that all logical truths are provable by purely logical means. However, due to the first incompleteness theorem, it

is impossible, because we have true sentences, but unprovable by logic (see Woleński 1995, 1995a).

The matter of intuitionism is perhaps less evident, but consider the following reasoning. The intuitionist demands that all mathematical theorems must be proved by constructive methods. Whatever it means, constructive methods have to avoid the principle of excluded middle. The second incompleteness theorem says that consistency of arithmetic is not provable in arithmetic itself. Now, it is reasonable to say that constructive methods should not go beyond arithmetic. Since Peano arithmetic is interpretable in Heyting arithmetic (Peano arithmetic formalized in logic without the principle of excluded middle via the Gödel translation: $\neg\neg A$ is a theorem of **HA** iff A is a theorem of **PA**; the symbol $\neg$ stands for the intuitionistic negation), the latter is also subjected to the second Gödel theorem. Now let CON(**HA**) abbreviate "**HA** is consistent". Due to the Gödel result, CON(**HA**) is not provable in **PA**. Should the intuitionist prove that **HA** is consistent? I say "yes". The intuitionist says that existence means consistency + the method of construction. Thus, consistency is a necessary, though not sufficient condition of existence in the intuitionistic sense, and this is a reason that the intuitionist should be able to prove that his basic theory, namely arithmetic, is consistent. Perhaps one will remark that something improper was smuggled in the above reasoning, namely that CON(**HA**) is to be provable by the intuitionist. In fact, if we inverse the link between **PA** and **HA**, we obtain that if A is not a theorem of **PA**, then $\neg\neg A$ is not a theorem of **HA**. Hence, $\neg\neg$CON(**HA**) is not a theorem of **HA**. The reasoning that leads to the result is of course classical. A possible counterattack of the intutionist is that we must distinguish $\neg\neg$CON(**HA**) and COH(**HA**). The former is too weak in order to capture the consistency of **HA** in the intuitionistic sense, but the latter also is defective as formalized by classical devices and it does not express the intuitive (intuitionistic) consistency of **HA**. We know, the intuitionist continues, that **HA** is consistent, because it is a true manisfestation of the Mathematician's Mind. I consider this way out as begging the question. It is really strange to say that consistency is a matter of intuitive faith, but everything else in mathematics is subjected to constructive proofs. The situation seems rather like this: either the intuitionist is not able to express CON(HA) in a rigorous way or he or she cannot constructively prove it (in fact, $\neg\neg$CON(**HA**) expresses something very close to the requirement of consistency; so if this formula is not intutionistically provable, a stronger one is unprovable too). No horn of this dilemma is nice.

Due to various circumstances, mainly works of Hilbert, Brouwer, Heyting, Gödel, Tarski and Skolem, the new situation in logic and the foundations of mathematics consisted in replacing old positions, heavily burdened by philosophical assumptions, by three new schemes: set theoretical (akin to logicism), proof theoretical (akin to formalism) and constructive (akin to intuitionism). I will take the first one as the point of reference. According to the set theoretical foundational project, mathematics is not reducible to logic, but to set theory. This brought the question concerning the limits of logic. Considering the further development, the question is to be reduced to another one: what is *the* logic? First-order or higher-order logic? If we add formalism to this business, we encounter another problem, namely the relation between proof and proof or, more generally, syntax and semantics. The Hilbert program was in fact based on a hope that all mathematical questions are solvable by finite (or combinatorial) syntactic methods. In Poland, due to Tarski, set theoretical methods resulted with the rise of rigorous semantics done by exact mathematical methods. I regard the Gödel incompleteness theorems and the Tarski undefinability theorem as signs of the limitations of syntax over semantics. These results so deeply changed the foundational scenario that we can properly speak about the semantic revolution (see Woleński 1999), which produced a new style of thinking in logic, the foundations of mathematics and philosophy. When we look at the interplay between syntax and semantics in formal theories, a natural question that arises is this: how to characterize constructions in which syntactic and semantic descriptions coincide? Perhaps we should answer that logic is the domain in which syntax and semantics are equivalent. What about constructivism? Well, it has its own merits because it is always good to know what the limitations of constructive methods are, that is, what can be constructively proved and what require other methods. However, constructivism is not directly involved in the problem of how syntax is related to semantics, because the latter is clearly non-constructive. So I will not touch constructivism in my further remarks. I will assume, somehow dogmatically, that constructive or effective procedures do not exceed primitive recursive arithmetic. This view seems to be a reasonable minimal understanding what should be included into the domain of constructive methods.

I take semantic revolution as being of the utmost importance. Yet, I understand other preferences, in particular pointing out that the most revolutionary work was given by Turing and consisted in elaborating the concept of computability. The reason for this view is obvious because of the significance of Turing's ideas for computer science which, their technological

effects and their influence on philosophy, particularly the philosophy of mind. It is highly probable that logic in the 21^{th} century will be dominated by the needs of theoretical informatics. On the other hand, I am inclined to think that ideas advanced by Turing still belong to syntax. If I can prophet something, I will do it by saying that sooner or later the Tarski--style computer semantics will become equally important as it is in logic of the second half of 20^{th} century. Even if not, let us look at the semantic revolution as a historical fact.

3. The rise of first-order logic (see Moore 1980, Moore 1988)

Famous (and less famous) axiomatizations at the end of the 19^{th} century, namely Dedekind's (number theory), Peano's (number theory) and Hilbert's (geometry) were second-order, due to axioms, like induction (Dedekind, Peano) and completeness (of real numbers) (Hilbert). Frege's logic was also second-order, and the system of *Principia* covered what is presently known as ω-logic. Nobody at that time made any difference between first--and higher-order logic. The situation changed about 1915 when Löwenheim proved his famous result, later improved by Skolem, about satisfiability of first-order formulas in the domain of natural numbers. What is interesting in this context is that Löwenheim was strongly influenced by Schröder and the algebraic tradition in logic. So, at least in this respect, this tradition became something more that only a historical blind path of the development of logic. Another important fact, which helped to see the difference between first and second-order logic consisted in the change of formalization of set theory. Zermelo's system distinguishes sets and their elements in this way that there are some objects (atoms), which are not sets. It invites elementary quantification (over atoms) and non-elementary one (over sets, their families, etc.). Von Neumann recommended (in the twenties) another approach, based on the principle that everything is a set. This principle, somehow at odds with ordinary intuition, but natural in the realm of mathematics, enabled to formalize the set theory as an elementary (first--order) construction.

First-order system logic was consciously extracted from the whole body of logic by Hilbert (see Hilbert-Ackermann 1928, Hilbert 1928). It is sometimes said that Hilbert accepted the view that all mathematical theories can be formalized in first-order language (the Hilbert thesis; see Pogorzelski 1994, p. 170). Historically speaking, this view is false. Hilbert never said something like that. He only came to the conclusion that all deductive rules of inference can be formalized in first-order logic. It was the reason that he became interested in first-order logic and its properties. In particular, Hilbert stated as an open problem the question whether first-order logic is (semantically) complete. The positive answer given by Gödel in 1930 was an essential contribution to Hilbert problem because it assured that every universally valid formula of first-order logic was provable. Hilbert had to be very happy that a semantic concept (validity) was reducible to a syntactic one, namely provability. Partial successes (Bernays, Schöfinkel, Ackermann, Gödel, Behmann) in attempts to solve the decision problem also looked promising. However, the hopes concerning the final solution of the decision problem became destroyed after 1936, when Church proved the undecidability of first-order logic. Yet, elementary logic remained the most secure logical quantification system, due to its completeness and, in the course of time, it attracted many logicians (including Tarski and Gödel). Also the *opus magnum* of Hilbert and Bernays (see Hibbert-Bernays 1934-1939) seems to favour first-order logic for its "hard" properties, like consistency, completeness or effective syntax. However, I do not know any explicit statement of Hilbert, Gödel or Tarski suggesting that logic should be reduced to **FOL**. In general, every textbook of mathematical logic, published since the 40s of the 20th century extracts **FOL** as a separate and basic order logic. It was Quine (see Quine 1970) who made the claim that the logic should be identified with **FOL**. This claim, rather philosophical than purely logical, began to be extensively discussed in the last twenty five years (see Westerståhl 1976, papers in Barwise-Feferman 1985, Shapiro 1991, papers in Shapiro 1996).

4. The rise of metamathematics and formal semantics

It is self-understandable that the Hilbert program with its demands of solvability of every mathematical problem (including the consistency of mathematics) by finitary devices resulted with a vital interest concerning properties of formal mathematical systems. It does not mean that this question was entirely overlooked by other logicians. Russell and Whitehead clearly saw that consistency and completeness (adequacy) are fundamental properties of logical systems. This is well documented by the following fragment of *Principia Mathematica* (Whitehead-Russell 1910, p. 12).

> "The proof of a logical system is its adequacy and completeness. That is: (1) the system must embrace among its deductions all those propositions

> which we believe to be true and capable of deduction from logical premisses alone, though possibly they may require some slight limitation in the form of an increased stringency of enunciation; and (2) the system must lead to no contradictions, namely by pursuing our inferences we must never be led to assert both p and not-p, i.e. both "$\vdash p$" and "$\vdash \neg p$" cannot legitimately appear."

However, the authors of *Principia Mathematica* did not think that consistency and completeness could be solved by applying exact mathematical methods. Post went further and proved formally (Post 1921) that propositional calculus was complete. He clearly saw that the problem of completeness is about propositional calculus but not a question, which could be solved within the system developed in *Principia Mathematica* (Post 1921, p. 21-22; page-reference to reprint):

> "We here wish to emphasize that the theorems of this paper are *about* the logic of propositions but are *not included* therein. More particularly, whereas the propositions of 'Principia' are *particular* assertions introduced for their interest and usefulness in later portions of thr work, those of the present paper are about the set of *all* such possible assertions."

On the other hand, there is nothing in Post, which would testify that he saw the completeness problem as an example of a wider project of the foundational research. It was Hilbert who consciously and systematically initiated such a program. He established metamathematics as a mathematical field. More specifically, metamathematics in the Hilbertian sense consisted in investigating formal, that is, logical and mathematical systems by so called finitary methods.

Hilbert's idea of metamathematics became popular in Poland. In fact, the Warsaw school of logic (Łukasiewicz, Leśniewski, Tarski, Lindenbaum and others) were doing some metamathematical work in the twenties of the 20th century, probably independently of any knowledge about the Hilbert project. In the end of the 20s, Tarski published two papers (Tarski 1930, Tarski 1930a) in which he defined and systematised many metamathematical concepts with a full consciousness that he followed Hilbert and his school (Tarski 1930a, p. 60; page-reference to English translation):

> "The deductive disciplines constitute the subject-matter of the *methodology of the deductive sciences*, which today, following Hilbert, is usually called *metamathematics*, in much sense in which spatial entities constitute the subject-matter of geometry and animals that of zoology. [...]. Strictly speaking

metamathematics is not to be regarded as a single theory. For the purpose of invetigating each deductive discipline a special metadiscipline should be constructed. The aim is *to make precise the meaning of a series of important metamathematical concepts* which are common to the special metadisciplines, *and to establish the fundamental properties of these concepts.* One result of this approach is that some concepts which can be defined on the basis of special metadiscplines will here be regarded as primitive concepts and characterized by a series of axioms."

Tarski did not remark, however, that his work was fairly different from that of Hilbert at a very basic point. The main difference is that Tarski did not restrict metamathematical methods to finitary ones. A famous Lindenbaum's lemma, which appeared in Tarski 1930 for the first time, namely the statement that every consistent set of sentences has a maximally consistent extension is perhaps the best exemplification of a metamathematical and non-constructive theorem. Tarski, after years, stressed that it was a crucial point that Warsaw school was more liberal in the repertoire of sound mathematical methods as metamathematical devices (Tarski 1954, p. 713; page-reference to reprint):

"As an essential contribution of the Polish school to the development of metamathematics one can regard the fact that from the very beginning it admitted into mathematical research all fruitful methods, whether finitary or not. Restriction to finitary methods seems natural in certain parts of metamathematics, in particular in the discussion of consistency problems, though even here these methods may be inadequate. At present it seems certain, however, tha exclusive adherence to these methods would prove a great handicap in the development of metamathematics."

Why did Polish logicians (with some exceptions, like Chwistek or Leśniewski) admit infinitary methods? It was due to the way of looking at set theory and its controversial problems, for instance the status of the axiom of choice. Perhaps this way of thinking is best represented by two following quotations, very similar in their content, although separated by a few decades (Sierpiński 1965, p. 94; Tarski 1962, p. 124; page-reference to reprint; in order to clarify the phrase "separated by a few decades", I note that Sierpiński's view was expressed by him in the twenties of 20th century):

"Still, apart from our personal inclination to accept the axiom of choice, we must take into consideration, in any case, its role in the Set Theory and in the Calculus. On the other hand, since the axiom of choice has been questioned by some mathematicians, it is important to know which theorems are proved with its aid and to realize exact point at which the proof has been based on

the axiom of choice; for it has frequently happened that various authors have made use of axiom of choice in their proof without being aware of it. And after all, even if no one questioned the axiom of choice, it would not be without interest to investigate which proofs are based on it, and which theorems can be proved without its aid – this, as we know, is also done with regard to other axioms."

"We would of course fully dispose of all problems involved [that is, concerning the existence of inaccessible cardinals – J. W.], if we decide to enrich the exiom system of set theory by including (on a permanent basis so to speak) a statement which precludes the existence of "very large cardinals", e.g. by a statement to a effect that every cardinal $> \omega$ is strongly incompact. Such a decision, however, would be contrary to what is regarded by many as one of the main aims of research in the foundations of set theory, namely, the axiomatization of increasingly large segments of "Cantor's absolute'. Those who share these attitude are always ready to accept new "constructions principles", new axioms securing the existence of new classes of "large" cardinals (provided they appear to be consistent with old axioms), but are not prepared to accept any axioms precluding the existence of such cardinals – unless this is done on a strictly temporary basis, for the restricted purpose of facilitating the metamathematical discussion of some axiomatic system of set theory."

Metamathematics in the understanding of Hilbert and (early Tarski) was restricted to syntactic matters. If we say that metamathematics is "about" mathematics and deals with its subject-matter by mathematical methods, nothing prevents us to add formal semantics to metamathematical investigations. Adjective "formal" is important here, because only formal semantics is done by mathematical methods. Since I described the development of semantics elsewhere, I restrict here to the facts concerning formal semantics. Two conceptions of semantics have to be distinguished. The best way to see the difference consists in an appeal to Frege's distinction between sense and reference, although he did not invent it in order to clarify various ways of semantic thinking. Traditionaly, the linguists considered semantics as devoted to studies about meanings (senses) and their changes. This understanding attracted also many philosophers, who often worried about meanings of expressions. The further development of formal semantics, at least that important for mathematical logic, gave priority to referential issues.

The Löwenheim-Skolem theorem and the Gödel completeness theorem are early semantic results. Post probably did not observe that his completeness theorem had an explicit semantic flavour. Neither Löwenheim, nor Skolem, nor Gödel defined semantic concepts, which they used, for instance domain, validity or satisfaction. These ideas were understood in their writings as it was practicized in the ordinary mathematical parlance of that time. Gödel himself clearly appreciated the importance of semantic methods. After years, he documented it in the following way with respect to the concept of truth (quoted after Wang 1996, p. 242):

> "[...] it should be noted that the heuristic principle of my construction of undecidable number theoretical proposition in the formal systems of mathematics is the highly transfinite concept of 'objective mathematical truth', as opposed to demonstrability [...] which with it was generally confused before my own and Tarski's work. Again, the use of this transfinite concept eventually leads to finitary provable results, fo example, the general theorems about the existence of undecidable propositions in consistent formal systems. [...]. A similar remark applies to the concept of mathematical truth, where formalists considered formal demonstrability to be an *analysis* of the concept of mathematical truth and, therefore, were of course not in position to *distinguish* the two."

Gödel even accused Skolem for being insensitive to non-finitistic methods (in fact, to semantic matters) and thought that it prevented the latter to prove the completeness theorem (quoted after Wang 1974, p. 7-8):

> "The completeness theorem, mathematically, is indeed an almost trivial consequence of Skolem 1922. However, the fact is that, at that time, nobody (including Skolem himself drew this conclusion (neither from Skolem 1922 nor, as I did, from similar consideration of his own. [...] This blindness (or prejudice [...]) of logicians is indeed surprising. But I think the explanation is not hard to find. It lies in widespread lack, at that time, of the required epistemological attitute toward metamathematics and toward non-finitary reasoning."

Yet, as I already mentioned, Gödel himself also did not define semantic notions. Why? It seems that Gödel did not believe that they are subjected to rigorous mathematical treatment, although he always considered semantic intuitions as very powerful heuristic pathways.

It was Tarski who introduced semantics as a part of metamathematics in his seminal treatise on the concept of truth in formalized languages. The success of formal semantics was rooted in four factors. First, the fear of semantic antinomies had to be overcome. Perhaps it was more important in Poland than in other countries, because Polish logicians with their considerable philosophical inheritance were more sensitive to the problem of antinomies than their mostly mathematical colleagues in other countries. Secondly, a new conception of logic (see van Heijenoort 1967) had to replace the old one. This new conception considered logic as a reinterpretable calculus, contrary to the looking at logic as a language (as the universal medium; see Hintikka 1989, Kusch 1989, Woleński 1997a). The second conception, shared by Frege, to some extent by Russell and very radically by Wittgenstein, prevented any serious talk about relations between language and its referential relations to something else. In particular, it makes the distinction between language and metalanguage (crucial for semantics) simply meaningless. Quite contrary, logic conceived as reinterpretable calculus naturally suggested that language referred to something which was dependent on interpretation. Thirdly, language and its referential relations had to be dressed in a mathematical manner. It was done by recursive definition of language as a set of sentences and assuming that the concepts of interpretation and satisfaction, which establish the link between language and its subject matter, are also recursive. Thus, syntax and semantics became compositional. Sometimes it is regarded as a too restrictive approach, which does not fit intensional language, but the way of overcoming meanders of intensionality is unclear until now. Fourthly, semantics required infinitary methods. It is remarkable that the first mention of satisfiability by Tarski appeared in his paper on definable real numbers, that is, on the occasion of considering problems of descriptive set theory, which makes heavy use of infinitary methods. If we look at circumstances associated with the development of formal semantics, nothing is peculiar as compared with syntax. Both semantics and syntax use infinitary methods and both are compositional. What is then the difference between them? The fundamental question is this: is it possible to define semantic relations by syntactic machinery? The general answer, well motivated by basic metamathematical results, is: no (see Woleński 1997 for further philosophical comment about the relation between syntax and semantics). I claim that **FOL** is the only logic in which syntax and semantic are parallel in the way which can be called "logical", but this qualification must be somehow restricted even in this case.

5. First-order logic and its basic properties

There is an ambiguity in conceiving logic, even in the case of **FOL**. A more traditional account considers logic as the set of theorems derived from suitably adapted axioms by proper inference rules, for instance, modus ponens. According to another view, logic is a pair $\langle \boldsymbol{L}, Cn\rangle$, where $\boldsymbol{L}$ is a language and Cn is a consequence operation, which operates on $\boldsymbol{L}$. Under the second understanding, logic produces theorems from some assumptions dependent on the considered subject matter, for example, arithmetic or geography. On the other hand, logic as the set of theorems consists of propositions assumed to be tautologies, at least in the case of first-order logic. We can try to reconcile (at least, to some extent) both approaches to logic by the following way. Firstly, we introduce (first--order) Cn axiomatically by stipulating the following postulates (this way of introducing the consequence operation goes back to Tarski; see Tarski 1930, Tarski 1930a):

(C1) $\emptyset \leq \boldsymbol{L} \leq \aleph_0$

(C2) $X \subseteq CnX$

(C3) if $X \subseteq Y$, then $CnX \subseteq CnY$

(C4) $CnCnX = CnX$

(C5) if $A \in CnX$, then $\exists Y \subseteq X \wedge Y \in \mathbf{FIN}(A \in CnY)$

(C6) if $B \in Cn(X \cup \{A\})$, then $(A \rightarrow B) \in CnX$, provided that A, B and element of X are sentences, that is, closed formulas.

(C7) if $(A \rightarrow B) \in CnX$, then $B \in Cn(X \cup \{A\})$

(C8) $Cn\{A, \neg A\} = \boldsymbol{L}$

(C9) $Cn\{A\} \cap Cn\{\neg A\} = \emptyset$

(C10) $A(t_i/x_i) \in Cn\{\forall x_i A\}$, if the term t_i is substitutable in A for x_i;

(C11) $(A \rightarrow \forall x_i B) \in Cn\{\forall x_i(A \rightarrow B)\}$, if x_i is not free in A;

(C12) $\forall x_i A \in Cn\{A\}$.

Then, we define logic by

(D) $\mathbf{LOG} = Cn\emptyset$.

I will not enter into a deper motivation for defining logic as the set of consequences of the empty set (see Woleński 1998 for a more extensive discussion discussion). At the moment the observation will suffice that (D) sees logic as independent of any particular assumptions, that is, connected with specific domains. In the case of first-order logic, (D) has an additional justification in the (weak) completeness theorem

(WCT) $A \in Cn\emptyset$ if and only if A is universally valid, that is true in all models.

Thus, assuming (D), the completeness theorem establishes the parity between derivability from the empty set and the universal validity. Since

universality was always conceived as a basic property of logic, it gives a strong evidence for (D) as an intuitive definition of logic. The strong completeness theorem is important, when we understand logic as $\langle \boldsymbol{L}, Cn\rangle$. It is the statement

(SCT) $A \in CnX$ if and only if A is true in all models of the set X of sentences.

Of course, if we take the empty set instead of X, (SCT) becomes (WCT). Unfortunately, completeness does not select a logic uniquely, because it is a property of many formal systems. It is always possible to state axioms for Cn, that it will allow us to define the resulting systems as Cn. Some such systems are semantically complete, others not. Thus, semantic completeness as a property does not separate first-order logic from other candidates for being logics. Thus, we must look for an other characterization of first-order logic. One hint comes from the following theorem:

(NDC) **FOL** does not distinguish any extralogical content, that is, if something is provable in this logic about an individual object, property or relation denoted by a predicate letter, the same is also provable about any other object, property or relation.

This feature is certainly desirable, if we like to keep the intuition that logic is independent of specific subject-matters.

The recent and most popular characterization of first-order logic comes from the Lindström theorem (more strictly, one of theorems of this sort; note that this theorem applies above all to logic understood as $\langle \mathbf{B}, Cn\rangle$).

(L) A logic **LOG** is equivalent to **FOL** if and only if the following conditions hold:
(a) **LOG** is effectively regular (its syntax is recursive, its formulas are finite);
(b) **LOG** has Boolean connectives;
(c) **LOG** is compact (it has a model if its every finite subset has a model);
(d) **LOG** satisfies the downward Löwenheim-Skolem theorem (if it has an infinite model, it has a countable model).

This theorem says that **FOL** is the strongest logic, which jointly possesses properties (La)–(Ld). For instance, second-order logic is neither compact nor satisfies the Löwenheim-Skolem theorem

(L) is in its main part a semantic theorem and it has no clear syntactic counterpart, although the regularity of **FOL** and the Boolean character of its connectives are guaranteed by properties of *Cn*. If we have the completeness theorem, compactness as related to models, can be mapped on the finiteness of syntactic consistency. However, no simple syntactic counterpart of the downward Löwenheim-Skolem theorem is available. What is known is the Lindenbaum maximalization theorem for languages with uncountably many constants is equivalent to the downward Löwenheim-Skolem (see Surma 1968) theorem, but it goes beyond syntax of **FOL**. (L) is surprising, because it characterizes **FOL** by unexpected properties as basic. A more discussed matter concerns the consequences of (L) for the expressive power of **FOL**. Its compactness and (d) decide that many concepts important for mathematics, for instance, finiteness, are simply not definable in first-order languages. Moreover, first-order theories are automatically non-categorical, because, due to the Löwenheim-Skolem theorem, they have models, which differ in cardinalities. These properties of **FOL**, in particular, the limitations of its expressive power, are commonly regarded as disadvantages of first--order logic and reasons for favouring higher-order logics or logic with infinitely long formulas (see Shapiro 1991 for the first option and Barwise and Feferman 1985 for more wide spectrum of possibilities).

I have some reservations toward criticism of **FOL** via (L). Although I agree that (L) is surprising, I guess (similarly Tharp 1975) that properties listed in (L) are important for understanding logic and they clarify some controversial issue. (L) does not mention the completeness theorem which seems crucial for the concept of logic. However, we can say that (L) shows that (WCT), if it is considered as a supplement of (D), must be understood in a way. More specifically, since the completeness phenomenon is much more general than the related property of **FOL**, and it is too wide as a mark of logic, (L), so to speak, restricts (WCT) to a proper shape. Let me explain the point in such a way. Compactness and the downward Löwenheim-Skolem theorem are consequences of the completeness theorem (in the version: every consistent set of first-order formulas has a model), but only in **FOL**. It is clear, because second-order logic is complete but it does not satisfy conditions (Lc) and (Ld). It suggests that completeness in second-order logic means something different than in the case of **FOL**. Formally, everything is similar: every consistent set of second-order formulas has a model. On the other hand, it is well-known that the Henkin proof of the completeness theorem for second-order logic, introduces some distinction in the class of all models. Thus, not every model of second-order logic is treated in the same way. Putting it in another way, extralogical elements are present in selecting second-order models. This fact, together with the lack of recursive axiomatization of second-order modes of inference (I mentioned

it earlier), shows that second-order (a fortiori, higher-order too) logic has feature which are not proper for logic. Nothing like that occurs in the case of **FOL**. In particular, the Löwenheim-Skolem theorem says that all models of a first-order language are equal, independently of their cardinality. Hence, the definition of logic as the set of sentences true in all models and its equivalence with (D), has its full sense only in **FOL**. (WCT), (NDC) and (L) collectively taken exhibit fundamental properties of fist-order logic, in particular, contribute to a better understanding of its universality. If we insist that logic, should be universal, **FOL** satisfies this requirement to the greatest extent.

6. Final remarks

First-order logic certainly has serious expressive limitations. On the other hand, second-order logic, although richer in content, does not admit any recursive definition of its deductive machinery. Although the concept of logical (semantic) consequence is the same in all types of logic, it is not everywhere replaceable by effectively given proof-procedures. Now, one must decide what is expected from logic: a powerful expressive power or recursive production of the modes of inference, closely connected with the strict parallelism of syntax and semantics. The moral from my discussion is that both aims cannot be simultaneously achieved. Something must be chosen. As I already indicated, the strict parallelism of syntax and semantics is restricted even in the case of **FOL**. It is due to the fact that the completeness theorem for first-order logic, contrary to propositional calculus has no constructive proof. Thus, effective syntax is mapped into non-effective semantics and reversely, due to the use of methods, which are not constructive. Another point to be observed is that **FOL** has some extralogical aspects, at least two. Firstly, it is based on an assumption that something exists. Secondly, the identity predicate is somehow between logical and extralogical notions. On the one hand, first-order logic with identity obeys all principal metatheorems, which hold for **FOL** without identity, but, on the other hand, identity allows us to define numerical quantifiers (for example, "there are exactly two objects"), which are purely not logical items. I only note these points without entering into a deeper discussion of them.

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STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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ON PROOFS OF THE CONSISTENCY OF ARITHMETIC

1. The main aim and purpose of Hilbert's programme was to defend the integrity of classical mathematics (refering to the actual infinity) by showing that it is safe and free of any inconsistencies. This problem was formulated by him for the first time in his lecture at the Second International Congress of Mathematicians held in Paris in August 1900 (cf. Hilbert, 1901). Among twenty three problems Hilbert mentioned under number 2 the problem of proving the consistency of axioms of arithmetic (under the name "arithmetic" Hilbert meant number theory and analysis).

Hilbert returned to the problem of justification of mathematics in lectures and papers, especially in the twentieth[1], where he tried to describe and to explain the problem more precisely (in particular the methods allowed to be used) and simultaneously presented the partial solutions obtained by his students.

Hilbert distinguished between the unproblematic, finitistic part of mathematics and the infinitistic part that needed justification. Finitistic mathematics deals with so called real sentences, which are completely meaningful because they refer only to given concrete objects. Infinitistic mathematics on the other hand deals with so called ideal sentences that contain reference to infinite totalities. It should be justified by finitistic methods – only they can give it security (*Sicherheit*). Hilbert proposed to base mathematics on finitistic mathematics via proof theory (*Beweistheorie*). It should be shown that proofs which use ideal elements in order to prove results in the real part of mathematics always yield correct results, more exactly, that (1) finitistic mathematics is conservative over finitistic mathematics with respect to real sentences and (2) the infinitistic

[1] More information on this can be found for example in (Mancosu, 1998).

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mathematics is consistent. This should be done by using finitistic methods only.

2. It seems that the first result in this direction was obtained by Wilhelm Ackermann in 1924. In his paper "Begründung des 'tertium non datur' mittels der Hilbertschen Theorie der Widerspruchsfreiheit" (cf. Ackermann, 1924) Ackermann gave a finitistic proof of the consistency of arithmetic of natural numbers without the axiom (scheme) of induction. In fact it was a much weaker system than the usual systems of arithmetic but the paper provided the first attempt to solve the problem of consistency. Add that Ackermann used in (1924) a formalism with Hilbert's ε-functions.

3. Next attempt to solve the second Hilbert's problem was the paper by Janos (later Johann, John) von Neumann "Zur Hilbertschen Beweistheorie" published in 1927. He used another formalism than that in (Ackermann, 1924) and, similarly as Ackermann, proved in fact the consistency of a fragment of arithmetic of natural numbers obtained by putting some restrictions on the induction. It is worth mentioning here that in the introductory section of von Neumann's paper a nice and precise formulation of aims and methods of Hilbert's proof theory was given. It indicated how was at that time the state of affairs and how Hilbert's programme was understood. Therefore we shall quote the appropriate passages.

Von Neumann writes that the essential tasks of proof theory are (cf. von Neumann, 1927, 256–257):

I. First of all one wants to give a proof of the consistency of the classical mathematics. Under 'classical mathematics' one means the mathematics in the sense in which it was understood before the begin of the criticism of set theory. All settheoretic methods essentially belong to it but not the proper abstract set theory. [...]

II. To this end the whole language and proving machinary of the classical mathematics should be formalized in an absolutely strong way. The formalism cannot be too narrow.

III. Then one must prove the consistency of this system, i.e., one should show that certain formulas of the formalism just described can never be "proved".

IV. One should always strongly distinguish here between various types of "proving": between formal ("mathematical") proving in a given formal system and contents ("metamathematical") proving [of statements] about the system. Whereas the former one is an arbitrarily defined logical game (which should to a large extent be analogues to the classical mathematics), the latter is a chain of directly evident contents insights. Hence this "contents proving" must proceed according to the intuitionistic logic of Brouwer and Weyl. Proof theory should so to speak construct classical mathematics on the intuitionistic base and in this way lead the strict intuitionism ad absurdum[2].

Note that von Neumann identifies here finitistic methods with intuitionistic ones. This was then current among members of the Hilbert's school. The distinction between those two notions was to be made explicit a few years later – cf. (Hilbert and Bernays, 1934, pp. 34 and 43) and (Bernays 1934, 1935, 1941).

4. In 1930 Kurt Gödel obtain a result which undermined Hilbert's programme. Gödel proved that any consistent theory extending the arithmetic of natural numbers and based on a recursive set of axioms is incomplete (this result is called today Gödel's First Incompleteness Theorem). This result was announced for the first time by Gödel during a conference in Königsberg in September 1930. It seems that the only participant of the conference in Königsberg who immediately grasped the meaning of Gödel's theorem and understood it was J. von Neumann. After Gödel's talk he had a long discussion with him and asked him about details of the proof. Soon after coming back from the conference to Berlin he wrote a letter to Gödel (on 20th November 1930) in which he announced that he had received a remarkable corollary from Gödel's First Theorem, namely a theorem on the unprovability of the consistency of arithmetic in arithmetic itself. In the meantime Gödel developed his Second Incompleteness Theorem

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[2] I. In erster Linie wird der Nachweis der Widerspruchsfreiheit der klassischen Mathematik angestrebt. Unter „klassischer Mathematik" wird dabei die Mathematik in demjenigen Sinne verstanden, wie sie bis zum Auftreten der Kritiker der Mengenlehre anerkannt war. Alle mengentheoretischen Methoden gehören im wesentlichen zu ihr, nicht aber die eigentliche abstrakte Mengenlehre. [...]

II. Zu diesem Zwecke muß der ganze Aussagen- und Beweisapparat der klassischen Mathematik absolut streng formalisiert werden. Der Formalismus darf keinesfalls zu eng sein.

III. Sodann muß die Widerspruchsfreiheit dieses Systems nachgewiesen werden, d.h. es muß gezeigt werden, daß gewisse Aussagen „Formeln" innerhalb des beschriebenen Formalismus niemals „bewiesen" werden können.

IV. Hierbei muß stets scharf zwischen verschiedenen Arten des „Beweisens" unterschieden werden: Dem formalistischen („mathematischen") Beweisen innerhalb des formalen Systems, und dem inhaltlichen („metamathematischen") Beweisen über das System. Während das erstere ein willkürlich definiertes logisches Spiel ist (das freilich mit der klassischen Mathematik weitgehend analog sein muß), ist das letztere eine Verkettung unmittelbar evidenter inhaltlicher Einsichten. Dieses „inhaltliche Beweisen" muß also ganz im Sinne der Brouwer-Weylschen intuitionistischen Logik verlaufen: Die Beweistheorie soll sozusagen auf intuitionistischer Basis die klassische Mathematik aufbauen und den strikten Intuitionismus so ad absurdum führen.

and included it in his paper "Über formal unentscheidbare Sätze der 'Principia Mathematica' und verwandter Systeme. I" (cf. Gödel, 1931). In this situation von Neumann decided to leave the priority of the discovery to Gödel.

In fact in (Gödel, 1931) one finds only a statement of the theorem on the unprovability of consistency (called today Gödel's Second Incompleteness Theorem) and a remark that it can be proved by formalizing the proof of the first theorem. Gödel promissed also there to publish the full proof in the second part of the paper which would be ready soon. But this second part was never written and Gödel published in fact no proof of his second theorem. Moreover, his remark on the proof was not correct. The first proof of the theorem on the unprovability of consistency appeared in the second volume of Hilbert and Bernay's monograph *Grundlagen der Mathematik* (1939). It has turned out that the way in which the metamathematical sentence "the theory T is consistent" is formalized in the formal language of T is significant here. Hilbert and Bernays formulated certain so called derivability conditions for formulas representing in T the metamathematical notion of provability in T (in fact those conditons require certain internal properties of provability to be formally derivable in T). If those conditions are fulfilled then the second incompleteness theorem holds.

Hilbert-Bernay's conditions were not elegant. A useful and elegant form of them was given by M. H. Löb in 1954 (cf. Löb, 1955). It was also shown that there exist formal translations of the sentence "T is consistent" which are provable in T and for which the second incompleteness theorem fails. Examples of such formulas were given by J. B. Rosser and A. Mostowski[3].

Those results weakened in a sense (the metamathematical and philosophical meaning of) Gödel's Second Incompleteness Theorem. In fact this theorem does not say simply that Peano arithmetic, if consistent, cannot prove its own consistency (and similarly for any consistent extension of it). It turns out that the way in which the metamathematical property of consistency is expressed in the language of the considered theory plays here the crucial role. The crude numerical adequacy in the sense of strong representability is not enough here – one needs in fact that the formal representation "reflects" the very structure of the notion of provability (cf. Feferman, 1960). Nevertheless Gödel's theorem indicated certain limitations of formalized systems and showed that certain corrections in Hilbert's programme are necessary.

[3] For technical as well as philosophical and historical information on Gödel's theorems see, e.g., (Murawski, 1999).

In spite of those new circumstances Hilbert defended the very idea of his programme. In the Preface to the first volume of *Grundlagen der Mathematik* he wrote:

> [...] the occasionally held opinion that from the results of Gödel follows the non-executability of my Proof Theory, is shown to be erroneous. This result shows indeed only that for more advanced consistency proofs one must use the finite standpoint in a deeper way than is necessary for the consideration of elementary formalisms[4].

5. Through von Neumann about Gödel's incompleteness theorems learned (in November 1930) Jacques Herbrand. He found them to be of great interest. They also stimulated him to reflect on the nature of intuitionistic proofs and of schemes for the recursive definition of functions. In a letter to Gödel of 7th April 1931 Herbrand suggested the idea of extending the schemes for the recursive definition of functions. His remarks inspired Gödel to formulate the notion of general recursive function (in the lectures he gave at Princeton in 1934 – cf. Gödel, 1934).

From the point of view of the present paper however more important is Herbrand's paper "Sur la non-contradiction de l'arithmétique" published in 1931 already after the Gödel's "Über formal unentscheidbare Sätze...". Herbrand probably started to write his paper before Gödel's paper reached him (the manuscript sent for publication to the *Journal für reine und angewandte Mathematik* was dated "Göttingen, 14 July 1931"; it was sent just before Herbrand left for a vacation trip in the Alps, and was received on 27 July 1931 – on that day Herbrand was killed in a fall). Nevertheless, he had opportunity to examine Gödel's results (in particular his second theorem) and in the last section of his paper he was dealing with them.

Herbrand's paper presents a proof of the consistency of a fragment of arithmetic of natural numbers. It was certainly intended to be a contribution to the realization of Hilbert's programme. The fragment considered by Herbrand is arithmetic with induction for formulas containing no bounded variables and induction for formulas containing bounded variables but

[4] [...] die zeitweilig aufgekommene Meinung, aus gewissen neueren Ergebnissen von Gödel folge die Undurchführbarkeit meiner Beweistheorie, als irrtümlich erwiesen ist. Jenes Ergebnis zeigt in der Tat auch nur, daß man für die weitergehenden Widerspruchsfreiheitsbeweise den finiten Standpunkt in einer schärferen Weise ausnutzen muß, als dieses bei der Betrachtung der elementaren Formalismen erforderlich ist.

containing no function symbols except eventually the successor function. The proof uses Herbrand's fundamental theorem[5] (section 1 consists of a very clear presentation of this theorem).

It is worth noting here that Herbrand, similarly as von Neumann (see above), uses the name "intuitionistic" to describe methods which are allowed in the metamathematics, hence finitistic methods. This identification was then current in Hilbert's school.

The key trick of Herbrand's proof of the consistency of the indicated fragment of aritmetic is the elimination of the induction axiom scheme through the introduction of functions. The definition conditions for those functions are such that, for every set of arguments, a well-determined number can be proved in a finitary way to be the value of the function. It should be noted that those functions are (general) recursive functions. This is in fact the first appearance of the notion of a general recursive function as opposed to primitive recursive (cf. Gödel's definition of general recursive functions from 1934 "suggested by Herbrand" – see Gödel, 1934, p. 26).

As indicated above, in the last section of his paper (1931) Herbrand considered the problem of connections between his result and Gödel's theorem on the unprovability of consistency. He explains very clearly why the latter does not hold for the fragment of arithmetic he considers. The reason is that the metamathematical description of the system cannot be projected into the system itself (because the system is too weak).

6. First proof of the consistency of the arithmetic of natural numbers was given by Gerhard Gentzen in the paper "Die Widerspruchsfreiheit der reinen Zahlentheorie" (1936) (cf. also his paper "Neue Fassung des Widerspruchsfreiheitsbeweises für die reine Zahlentheorie" from 1938). According to Gödel's Second Incompleteness Theorem a proof of the consistency of the full arithmetic of natural numbers should use means stronger than those available in the arithmetic itself (modulo the restrictions concerning the way of expressing in the formal language the property of consistency). Indeed the analysis of Gentzen's proof shows that it is just in the concept of a reduction process applied by Gentzen in (1936) that the transgression of the methods formalizable in the formal system under

[5] This theorem contains a reduction (in a certain sense) of predicate logic to propositional logic, more exactly it shows that a formula is derivable in the axiomatic system of quantification logic if and only if its negation has a truth-functionally inconsistent expansion. Herbrand intended to prove this theorem by finitistic means. The theorem was contained in Chapter 4 of his doctoral dissertation presented to the Sorbonne in 1930 and published in the same year – cf. Herbrand, 1930.

consideration comes about. By assigning ordinals to the derivations one sees that the transfinite induction up to ε_0 suffices for the proof[6].

It is worth noting here that the first version of Gentzen's consistency proof was submitted in 1935 but was withdrawn after criticism directed against the means used in the proof which were considered to be too strong. Gentzen took care of the criticism and modified his original proof before it was published (the modified proof was published in the paper (1936)). Fortunately the text of the original proof was preserved in galley proof. It became publically known because of the paper by Bernays (1970) and was recently published in the name of Gentzen (cf. Gentzen, 1974). Bernays remarks in (1970) that Gentzen's original proof was certainly easier to follow than the first published proof and at least as easy to follow as the second Gentzen consistency proof from (1938).

7. Gentzen's proof was apparently accepted by Hilbert and Bernays in the second volume of *Grundlagen der Mathematik* (1939). Indeed, in the Preface Bernays wrote there (p. VII):

> In any case one can say on the basis of Gentzen's proof that the short-lived failure of proof theory was caused solely by the whimsicality of the methodological demand put on it[7].

In the same Preface it was also announced that W. Ackermann is working on extending his earlier consistency proof (published in 1927) along the lines indicated by Gentzen, i.e., by applying the transfinite induction. Indeed, in 1940 appeared Ackermann's paper "Zur Widerspruchsfreiheit der Zahlentheorie" in which the consistency of the full arithmetic of natural numbers was proved by using methods from his paper (1927) and the transfinite induction.

Since then other proofs along Gentzen's lines have been published. One should mention here among others papers by Lorenzen (1951), Schütte (1951, 1960) and Hlodovskii (1959).

[6] The countable ordinal ε_0 is defined as the smallest ordinal ε such that $\omega^\varepsilon = \varepsilon$ or as the limit of the sequence $\omega, \omega^\omega, \omega^{\omega^\omega},\ldots$

[7] Jedenfalls kann schon auf Grund des Gentzenschen Beweises die Auffassung vertreten werden, daß das zeitweilige Fiasko der Beweistheorie lediglich durch eine Überspannung der methodischen Anforderung verschuldet war, die man an die Theorie gestellt hat.

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STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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LATTICE PROPERTIES OF A PROTOLOGIC INFERENCE

A protologic construction of q-consequence in [1] has been designed as a formal counterpart of reasoning admitting rules of inference which lead from non-rejected premises to accepted conclusions. The very concept is a generalisation of Tarski consequence and, as such, it may be investigated similarly cf. [3]. In the paper we present results concerning the lattice $Q(L)$ of all q-consequence operations of a given sentential language L.

1. Q-consequence as a protologic inference

The concept of a q-consequence was introduced in [1] for the purpose of formalization of reasoning leading from non-rejected premisses to accepted conclusions. Accordingly, the first approach is based on extended logical matrices having two disjoint sets of distinguished values: rejected and accepted. The so-called q-matrix consequence relation, on its turn, imitated an inference not necessarily accepting the rule of unlimited repetition.

Let $L = (For, F_1, \ldots, F_m)$ be a sentential language. Formulas, i.e. elements of For, are then built from variables using the operations $F_1, \ldots, F_m$ representing the sentential connectives. In algebraic terms, L is freely generated by the set of its variables, $Var = \{p, q, r, \ldots\}$. A *q-matrix* is a triple

$$M^* = (A, D^*, D),$$

where A is an algebra similar to L and D^*, D are disjoint subsets of A interpreted as sets of *rejected* and *distinguished* values of M, respectively. For any such M^* one defines the relation $\vdash_{M*}$ between sets of formulae and formulae, a *matrix q-consequence of* M^* putting for any $X \subseteq For, \alpha \in For$

$$X \models_{M^*} \alpha \text{ iff for every } h \in Hom(L, A)(h\alpha \in D \text{ whenever } hX \cap D^* = \emptyset)$$

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The q-concepts reduce to usual concepts of matrix and consequence only if $D^* \cup D = A$, i.e. when the sets D^* and D are complementary. In that case the set of rejected elements coincides with the set of non-designated elements. So, the first set may be omitted and what then we have is a *matrix*

$$M = (A, D)$$

based on the same algebra of values and D serving as the set of distinguished elements.

The matrix consequence relation $\models_M \subseteq 2^{For} \times For$ for M,

$$X \models_M \alpha \text{ iff } h \in Hom(L, A)(h\alpha \in D \text{ whenever } hX \subseteq D),$$

may obviously be regarded as a special q-matrix consequence.

Given a matrix M for a language L, the system $E(M)$ of sentential logic is defined as the *content* of M, i.e. the set of all formulas taking for every valuation h (a homomorphism) of L in M. Thus

$$E(M) = \{\alpha \in For : \text{for every } h \in \mathrm{Hom}(L, A), h(\alpha) \in D\}.$$

Notice, that $E(M) = \{\alpha : \emptyset \models_M \alpha\} = \{\alpha : \emptyset \models_{M^*} \alpha\}$. This obviously means that both, the matrix consequence, and q-matrix consequence may serve as inferential extensions of the single logical system.

With every $\models_{M^*}$ there may be uniquely associated an operation $Wn_{M^*} : 2^{For} \to 2^{For}$ such that

$$\alpha \in Wn_{M^*}(X) \text{ if and only if } X \models_{M^*} \alpha.$$

called a *q-matrix consequence operation* of M^*. $Cn_M : 2^{For} \to 2^{For}$ defined by

$$\alpha \in Cn_M(X) \text{ if and only if } X \models_M \alpha,$$

a *matrix consequence operation* of M, is a special case of Wn_{M^*}.

As known, the concept of structural sentential logic is the ultimate generalisation of the notion of the matrix consequence operation. A structural logic for a given language L is identified with *Tarski's consequence* $C : 2^{For} \to 2^{For}$,

(T0) $X \subseteq C(X)$

(T1) $C(X) \subseteq C(Y)$ whenever $X \subseteq Y$

(T2) $C(C(X)) = C(X)$,

satisfying the following condition of *structurality*

(S) $eC(X) \subseteq C(eX)$ for every *substitution* of L.

cf. [3].

The investigation in [1] show^ that similar generalisation of the q-matrix consequence aims at the theory of the *q-consequence operation* $W : 2^{For} \to 2^{For}$ satisfying the following postulates:

(T1) $W(X) \subseteq W(Y)$ whenever $X \subseteq Y$

(T2) $W(X \cup W(X)) = W(X)$,

and, possibly,

(S′) $eW(X) \subseteq W(eX)$ for every *substitution* of L.

2. The lattice *Q(L)* – general properties

Let $L = (For, F_1, \dots, F_m)$ be a given sentential language and let $Q(L)$ be a class of all q-consequence operations on L.

Consider $W_1, W_2 \in Q(L)$. If $W_1(X) \subseteq W_2(X)$ for any $X \subseteq For$, then we say that W_1, is weaker than W_2, or that W_2, is stronger than W_1 and we write $W_1 \leq W_2$. Since $\subseteq$ partially orders the powerset of the set of formulas, 2^{For}, we obtain

2.1. $\leq$ is a partial ordering in $Q(L)$.

Where $U \subseteq Q(L)$, let $Sup(U)$ and $Inf(U)$ denote the least upper bound and the greatest lower bound, respectively, i.e.

(1) $Sup(U)$ is the weakest q-consequence operation in $Q(L)$, such that for every $W \in U$, $W \leq Sup(U)$,

(2) $Inf(U)$ is the strongest q-consequence operation in $Q(L)$, such that for every $W \in U$, $inf(U) \leq W$.

In what follows, we adopt a standard concept of a rule inference R as a set of sequents (X, α), i.e. $R \subseteq 2^{For} \times For$; cf. [3]. Next, we also need the notion of a q-consequence operation $W_{n_{\mathsf{R}}} : 2^{For} \to 2^{For}$ based on the set R of rules of inference. Thus, for any $X \subseteq For$

$$W_{n_{\mathsf{R}}}(X) = \bigcap\{Y \subseteq For : Y \text{ is } \mathsf{R}\text{-closed relative to } X\}$$

Recall, that $Y \subseteq For$ is R-closed relative to $X \subseteq For$ if and only if for each $(Z, \alpha) \in R \in \mathsf{R}$ if $Z \subseteq X \cup Y$, then $\alpha \in Y$; cf. [1].

Finally, let $Rule(W)$ denote the set of all rules of inference of a given q-consequence W:

$$Rule(W) = \{R : \text{ for every } (X, \alpha) \in R,\ \alpha \in W(X)\}.$$

Clearly, $Rule(W)$ is the biggest inferential basis for W.

2.2. For any $W_1, W_2 \in Q(L)$

$$W_1 \leq W_2 \text{ iff } Rule(W_1) \subseteq Rule(W_2).$$

Assuming that $U = \{W_i : i \in I\}$ we get the characterisation theorems concerning $Sup(U)$ and $Inf(U)$:

2.3. $Sup(U) = W_Q$, where $Q = \bigcup\{Q_i : W_i = Wn_{Q_i}\}$.

PROOF. Since $Rule(W_i) \subseteq Rule(Sup(U))$, for every $W_i \in U, W_i \leq Sup(U)$. On the other hand, for any W^* such that $W_i \leq W^*$ for all $W_i \in U$ we get $\bigcup\{Rule(W_i) : W_i \in U\} \subseteq Rule(W^*)$. So $W_Q = W_{\bigcup\{Rule(W_i):W_i \in U\}} \leq W^+$ and, therefore, $W_Q \leq W^+$.

2.4. $Inf(U) = \bigcap\{W_i : i \in I\}$, i.e. for every $X \subseteq For$ $Inf(U)(X) = \bigcap\{W_i(X) : i \in I\}$.

PROOF. Assume that $W^* : 2^{For} \to 2^{For}$ is an operation defined as:

$$W^*(X) = \bigcap\{W_i(X) : i \in I\}.$$

What then we have to prove as first is that W^* is a q-consequence, i.e. that it satisfies the conditions $(W1)$ and $(W2)$. W^* obviously satisfies $(W1)$. To check that it also satisfies $(W2)$ let us assume that for some $X \subseteq For$ and $\alpha \in For$, $\alpha \in W^*(W^*(X) \cup X)$. Then, also, for every $i \in I$, $\alpha \in W_i(W^*(X) \cup X)$ and due to $W^*(X) \subseteq W_i(X)$, $\alpha \in W_i(W_i(X) \cup X)$. Since each W_i is a q-consequence we obtain that $\alpha \in W_i(X)$ and, consequently, $\alpha \in W^*(X)$. So, $W^*(W^*(X) \cup X) \subseteq W^*(X)$. Taking into account that the reverse inclusion is implied by $(W1)$, the task is concluded.

Assume now that W^- is a q-consequence weaker than all $W_i^!$. Thus $W^-(X) \subseteq W_i(X)$ for every $X \subseteq For$ and every $i \in I$. Therefore, $W^-(X) \subseteq W^*(X) = \bigcap\{W_i(X) : i \in I\}$. This justifies that $W^* = Inf(U)$. $Inf(U)(X) = W^*(X) = \bigcap\{W_i(X) : i \in I\}$.

2.5. Corollary. $Q(L)$ is a complete lattice under $\leq$.

Now, let $R \subseteq Rule(L)$ be any set of inference rules in L. Next, let $Q_R(L)$ be the subclass of all q-consequence operations with the very property of having (at least) all rules in R:

$$Q_R(L) = \{W : W \in Q(L) \text{ and } R \subseteq Rule(L)\}$$

2.6. $Q_R(L)$ is a complete sublattice of $Q(L)$.

PROOF. The property follows easily from 2.2, 2.3 and 2.4.

3. Important sublattices of $Q(L)$

Since adding the unrestricted rule of repetition,

$$rep = \{(\{\alpha\}, \alpha) : \alpha \in For\}$$

to the set of rules of any q-consequence W changes it into the consequence operation, cf. [1]. p. 51, from 2.6 we immediately get

3.1. The class $C(L)$ of all consequence operations on L is a complete sublattice of $Q(L)$.

3.2. The class $Q_S(L)$ of all structural q-consequence operations on L is a complete sublattice of $Q(L)$.

Further to 3.2 and also 3.1 we easily get another characterisation Sup and Inf in $Q(L)$. Namely,

3.3. For every $U \subseteq Q(L)$:

(1) If at least one of $W \in U$ is a consequence operation, then $Sup(U)$ is a consequence operation as well.

(2) If at least one of $W \in U$ is not a consequence operation, then $Inf(U)$ is not a consequence operation.

Using the last corollary we may localize the greatest and the smallest elements of $Q(L)$. Let us consider the following two operations on L:

$$W_S \text{ such that for every } X \subseteq For, W_S(X) = For$$

$$W_\emptyset \text{ such that for every } X \subseteq For, W_\emptyset(X) = \emptyset$$

The former, W_S, is the inconsistent consequence and the biggest element of $Q(L)$. The latter, $W_\emptyset$, is the smallest element of $Q(L)$. To complete the view, let us recall that the identity operator:

$$W_T \text{ such that for every } X \subseteq For, W_T(X) = X$$

the smallest element of $Q_C(L)$

Given a language L, let $Q(\emptyset, T) = \{W \in Q(L) : W_\emptyset < W < W_T\}$, where $<$ means that $\leq$ and $\neq$.

3.4. $Q(\emptyset, T) \geq 2^\omega$.

PROOF. $card(For) = \omega$. For every $Z \in 2^{For}$ let us put:

$$W_Z(X) = \begin{cases} Z & \text{if } X = For \\ \emptyset & \text{if } X \neq For. \end{cases}$$

It is obvious, that every such W_Z is a q-consequence operation in between $W_\emptyset$ and W_T. The aproximation comes, directly from the theory of characteristic functions.

4. Two negative results

Every consequence operation C has the property that all C-indistinquistable formes are C-equivalent. We say that $\alpha, \beta \in For$ are *C-indistinguishable*, $\alpha =_C \beta$, if

$$\alpha =_C \beta \text{ iff } \forall_X C(X, \varphi(\alpha/p)) = C(X, \varphi(\beta/p))$$

and *C-equivalent*, $\alpha \approx_C \beta$, if

$$\alpha \approx_C \text{ iff } \forall_X (\varphi(\alpha/p) \in C(X) \text{ iff } \varphi(\beta/p)) \in C(X)).$$

The inclusion

$$(*) \qquad =_C \subseteq \approx_C .$$

exemplifying the property does not hold generally, i.e. it fails for some $W \in Q(L)$.

For the purpose of discerning between two kinds of operations in $Q(L)$ in [2] the notions of extensional and intensional q-consequence were introduced. Assuming the same definitions for $=_W$, $\approx_W$, we say that a q-consequence is *extensional* provided that

$$(*) \qquad =_W \subseteq \approx_W .$$

Otherwise, W is called intensional.

A natural question obtains whether or not the two classes of q-consequences are sublattices of $Q(L)$. Below, we give two examples showing that the answer in both cases is negative.

EXAMPLE 1. Showing that Inf of two extensional q-consequence operations may be intensional. Given $p, q, r \in Var(L)$ we put

$W_1(\emptyset) = \emptyset$

$W_1(\{p\}) = W_1(\{q\}) = W_1(\{p,q\}) = \{p,q\}$

$W_1(X) = For$, otherwise.

It is easy to verify that W_1 is a consequence operation and, as such, it is an extensional q-consequence operation.

Next, we put

$W_2(\emptyset) = \emptyset$

$W_2(\{r\}) = \{r\}$

$W_2(\{q\}) = \{q\}$

$W_2(\{p\}) = W_2(\{p,q\}) = W_2(\{p,r\}) = W_2(\{q,r\}) =$
$= W_2(\{p,q,r\}) = \{q,r\}$

$W_2(X) = For$, otherwise.

Now, note that

$$p =_{W_2} p, \; q =_{W_2} q, \; r =_{W_2} r$$

and

$$\alpha =_{W_2} \beta$$

for $\alpha, \beta \in For - \{p,q,r\}$. Moreover, each variable p, q, r, is not W_2 equal to other formula different from itself, i.e.

$p \neq_{W_2} \alpha$ for every $\alpha \neq p$

$q \neq_{W_2} \alpha$ for every $\alpha \neq q$

$r \neq_{W_2} \alpha$ for every $\alpha \neq r$.

On the other hand, we have

$$q \approx_{W_2} q, r \approx_{W_2} r$$

and

$$\alpha \approx_{W_2} \beta$$

for $\alpha, \beta \in For - \{q,r\}$. And, further to that,

$q \not\approx_{W_2} \alpha$ for every $\alpha \neq q$

$r \not\approx_{W_2} \alpha$ for every $\alpha \neq r$

One may easily verify that W_2 is an extensional consequence operation, i.e. that W_2 is a q-consequence and that $=_{W_2} \subseteq \approx_{W_2}$.

In the end, let $W = Inf(W_1, W_2)$.

Then, due to ..., we get that

$W(\emptyset) = \emptyset$

$W(\{p\}) = \{q\}$

$W(\{q\}) = \{q\}$

$W(\{r\}) = r$

$W(\{p,q\}) = \{p\}$

$W(\{p,r\}) = W(\{q,r\}) = W(\{p,q,r\}) = \{q,r\}$

$W(X) = For$ otherwise.

One may check that $p =_W q$. However, at the same time $p \notin W(\{p\})$ and $q \in W(\{q\})$. Consequently, $p \not\approx_W p$. This means that W is not extensional.

EXAMPLE 2. (Showing that inf of two intensional q-consequence operations may be intensional). Given $p, q \in Var(L)$, we put

$$W_1(\emptyset) = \emptyset$$
$$W_1(\{p\}) = W_1(\{q\}) = W_1(\{p,q\}) = \{p\}$$
$$W_1(X) = For, \text{ otherwise.}$$

The q-consequence thus defined is intensional since $p =_{W_1} q$, but $p \in W_1(\{p\})$, $q \notin W_1(\{p\})$ and, therefore $p \not\approx_W q$.

Next, we put

$$W_2(\emptyset) = \emptyset$$
$$W_2(\{p\}) = W_2(\{q\}) = W_2(\{p,q\}) = \{q\}$$
$$W_2(X) = For \text{ otherwise.}$$

For this q-consequence we have:

$$p =_{W_2} q, p \notin W_2(\{q\}) \text{ and } q \in W_2(\{p\})$$

and thus, W_2 is also intensional.

Now consider $W = Inf\{W_1, W_2\}$. W is then characterised as below:

$$W(\emptyset) = W(\{p\}) = W(\{q\}) = W(\{p,q\}) = \emptyset$$
$$W(X) = For, \text{ otherwise.}$$

Accordingly, every two formulas α and β of the language are q-equivalent, $\alpha \approx_W \beta$. So, W is an extensional q-consequence operation.

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STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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FORMAL MATHEMATICAL TEXTS. TOWARDS THEIR RENDERING INTO NATURAL LANGUAGE*

1. Introduction

Formal proof systems have long been studied as part of mathematical logic, especially proof checker systems were originally intended for the actual use in carrying out standard mathematical texts. With the advent of powerful and sophisticated implementations of logics, formal reasoning in general, and formal proofs in particular, are becoming relevant and accessible to other fields that use and rely on mathematical reasoning techniques. Though the use of formalisms enables formal proofs to be written by a human and checked with the help of a computer, such articles have one serious deficiency: they are overburdened by large amounts of technical detail of the underlying formal systems. This formal view obscures the basic line of reasoning and hinders human comprehension. There is evidently a wide gap, then, between formal texts and conventional mathematical proofs, whose essential purpose, in addition to establishing the truth of propositions, is to provide insight and understanding. One may argue that it is not worthwhile trying to understand a formal proof at all, once it has been machine-checked for correctness. This is certainly the case where proofs are technical and tedious, and fail to offer any insights, and we would be happy to leave the verification of such arguments to the machine. But in other cases, a formal mathematical text, just as its informal counterpart, carries important information that we would like to communicate.

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Traditionally, research in the area of computer aided formalization of mathematics focussed on how the computer can help in the process of constructing correct, mechanically checked mathematical articles. Consequently, the research concentrated on building a library of articles. The largest, developed since 1989, is the Mizar Mathematical Library [RudTry99]. In the last few years there has been a growing interest in reading and using proof checked articles, by authors of formalized articles and by the wider audience through the internet. Understanding and then practical application of articles depends strongly on their presentation. Language and notation are used to explain the reasoning, intuition, association and stylistical paraphrasing may be used to help the reader. We therefore propose a planning approach to the presentation of Mizar proofs that integrates a formal proof and its explanation into a single document. The support of mathematical notation deserves special attention. Well designed notation, especially in TEX, plays an important role in the communication of mathematical understanding.

In the research we propose an approach to formal text presentation that attempts to combine formality with comprehensibility. This approach is guided by an analogy relating the activities of proving and programming. Although proving in Mizar is a declarative process, programming is a procedural process. Developing a program from a specification is very much like developing a proof for a theorem. By following this analogy, we apply techniques and principles known from program design to proof design and presentation. Most important, we apply the principle of refinement to proofs. Refinement has been used as both an informal and a formal abstraction principle to control complexity and to structure and guide the process of programming. By transferring the refinement paradigm to formal proof design, we will show how we arrive at *formal* and *hierarchically structured* texts, which are presented at different levels of abstraction. The upper levels indicate how complete formal text can be constructed, and they carry the essential information that constitutes the basic line of reasoning. The lower levels fill in the necessary technical detail, a task that can be left entirely up to the machine. Thus, the choice of the most appropriate level of abstraction depends on the difficulty of the proof, and of those for whom the presentation is intended (education, report of the database, journal, etc.). Its depends also on the mechanical capabilities of the underlying reasoning system, which besides proof checking of the formal text must also maintain and verify the database of mathematical knowledge.

In this article we will show, how based on a proof plan, a structured presentation at the level of proof methods can be investigeted. Mainly two

kinds of knowledge are incorporated into the content planning in the form of presentation of proofs. The *hierarchical planning* split the task of presenting a particular proof into subtask of presenting subproofs. A *Local navigation* operators simulates the unplanned aspect, where the next conclusion to be presented is chosen under guidance of a local focus mechanism. From the other side we have to investigate textbooks [BelSlo69, Gra79, HarWri79, KurMos76, Lan80] proofs consists the *structural* and *logical* information, which is contained in the proof. This includes the identification of:

- sequences of sentences that constitute subproofs,
- the contribution of each of the proof segments to the overall goal to decrease the proof obligations,
- the scope and quantification of the variables,
- the logical/structural relations between sentences/segments.

The very important principle in our approach is, that investigated formal mathematical text is logically and mathematically correct (proof checked). It is important to note, that this assumption give guaranty, that text will be coherent. The document containing the formal proofs is the same document containing the presentation of the proofs, guaranteeing that once the document is checked, the presented version is correct.

The readability of a proof depends on the effort required by the reader to understand it. Therefore, in order to be readable, a proof should contain the necessary information to be followed without undue effort. It should also omit irrelevant information, or any information, which can be easily deduced by the intended reader of the proof. Furthermore, in order to facilitate its readability, the information contained in a proof should be organized in a way, which highlights its structure.

Presenting a formal mathematical text may be seen as an attractive idea, particularly since formal arguments tend to be bogged down by technical details. These circumstantiality usually hides the basic line of reasoning underlying a proof, but is necessary to enable a computer to check the correctness of an argument. Strictly formal proofs contain too much technical detail, which is of no interest to the human reader, who only wishes to understand *the basic idea*. This results in a long, overly detailed proof in which the basic line of reasoning is obscured. The representation of a formal text is geared towards a form that is easy to parse for the computer, which differs from the form a human would choose in order to understand it. This results in a lack of structural information. Both above combined result in superfluous information on the one hand and the lack of helpful information on the other hand. But still, such a proof contains a representation of the basic proof idea that was on the mind of the person who has written the *Mizar article*. Thus, by hiding the unnecessary information and by providing additional information it should be possible to recover the proof idea.

On the other hand, the primary goal of our research should be to convince the user of the correctness of an argument and not the machine. The intelligible presentation of formal proofs is usually not attempted because of their technical detail. We want to separate the discussion of the requirements into two areas: what features are necessary to present formal reasoning in a structured and natural language way, and how can the whole system be kept flexible, i.e., applicable for various instances of formal reasoning. We will keep the discussion at a rather abstract level so that the software architecture of the supports system becomes visible.

The mechanical translation of formal proofs into natural language needs several steps. On the *high level planning* will be profiling of the structure of the proof, extracting references and subproofs. On the *low level planning* will be reorganizing of linguistic resources in order to produce connected text. On the *output realizer level* generated text will be enhanced and pruned.

Another question is the graphical representation of the text. The fact, that the formulas are displayed in the severely restricted ASCII character set doesn't add to the comprehensibility either. Thus we take this representation merely as a basis to derive step by step proof document that is independent from the syntax of the system, well structured, and oriented at common proving styles. Afterwards, a TEX, or HTML, document can be generated, where all the operators, constants, and so on are replaced by their appropriate mathematical symbols.

In contrast to the belief that mathematical texts are only schematic and mechanical, *state of the art* techniques of natural language processing are necessary to produce coherent texts that resemble those found in typical mathematical textbooks. The human proof presentation process is based on the natural language generation techniques. On one side we have a formal text as input, and as output we will have a text in natural language. Traditionally, the generation process has been divided into two stages:

- ***what to say***, and
- ***how to say it***.

The first stage comprises of processing from the concept and intermediate representation of the formal proof to the planning of contents. The second covers realization of the plans into text or output in other modalities.

The planning of the contents could be developed through the research of the Discourse Representation Theory [GroSid90]. While the realization of the plans – through the Rhetorical Structure Theory [ManTho87].

2. Current Situation in the Related Research

Over the past thirty years there have been significant achievements in the field of automated theorem proving with respect to the reasoning power of the inference engines. Although some effort has also been made to facilitate more user friendliness of the deduction systems, most of them failed to benefit from more recent developments in the related fields of artificial intelligence, such as natural language generation and user modeling. In particular, no model is available which accounts both for human deductive activities and for human proof presentation. In this thesis, a reconstructive approach is suggested which substantially abstracts, reorganizes and finally translates machine checked proofs into a natural language. Both the procedures and the intermediate representations of our architecture find their basis in the Discourse Representation Theory and in the Rhetorical Structure Theory for informal mathematical reasoning and for proof presentation. User modeling is not incorporated into the current theory, although we plan to do so later.

The need for better outputs of formal texts was recognized some years ago and several attempts have been made to produce proof presentations based on rules of the natural deduction calculus and otherwise readable proofs.

The system EXPOUND [Che76] was pioneering presentation of proofs in natural language, in 1976. It is an example of *direct translation*. Although a sophisticated linearization is applied on the input natural deduction proofs, the steps are then translated locally in a template driven way.

Proof presentation in natural language has recently been realized in ILF [Dah94] and PROVERB [Hua94], which slightly abstract proofs before the presentation:

- ILF provides a schematic verbalization, not natural language generation but merely the application of templates. Each logical rule, as well as each of the various manifestation of reasoning rules, has a template associated with it.
- PROVERB returns a more elaborate proof presentation at the so-called assertion level that employs linguistic knowledge in order to combine single assertion level steps. However a proof verbalization at the assertion level is not necessarily the most natural and best way to communicate a proof to mathematicians or to students. In particular, often the proof is not abstract enough and the user cannot go from an abstract level to a more detailed level because a hierarchical structure is missing.

Last years research was done also by:

- Yann Coscoy [Cos94] – describe how a natural language proof description can be extracted automatically from proof objects, which are in their case – terms of the Calculus of Construction of COQ, encoding a natural deduction proof. They use techniques that are similar to the ones employed to extract programs from proofs. Questions such as hierarchical structuring are not addressed, nor is the treatment of special proof styles. This technique would therefore appear – thus far – to be usable for small proofs only.
- Martin Simons [SimSin97] – literate and structured presentation of formal proofs of Deva language.
- Sh. Kobayashi, Y. Nakamura and Y. Fuwa [KobNakFuw97] – developed automatic translation from Japanese into English, using one intermediary language INE (Internet New Esperanto). This approach was based on translation into Function Format Language (predicate calculus), with help of specialized templates (devised by Writing Aid).

Concerning automatic translation of Mizar texts into natural language, besides research noted in [BanCar93], there was also one attempt to translation of Mizar-MSE texts into Chinese by Bin Qin [Qin84]. Similar approach as in [Mat89] was made by P. Rudnicki and A. Trybulec [RudTry89] – *A Collection of TEX-ed Mizar Abstracts*, but mostly on the typesetting level.

3. Mathematical Vernacular

The term *Mathematical Vernacular* has been used with varying meaning, e.g. in [deB87] – it is a mixture of words and formulas that mathematicians speak and write. It is a language, which is suitable for ordinary mathematical practice, and which can be implemented on the computer under the guidance of formal semantics. But more precisely, we mean that a mathematical and natural language which is suitable for developing mathematics, has formal semantics, and is implementable for interactive mathematical development based on the technology of a computer assisted formal reasoning and natural language processing. In this language logical content is a prominent part of meaning.

Mathematical vernacular is characterized in part by an open system of standardized notation. A writer of mathematics is not free to fill his text with an undisciplined growing of freely invented notation. If a standard notation is adequate for the purpose, the author is well advised to use

it. Only the rarest circumstances permit a relaxation of this practice. Nevertheless, excessive formalism should be avoided since it invites a level of detail simply too distracting, indeed boring, from the main point of the argument. The standardization of notation permits the possibility of a formal reconstruction of mathematical vernacular. The requirement that the mathematical presentation be not too formally detailed, but nevertheless clear in a step wise style, permits the use of some aspects of automated reasoning in a reasoning assistance system. A single human oriented step in a mathematical argument is viewed as a small, quickly solvable, automated reasoning task. The ultimate success and value of such system is determined by how useful a tool it proves to be in practice and not by how well it is alleged to embody a particular teaching style.

The reason is, that mathematicians define the meaning of their words and sentences locally, without having to compare them to the existing habits in the outside world. On the one hand, the grammar of mathematical vernacular is much simpler than the one of natural languages. The rules of mathematical vernacular are expressed in terms of three grammatical categories:

- sentence,
- name,
- substantive.

On the other hand, it is more complex, since the correctness of mathematical statements depends on the context and on everything said before.

The next question is the conceptual structure of mathematical language, which depends on the notion of conceptual category. An important issue in this analysis is that of well-formedness and meaningfulness of expressions in mathematical vernacular. Mathematicians attach importance to the criterion of semantic well-formedness, as well as to grammatical well-formedness. Conceptual categories play an important role not only in the correctness of checking (i.e. deciding whether an expression or a sentence is well-formed and meaningful) but also in capturing the generative nature of conceptual composition in mathematical vernacular. To mechanize any aspect of mathematics, we need a good formal understanding of the language.

As mentioned above, there is quite a lot of interesting and novel problems attached to mathematical vernacular. Given the prime need for correctness in implementing mathematical vernacular, we believe it is necessary to identify its successful parts, together with good practice suggested by our experience of formal mathematics, and fully formalize that, rather than attempt to formalize "all" of mathematics, a concept which we find hard to define precisely. Naturally, the language should be as close as possible to good mathematical language, in the sense of exposing the richness of it, without having too many restrictions. Our view is, that mathematical vernacular should be developed by formalizing the essential *core* of the mathematical language, without which no useful mathematics may be practiced – even if the means of expression are cumbersome, then by extending this core to make the language more flexible without losing the formal properties.

To achieve better 'meta-variable' facilities, we have identified an aspect, which will allow the user to omit parts of his proofs temporarily – such as details he considers trivial. This 'feedback' of ideas can also occur in informal mathematical language: studying it in order to create mathematical vernacular will help us to identify good parts and bad parts, showing a way of improvements in mathematical language. Thus, we can regard development of mathematical vernacular as a constraint satisfaction problem – mathematical vernacular is between mathematics and its realization in today's mathematical language.

Another important question is, whether the current set of vernacular is sufficient to formalize all proofs in a mathematical style. Some existing vernaculars (COQ [Dow90] and LEGO [Luo89, CalLuo98]) have one disadvantage – the inability to express a proof in a traditionally mathematical style. Only Mizar and based on it declarative languages seems to be sufficiently close to today's mathematical language.

4. Formal Proof

In Webster's dictionary, a proof is "the process or an instance of establishing the validity of a statement esp. by derivation from other statements in accordance with principles of reasoning". To put it more succinctly, a proof yields evidence. An informal proof provides readers with sufficient intuitive evidence to convince them of the validity of the statement that is to be proven. Human understanding of why the theorem is true is achieved by explaining the line of reasoning underlying the proof. A formal proof, on the other hand, provides evidence by reducing every aspect of the preceding definition to the level of symbols and their precise formal syntactic manipulation within a well defined and sound logical framework. The sole purpose of a formal proof is to establish the validity of a statement by mechanical – not necessarily automatical – deduction from a given fixed set of axioms, and possibly a set of hypotheses. The process of actually proving

a theorem, establishing the validity of a concrete statement, is insignificant with respect to providing insight. The result of the process is what counts: the truth of the theorem is either established or not. Only at the meta level is the process relevant again: it is formally expressed, proven sound and possibly complete, and it is the object of further study, e.g. of proof theory or research investigating possible automation. By disregarding the specific process during which a proof is successfully produced, and by focusing exclusively on the mechanical establishment of truth, a formal proof ignores – or rather abstracts form – all other aspects. Moreover, formal proofs, which are intended to be checked by a machine, are by their very nature overburdened with so much technical detail that any line of reasoning that might have existed when the proof was originally conceived is completely hidden.

There are two main approaches towards the formalization of proofs to enable automatic verification. One of them are theorem provers – they interactively seek a proof of a certain theorem, at the same time guaranteeing that the constructed proof is correct. The internal result of such proof is not readable for a human. It is like an internal language of programming; very "close" for computer, "far" for humans.

Formal proofs in general, and formal proofs arising during formal system development in particular, tend to be of a very technical and shallow nature. With tedious theorems, we are really only interested in whether they are true or not. We are only too glad to leave their actual proof to a machine. It is clear that a formal proof as such cannot mirror the cognitive and intellectual aspects of a proof and its presentation. A formal proof is a game with symbols, by its very nature devoid of aids to human understanding. If, then, we are to make formal proofs, i.e. proofs that are machine checkable, not only intelligible and manageable, but also ultimately as useful to humans as informal proofs, we must provide mechanisms and facilities that address these human oriented issues.

In [Har97] Harrison describes several different uses of the word "proof" in the field of automated reasoning. Three of these are of interest here:

- a proof as found in a mathematical text book, i.e. a sketch given in a mixture of natural, symbolic and formal languages, sufficient to convince the reader,
- a script to be presented to a machine for checking. This may be just a sketch, or a program, which describes the syntactic manipulations, needed to construct a formal proof,
- a formal "fully expansive" proof in a particular formal system, e.g. a derivation tree of inference rules and axioms.

From the practical point of view we can say, that mathematics is the language of mathematicians, and a proof is a method of communicating a mathematical truth to another person, who also 'speaks' the language. Our general problem is, how to communicate a proof to a machine. We use the word "proof" in the second listed above sense, and "proof outline" to mean proofs (again in the second sense) that are merely sketches, and that require significant reasoning to fill in gaps. Proofs in Mizar are expressed as proof outlines, in a language that approximates written mathematics. Therefore, the user must write a rigorous, i.e., completely formalized proof, that he believes represents the intent of the author of the textbook proof, and use the computer to check this rigorous proof.

Rigorous and formal proofs

The difference between *rigorous* and *formal* proofs is not easy, because the English meaning for both terms refers to the same. We can say, that *rigorous* proof is written in a formal language above inference level, independent of some calculus. This kind of proof interest is not on the individual, step by step, inferences of the proof – the application of individual inference rules – but, rather, on the main ideas of the proof. A rigorous proof relies to some extent on the reader's ability to judge its correctness, and the reader is certain that steps are correct, with no gaps or omissions. Then, a Mizar proof seems to be a rigorous proof.

In metamathematics, rigorous arguments showing how various pieces of rigorous mathematics can be codified in the predicate calculus. Textbooks of axiomatic set theory, which state the axioms of ZFC and then sketch how to introduce various mathematical concepts such as the real number system in (definitional extensions of) ZFC and prove standard theorems about such cocepts, all on the basis of the axioms.

The *formal* proof, in this comparison, is written in a language at inference level and the language depends only on the calculi used. The reader is able to expand a proof into primitive rules and it is constructed in conformance with a set of precisely defined and mechanically checkable rules.

The advantage of a rigorous proof is that a human can concentrate on formulating the important steps of a proof and not be diverted by technical details of how these steps must actually be performed. The notion of rigorous proof is related to the human activity of *planning* when proving a theorem. We will apply this activity in our presentation of the proof, see [Mat99].

Formal proofs vs. informal proofs

Our work in analyzing chosen mathematical textbooks: [BelSlo80], [Gra79], [HarWri79], [KurMos76], [Lan80] and Mizar texts shows, that the major difference between formal and informal proofs is the level of detail between the two. Informal proofs contain gaps in their reasoning that the reader is required to fill in order to understand the proof. The author of an informal proof usually has a specific type of reader in mind, one who has a certain amount of knowledge in a number of mathematical fields, and one who has read and understood the preceding sections of the literature containing the proof. The author can therefore rely on his, usually justified, assumptions about what the intended reader is able to understand when deciding what to include in an informal proof and what can be easily inferred by the reader, and can (or must) therefore be unjustified. For example, if one assumes that some set A is a subset of B, and that some element a is a member of A, then the inference which derives the membership of a in B can usually be omitted if the reader is assumed to be familiar with the notions of set membership and containment. This is dependings, of course, on the power of the proof checker. V. Zammit in [Zam99] proposes to reinforce the proof checker by facts of trivial knowledge (in Mizar we call them – requirements), which increase the speed of checking and which have been derived much earlier in the mechanization. Case studies have shown, that the length of the proofs can be substantially reduced through the use of a much more powerful proof checker.

Besides our approach, there are also results reported in [Zin98]. It is interesting to compare informal proofs taken from the above mentioned textbooks, with formal proofs:

- A formal proof is written in a formal language. An informal proof is written in an informal language, say English, enriched with terms and formulae. The syntactic constructions one encounters in proofs are relatively easy and stylized.
- Finding proofs (informal and formal) is not trivial.
- Machines are good at verifying formal proofs, but (currently) cannot check informal proofs. Humans are good at verifying informal proofs, and (will hopefully forever remain) bad in verifying formal proofs.
- In a formal proof, for each proof step, it is explicitly given which conclusion is derivable by which set of premises and by which inference rule. In an informal proof, many proof steps are omitted or incomplete (incomplete set of premises, and lack of reference to inference rules used).

- In formal proofs there is no ambiguity. In informal proofs there should be no ambiguity, but there is.
- In a formal proof, the theory in which the proof is stated is explicitly stated. The theory, and nothing else, defines the context. In an informal proof, there is no full and explicit theory that one can refer to. The context is to be completed by the proof reader.
- In a formal proof, form matters. In an informal proof, meaning matters.
- Formal proofs are structured (e.g., resolution graphs, natural deduction trees, semantics tableaux). Informal proofs are structured, too (e.g., a proof per induction consists of induction base case, induction hypothesis and induction step, the first and the latter contain subproofs themselves).

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STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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PROBLEMS WITH ω–RULES IN MECHANIZATION OF REASONING. A COMPARISON OF TWO SYSTEMS

1. Motivation

Gödel's First Incompleteness Theorem (Gödel, 1931) showed that in any consistent formal system powerful enough to do a certain sort of arithmetic there will be a true sentence that the system cannot prove. The theorem indicated a certain gap in Hilbert's programme (Hilbert, 1901): truth cannot be achieved by provability; it can be only approximated by syntactic means. How to extend the Hilbert's finitistic point of view and to overcome incompleteness of formal axiomatic systems? Hilbert in 1930 proposed to admit a new inference rule to be able to realize his program (it was a some kind of informal ω–rule). Also Turing in 1939 (Turing, 1939) pursued Church's idea for his Ph. D. thesis, looking to ordinal logic as a way to "escape" Gödel's incompleteness theorem. Now several approaches to the problem in question are known among them admitting the ω–rule, adding new axioms or adding (partial) notions of truth.

In logics of programs (such as dynamic logic (Harel, 1979), Hoare–like logics (Hoare, 1969) or algorithmic logic (Mirkowska, Salwicki, 1987; Mirkowska, 1971, 1981)) by reason of complexity there also do not exist formal finitistic proof systems that are complete in the classical sense. There are two fundamental approaches to the construction of these logics: to limit a class of considered interpretations or to admit some ω–rules. Algorithmic logic is a result of the second approach.

It is obvious that a formal verification of program correctness is a very important task. Consequences of possible errors in programs may prove expensive and sometimes irreversible, particularly nowadays when computers have become indispensable in all domains of our life. But

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the formal proofs of program properties in algorithmic logic (and also in dynamic logic or Hoare–like logics) are often nontrivial and with a large number of tedious technical details. For that reason no wonder that there is a growing need for mechanization of the proof process. The main obstacle in the mechanization of reasoning in algorithmic logic are, first of all, the ω–rules which are not implemented.

In the present paper two formal systems (S_{MIND} and S_{LOOP}) for the logic are compared. Each of them is suitable for computer realization but tries to overcome the problem of ω–rules in the different way: the first of them replaces ω–rules by metainduction; the second of them uses the notion of proof as a finite tree with redundant nodes, i.e. looping nodes satisfying some conditions.

2. Introduction

Both the S_{MIND} system and the S_{LOOP} system are finite cut–free Gentzen–type axiomatizations for propositional algorithmic logic (PAL).

The syntax of PAL is based upon two sets of symbols:

V_0 – an enumerable set of propositional variables,

V_p – an enumerable set of program variables.

From V_0 we construct the set of open formulas F_0 as usual propositional formulas (i.e. $V_0 \in F_0$) and if α, $\beta \in F_0$ then $(\alpha \vee \beta)$, $(\alpha \wedge \beta)$, $\neg\alpha \in F_0$ are propositional formulas.

Given sets F_0 and V_p the set Pr of *programs* is generated by the following grammar:

$$Pr ::= V_p \mid (Pr; Pr) \mid \textbf{if } F_0 \textbf{ then } Pr \textbf{ else } Pr \textbf{ fi} \mid \textbf{while } F_0 \textbf{ do } Pr \textbf{ od}.$$

The set of all *formulas* F is defined as follows

$$F ::= F_0 \mid F \vee F \mid F \wedge F \mid \neg F \mid MF.$$

Let B_0 be two–element Boolean algebra. By a *semantic structure* $\mathfrak{M}$ we shall understand a triple $\langle S, \mathcal{I}, w\rangle$ where S is a nonempty set of states, $\mathcal{I} : V_p \to 2^{S\times S}$ is an interpretation of the program variables (where $\mathcal{I}(Id) = \{(s,s) : s \in S\}$) and $w : S \to B_0^{V_0}$ is a function assigning to every state a valuation of propositional variables. For every program variable K and three states s, s_1, s_2 the following condition (condition of deterministic programs) is satisfied:

$$\text{if } (s, s_1) \in \mathcal{I}(K) \text{ and } (s, s_2) \in \mathcal{I}(K), \text{ then } s_1 = s_2.$$

For a given structure $\mathfrak{M}$ and a given state $s \in S$ the Boolean value of the formula α is denoted by $\alpha_{\mathfrak{A}}(s)$ and is defined for classical connectives as follows:

$$p_{\mathfrak{A}}(s) = w(s)(p) \quad (p \in V_o), \qquad (\neg\alpha)_{\mathfrak{A}}(s) = -\alpha_{\mathfrak{A}}(s),$$
$$(\alpha \vee \beta)_{\mathfrak{A}}(s) = \alpha_{\mathfrak{A}}(s) \cup \beta_{\mathfrak{A}}(s), \qquad (\alpha \wedge \beta)_{\mathfrak{A}}(s) = \alpha_{\mathfrak{A}}(s) \cap \beta_{\mathfrak{A}}(s),$$

Let us denote by $K_{\mathfrak{A}}$ a relation which is assigned to a program variable K by interpretation $\mathcal{I}$ in the semantic structure $\mathfrak{M}=\langle S, \mathcal{I}, w\rangle$. Let s be a state and M, M' be arbitrary programs then

$$Id_{\mathfrak{A}}(s) = s,$$

$K_{\mathfrak{A}}(s) = \{s' : (s, s') \in K_{\mathfrak{A}}\}$ and $K_{\mathfrak{A}}(s)$ is at most an one–element set,

$$(M; M')_{\mathfrak{A}}(s) = \bigcup_{s' \in M_{\mathfrak{M}}(s)} M'_{\mathfrak{A}}(s'),$$

$$\textbf{if } \gamma \textbf{ then } M \textbf{ else } M' \textbf{ fi}_{\mathfrak{A}}(s) = \begin{cases} M_{\mathfrak{A}}(s) & \text{if } (\gamma)_{\mathfrak{A}}(s) = 1, \\ M'_{\mathfrak{A}}(s) & \text{if } (\neg\gamma)_{\mathfrak{A}}(s) = 1, \end{cases}$$

$$\textbf{while } \gamma \textbf{ do } M \textbf{ od}_{\mathfrak{A}}(s) = \bigcup_{i \in N} \{s' \in (\textbf{if } \gamma \textbf{ then } M \textbf{ fi})^i_{\mathfrak{A}}(s) : (\neg\gamma)_{\mathfrak{A}}(s') = 1\}.$$

For an arbitrary state s in the structure $\mathfrak{M}$ we assume that

$$(M\alpha)_{\mathfrak{A}}(s) = 1 \text{ iff there exists the state } s' \in M_{\mathfrak{A}}(s) \text{ such that } (\alpha)_{\mathfrak{A}}(s') = 1.$$

We shall write $\mathfrak{M}, s \models \alpha$ in order to denote that $\alpha_{\mathfrak{A}}(s) = 1$. The formula α is *valid in the structure* $\mathfrak{M}$ ($\mathfrak{M} \models \alpha$) iff for every state $s \in S$ holds $\mathfrak{M}$, $s \models \alpha$. In the rest of the paper $(\alpha \Rightarrow \beta)$ is an abbreviation of the formula $(\neg\alpha \vee \beta)$ and $(\alpha \Leftrightarrow \beta)$ is an abbreviation of the formula $(\alpha \Rightarrow \beta) \wedge (\beta \Rightarrow \alpha)$. Let us denote by **true** the formula $(p \vee \neg p)$ and by **false** the formula $(p \wedge \neg p)$, for a fixed propositional variable p.

3. The S_{MIND} system

The S_{MIND} system correspond to earlier research on prover for algorithmic logic of Zalewska (1996, 2001). In this section we would like to recall some basic notions connected with the system.

The main idea of the S_{MIND} system is as follows: replace ω-rules such as the below one

$$\frac{\{\Gamma, \neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \Delta\}}{\{\Gamma, \Delta, \neg(\textbf{if } \gamma \textbf{ then } M \textbf{ fi})^i(\neg\gamma \wedge \alpha)\}_{i \in N}}$$

by *metainduction*, i.e. by the rule as follows

$$\frac{\langle\{\Gamma, \neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \Delta\},\ \mathcal{A}\rangle}{\langle\{\Gamma, \Delta, \neg\delta\},\ \mathcal{A}\ \rangle\ ;\ \langle\{\Gamma, \Delta, \neg\ \mathrm{IF}(\mathrm{IF}^j\delta)\},\ \mathcal{A} \cup \{\{\Gamma, \Delta, \neg \mathrm{IF}^j\delta\}\}\rangle}$$

where $\delta = \neg\gamma \wedge \alpha$, IF = **if** γ **then** M **fi**, j is a parameter of natural type.

It allows to prove only that the proof exists instead of carrying out full proof for a given formula. The notion of metainduction is formalized in this way that the conclusion and premises of rules are presented as ordered pair of the form $\langle \Pi, \mathcal{A}\rangle$ where Π denotes the main sequent and $\mathcal{A}$ stands for set (maybe empty) of sequents that are called *metainduction assumptions*. The notion of validity of the main sequent Π of the ordered pair $\langle \Pi, \mathcal{A}\rangle$ with respect to $\mathcal{A}$ is defined in the following way: Π *is valid assuming that each sequent from the set $\mathcal{A}$ is also valid.* In our calculus ω-rules are replaced by *metainduction rules.* In consequence the standard notion of sequent and process of inferences is modified. The main sequent Π of the ordered pair $\langle \Pi, \mathcal{A}\rangle$ is said to be

- *indecomposable* iff no rule can be applied to it;
- *fundamental* iff the formulas α and $\neg\alpha$ belongs to the sequent Π;
- *$\mathcal{A}$-provable* iff there exists a sequent $\Sigma \in \mathcal{A}$ such that $\Sigma \subseteq \Pi$ (we will say sometimes that Π is *$\mathcal{A}$-provable with respect to the sequent* Σ);
- A*-*provable* iff there exists a sequent $\Sigma \in \mathcal{A}$ for which at least one formula $\beta \in \Sigma$ is with negation and there exists a program M such that

  $$\forall_{\alpha\in\Sigma}\exists_{\beta\in\Pi}(\beta = \alpha_M)$$

  where $\alpha_M = \pm M\alpha'$ if $\alpha = \pm\alpha'$ and $\pm \in \{\neg, \varepsilon\}$ (we will say sometimes that Π is *$\mathcal{A}^*$-provable with respect to the sequent Σ and the program M*);
- *terminal* iff Π is indecomposable but Π is not fundamental and neither $\mathcal{A}$-provable nor $\mathcal{A}^*$-provable.

A *proof* of the sequent Π is a diagram (diagram is a decomposition tree obtaining by application of decomposition rules to the input formula) of the sequent such that all paths of the diagram are finite and each its leaf is labelled by the ordered pair $\langle \Pi, \mathcal{A}\rangle$ where Π is fundamental or $\mathcal{A}$-provable or $\mathcal{A}^*$-provable.

4. The S_{LOOP} system

The S_{LOOP} system correspond to earlier researches on the finite Gentzen-like axiomatization for PAL of Chlebus (1982) and Walukiewicz (1990). In this section we would like to present the system for some deterministic version of PAL.

The main rules of the S_{LOOP} system (where upper sequents of rules will be called *assumptions*, lower will be called *conclusion* and $\mp \in \{\neg, \epsilon\}$) are as follows:

(1)

$$\frac{\{\Gamma, \neg\neg\alpha, \Delta\}}{\{\Gamma, \alpha, \Delta\}}$$

(2)

$$\frac{\{\Gamma, \mp Id\alpha, \Delta\}}{\{\Gamma, \mp\alpha, \Delta\}}$$

(3)

$$\frac{\{\Gamma, \mp(\alpha\Box\beta), \Delta\}}{\{\Gamma, \mp\alpha, \Delta\}; \{\Gamma, \mp\beta, \Delta\}}$$

where $(\mp, \Box) \in (\epsilon, \wedge), (\neg, \vee)\}$

(4)

$$\frac{\{\Gamma, \mp(\alpha\Box\beta), \Delta\}}{\{\Gamma, \mp\alpha, \mp\beta, \Delta\}}$$

where $(\mp, \Box) \in \{(\epsilon, \vee)(\neg, \wedge)\}$

(5)

$$\frac{\{\Gamma, \mp(M'; M'')\alpha, \Delta\}}{\{\Gamma, \mp M'(M''\alpha), \Delta\}}$$

(6)

$$\frac{\{\Gamma, \mp\textbf{if } \gamma \textbf{ then } \mathrm{M'} \textbf{ else } M'' \textbf{ fi}\alpha, \Delta\}}{\{\Gamma, \mp((\gamma \wedge M'\alpha) \vee (\neg\gamma \wedge M''\alpha)), \Delta\}}$$

(7)

$$\frac{\{\Gamma, \neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \Delta\}}{\{\Gamma, \neg\alpha, \gamma, \Delta\};\ \{\Gamma, \neg\gamma, \neg M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha), \Delta\}}$$

(8)

$$\frac{\{\Gamma, \textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \Delta\}}{\{\Gamma, \alpha, \gamma, \Delta\};\ \{\Gamma, \neg\gamma, M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha), \Delta\}}$$

$$\frac{\{\Gamma, \neg M\alpha, \Delta\}}{\{\{\beta : M\beta \in \Gamma \text{ or } \neg M\beta \in \Gamma\}, \neg\alpha, \{\beta : M\beta \in \Delta \text{ or } \neg M\beta \in \Delta\}\}} \quad (9)$$

Given a sequence of sequents $\{\Gamma_i, \Delta_i\}_{i\in I}$ such that $\{\Gamma_{i+1}, \Delta_{i+1}\}$ is an assumption and $\{\Gamma_i, \Delta_i\}$ a conclusion in some rule of the S_{LOOP} system, we define:

- family of *trace relations* $R_{i,i+j}(i+j \in I, i, j > 0)$ in the following way:
  1. if formula $\alpha \in \Gamma_i$ is not reduced by the rule applied to $\{\Gamma_i, \Delta_i\}$ then $(\alpha, \alpha) \in R_{i,i+1}$;
  2. if formula $K\alpha \in \Gamma_i$ and a rule applied to $\{\Gamma_i, \Delta_i\}$ reduced it to the formula $M\beta$ then $(K\alpha, M\beta) \in R_{i,i+1}$;
  3. otherwise $R_{i,i+j} = R_{i,i+j-1} \circ R_{j-1,j}$ $(j > 1)$;
- *trace* of any formula $\alpha \in \Gamma_i$ to be a sequence $(\alpha_j)_{j\in J}$ such that $\alpha_j \in R_{i+j}$;
- *loop node* as such element $i \in I$ that there exists $j < i, \{\Gamma_i, \Delta_i\} = \{\Gamma_j, \Delta_j\}$ and $R_{j,i} = \emptyset$;
- *redundant node* as such element $i \in I$ that there exists $j_1 < j_2 < i$, such that
  1. $\{\Gamma_i, \Delta_i\} = \{\Gamma_{j_1}, \Delta_{j_1}\} = \{\Gamma_{j_2}, \Delta_{j_2}\}$;
  2. $R_{j_1,i} = R_{j_1,j_2} \neq \emptyset$;
  3. $\{\{\Gamma_k, \Delta_k\} : j_1 < k < i\} \subseteq \{\{\Gamma_k, \Delta_k\} : k < j_1\}$;
  4. there is no loop node between j_1 and i and between j_1 and j_2.

A *proof* of the sequent Π is a diagram (diagram is a decomposition tree obtaining by application of decomposition rules to the input formula) of the sequent such that all paths of the diagram are finite and each its leaf is labelled by the sequent Π_n where n is redundant node or Π_n is axiom (i.e. $\{\alpha, \neg\alpha\} \subseteq \Pi_n$).

EXAMPLE. Let us consider the following formula:

$$\{\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha \rightarrow \textbf{while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}.$$

In the proof process, after the application of rules for logical connectives: $\rightarrow$, we obtain the following sequent:

(*) $\{\neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \textbf{ while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}$.

After the application of the rule (7) to the sequent we obtain two new sequents:

(1) $\{\neg\gamma, \neg M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha), \textbf{ while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}$

(2) $\{\neg\alpha, \gamma, \textbf{ while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}$.

We can apply the rule (8) to the sequent (2). In this way we obtain as follows:

(2.1) $\{\neg\alpha, \neg\gamma, \gamma, \textbf{ while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}$

(2.2) $\{\neg\alpha, \neg\gamma, \gamma, \}$

Both of the above sequents are axioms. Now we can apply the rule (7) to the sequent (1). In this way we have two sequents:

(1.1) $\{\neg\gamma, \neg M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha), \gamma, \neg\gamma\}$

(1.2) $\{\neg\gamma, \neg M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha), M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma))\}$

The sequent (1.1) is an axiom. From (1.2) (after the application of the rule (9)) we obtain the following sequent:

(1.2.1) $\{\neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \textbf{ while } \gamma \textbf{ do } M \textbf{ od}(\neg\gamma)\}$

The sequent (1.2.1) is equal to the sequent (*). It is not a redundant sequent but repeating the proof for this sequent we obtain a redundant one.

5. Comparison of the systems

Both systems are sound, complete and decidable. In the section we prove derivability of the S_{LOOP} rules in the S_{MIND} system. Next we shall discuss the problem of the application of the systems to the first–order algorithmic logic.

Theorem. For every rule r if r is the S_{LOOP} rule then r is derivable in the S_{MIND} system.

Proof. The rule (1)÷(6) of the S_{LOOP} system are the same as the proper rules of the S_{MIND} system.

The rule (7). In order to derive the rule (7) in the S_{MIND} system we have to prove the main sequent of the following ordered pair

$$\langle\{\neg\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha, \},$$
$$\mathcal{A} = \{\{\neg\alpha, \gamma, \}, \{\neg\gamma, \neg M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha)\}\}\rangle.$$

After the application of the rule (1) to the main sequent and to the elements of the set $\mathcal{A}$ we obtain the following sequent:

$$\langle\{\neg\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\},\quad \mathcal{A}=\{\{\neg\alpha,\gamma,\},\{\neg\gamma,\neg M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\}\}\rangle.$$

Now we can apply the metainduction rule (3) to the main sequent. We have two following sequents:

(1) $\langle\{\neg(\neg\gamma\wedge\alpha),\},\quad \mathcal{A}=\{\{\neg\alpha,\gamma,\},\ \{\neg\gamma,\neg M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\}\rangle.$

(2) $\langle\{\neg\textbf{if } \gamma \textbf{ then } M \textbf{ fi}(IF^i(\neg\gamma\wedge\alpha)),\},\ \mathcal{A}'=\mathcal{A}\cup\{\ \{\neg IF^i(\neg\gamma\wedge\alpha)\}\}\rangle.$

The sequent (1), after the application of rules for logical connectives, is $\mathcal{A}$–provable with respect to the assumption $\{\neg\alpha,\gamma\}$:

$$\langle\{\gamma,\neg\alpha),\},\ \mathcal{A}=\{\{\neg\alpha,\gamma,\},\ \{\neg\gamma,\neg M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\}\}\rangle.$$

From the sequent (2), after the application of the rule for the program connective **if – then – fi**, rules for logical connectives and the rule (4), we obtain two sequents:

(2.1) $\langle\{\neg\gamma\ \neg M(IF^i(\neg\gamma\wedge\alpha)),\},\quad \mathcal{A}'=\mathcal{A}\cup\{\{\neg IF^i(\neg\gamma\wedge\alpha)\},\{\neg\gamma,\neg M(IF^i(\neg\gamma\wedge\alpha))\}\}\rangle.$

(2.2) $\langle\{\gamma\ ,\neg IF^i(\neg\gamma\wedge\alpha)),\},\quad \mathcal{A}'=\mathcal{A}\cup\{\{\neg IF^i(\neg\gamma\wedge\alpha)\},\{\neg\gamma,\neg M(IF^i(\neg\gamma\wedge\alpha))\}\}\rangle.$

The first of them is $\mathcal{A}'$–provable with respect to the sequent $\{\neg\gamma,\neg M(IF^i(\neg\gamma\wedge\alpha))\}$. The second of them is $\mathcal{A}'$–provable with respect to the sequent $\{\neg IF^i(\neg\gamma\wedge\alpha)\}$.

The rule (8). In order to derive the rule (8) in the S_{MIND} system we have to prove the main sequent of the following ordered pair

$$\langle\{\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha,\},\quad \mathcal{A}=\{\{\alpha,\gamma\},\ \{\neg\gamma,M(\textbf{while } \gamma \textbf{ do } M \textbf{ od}\alpha)\}\}\rangle.$$

After the application of the rule (1) to the main sequent and to the elements of the set $\mathcal{A}$ we obtain the following sequent:

$$\langle\{\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\},\quad \mathcal{A}=\{\{\alpha,\gamma\},\ \{\neg\gamma,M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ od}(\neg\gamma\wedge\alpha)\}\}\rangle.$$

Now we can apply the rule (2) to the main sequent. We have the following sequent:

$$\langle\{\neg\gamma\wedge\alpha,\textbf{if } \gamma \textbf{ then } M \textbf{ fi}\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha))\},\ \mathcal{A}\}\rangle.$$

From the sequent, after the application of the rule for the program connective **if – then – fi** and rules for logical connectives, we obtain as follows:

(1) $\langle\{\neg\gamma,\gamma,\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle,$

(2) $\langle\{\neg\gamma,M\ \bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle,$

(3) $\langle\{\alpha,\gamma,\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle,$

(4) $\langle\{\alpha,M\ \bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle.$

The sequent (1) is fundamental. The sequent (2) is $\mathcal{A}$–provable with respect to the sequent $\{\neg\gamma,M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\}$. The sequent (3) is $\mathcal{A}$–provable with respect to the sequent $\{\alpha,\gamma\}$. Let us observe that the sequent (4) is equivalent to the following sequent:

$$\langle\{(\gamma\wedge\neg\gamma),\alpha,M\ \bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle.$$

After the application of the rule for logical connective $\wedge$ we obtain two sequent:

$$\langle\{\gamma,\alpha,M\ \bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle,$$

$$\langle\{\neg\gamma,\alpha,M\ \bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha),\neg\gamma\wedge\bigcup \textbf{then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\},\ \mathcal{A}\}\rangle.$$

The first of them is $\mathcal{A}$–provable with respect to the sequent $\{\alpha,\gamma\}$. The second of them is $\mathcal{A}$–provable with respect to the sequent $\{\neg\gamma,M\bigcup \textbf{if } \gamma \textbf{ then } M \textbf{ fi}(\neg\gamma\wedge\alpha)\}$.

The rule (9). In order to derive the rule (8) in the S_{MIND} system we have to prove the main sequent of the following ordered pair

$$\langle\{\Gamma,\neg M\alpha,\Delta\},\ \mathcal{A}=\{\{\{\beta : M\beta\in\Gamma \textit{ or } \neg M\beta\in\Gamma\},\neg\alpha,\{\beta : M\beta\in\Delta \textit{ or } \neg M\beta\in\Delta\}\}\}\rangle.$$

Let us observe that the main sequent is $\mathcal{A}^*$–provable with respect to assumption and the program M. □

Because

any valid formula of propositional algorithmic logic becomes a valid formula of the first-order algorithmic logic following a substitution of formulas for propositional variables and programs for programs variables

we can extend each of the systems presented in the paper to the first–order algorithmic logic (AL). Of course AL is not complete. We can only try to enlarge the class of provable formulas in a given extended system by some modifications and other new rules. In order to do this we have to base on some kinde of "open" system which allows to do such modifications and which allows to extend the basic system by new rules in natural way.

Let us consider the following AL formula:

$$\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\quad \textbf{while } \neg P(x) \textbf{ do } x := f(x) \textbf{ od}P(x)\},\ \emptyset\rangle.$$

The proof in the S_{MIND} system is presented below.

After the application of the rule (1) to the main sequent we obtain the following sequent:

$$\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)\},\ \emptyset\rangle.$$

Now we can apply the rule (2) to the main sequent. We obtain the following one:

$$\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)\},\ \emptyset\rangle.$$

From the sequent, after the application of the rule for the program connective **if** – **then** – **fi** and rules for logical connectives, we have the following sequents (where $\delta = P(x) \wedge \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)$):

(1) $\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ \neg P(x),\ \delta\},\ \emptyset\rangle$

(2) $\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ (x := f(x)) \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x), \delta\},\ \emptyset\rangle.$

The sequent (1) is fundamental. Now we can apply to the sequent (2) again the rule (2):

$$\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ (x := f(x))P(x),\ (x := f(x))\textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x), \delta\},\ \emptyset\rangle.$$

This sequent is also fundamental (after the application of the rule (5)):

$$\langle\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ P(f(x)),\ (x := f(x))\textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x), \delta\},\ \emptyset\rangle.$$

Now we shall try to prove the same formula in the S_{LOOP} system.

After the application of the rule (8) we obtain the following two sequents:

$$\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x)\ \neg P(x)\},$$

$$\{\neg(x := f(x))Q(x),\ \neg P(f(x)),\ P(x),\ (x := f(x) \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)\}.$$

The first sequent is an axiom. Now we can apply the rule (9) to the second sequent for $M = (x := f(x))$:

$$\{\neg Q(x), \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)\}.$$

Let us observe that the formula $\neg P(f(x))$ has been removed from our sequent. Of course we can again apply the rule (8) obtaining in that way two sequents:

$$\{\neg Q(x),\ P(x),\ \neg P(x)\},$$

$$\{\neg Q(x), P(x),\ (x := f(x)) \bigcup \textbf{if } \neg P(x) \textbf{ then } x := f(x) \textbf{ fi} P(x)\}.$$

The first sequent is fundamental but the second of them is not provable in the S_{LOOP} system.

Let us recapitulate the comparison of the systems. Let C_{LOOP} be the class of provable formulas in the S_{LOOP} system, C_{MIND} be the class of provable formulas in the S_{MIND} system, $\overline{C_{LOOP}}$ be the class of provable formulas in the S_{LOOP} system extended to AL and $\overline{C_{MIND}}$ be the class of provable formulas in the S_{MIND} system extended to AL. The following relations describe the dependences between these classes:

1. $C_{LOOP} = C_{MIND}$
2. $\overline{C_{LOOP}} \subset \overline{C_{MIND}}$.

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Appendix

Decomposition rules for S_{MIND} system

We present only rules which are used (in the proof) in the present paper and are not the same as the rules of the S_{LOOP} system.

Let Γ denotes a set of indecomposable formulas; Δ is an arbitrary set of formulas; α, β are arbitrary formulas; γ denotes a propositional formula; j is a parameter of natural type; M, M', M'' denote arbitrary programs; $\circ \in \{-,+\}$; $\mp \in \{\neg, \epsilon\}$; $\square \in \{\wedge, \vee\}$; $Q \in \{\bigcup, \bigcap\}$; *pref*° is a sequence of simple programs.

(1)

$$\frac{\{\Gamma, pref^\circ \mp \textbf{while } \gamma \textbf{ do } M \textbf{ od} \alpha, \Delta\}}{\{\Gamma, pref^\circ \mp \bigcup \textbf{ if } \gamma \textbf{ then } M \textbf{ fi } (\neg\gamma \wedge \alpha), \Delta\}}$$

(2)

$$\frac{\langle\{\Gamma, pref^\circ \mp Q M \alpha, \Delta\}, \mathcal{A}\rangle}{\langle\{\Gamma, pref^\circ \mp \alpha, \Delta, pref^\circ \mp M Q M \alpha\}, \mathcal{A}\rangle}$$

where $(\circ, \mp, Q) \in \{(+,\epsilon,\bigcup), (-,\epsilon,\bigcap), (-,\neg,\bigcup), (+,\neg,\bigcap)\}$

(3)

$$\frac{\langle\{\Gamma, pref^\circ \mp Q M \alpha, \Delta\}, \mathcal{A}\rangle}{\langle\{\Gamma, pref^\circ \mp \alpha, \Delta\}, \mathcal{A}>; <\{\Gamma, \Delta, pref^\circ \mp M(IdM^j \alpha)\}, \mathcal{A} \cup \{\{\Gamma, \Delta, pref^\circ \mp IdM^j \alpha\}\}\rangle}$$

where $(\circ, \mp, Q) \in \{(-,\epsilon,\bigcup), (+,\epsilon,\bigcap), (+,\neg,\bigcup), (-,\neg,\bigcap)\}$

(4)

$$\frac{\langle\Pi, \mathcal{A} = \mathcal{A}' \cup \{\{\Gamma', pref^\circ \mp Q M \alpha, \Delta'\}\}\rangle}{\langle\Pi, \mathcal{A} \cup \{\{\Gamma', pref^\circ \mp \alpha, \Delta'\}, \{\Gamma', pref^\circ \mp IdM^j \alpha, \Delta'\}\}\rangle}$$

where $(\circ, \mp, Q) \in \{(-,\epsilon,\bigcup), (+,\epsilon,\bigcap), (+,\neg,\bigcup), (-,\neg,\bigcap)\}$ and j occurs in Π

(5)

$$\frac{\langle\{\Gamma, pref^\circ \mp s\gamma, \Delta\}, \mathcal{A}\rangle}{\langle\{pref^\circ \mp s\gamma, \Gamma, pref^\circ \mp \overline{s\gamma}, \Delta\}, \mathcal{A}\rangle}$$

where $\overline{s\gamma}$ is the execution of the substitution s in the formula γ

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TENSE LOGICS AND THE THESIS OF DETERMINISM

The **logical determinism**[1] is a point of view, that for prove the thesis of determinism only logical principles are sufficient. Logical determinists says, that the principle of bivalency and the excluded middle low are sufficient – without adduction on other principles – for construction of proof of an argument on determinism. Arguments on determinism were considered in antiquities already. The problem was clearly formulated by Aristotle in IX chapter of *Hermenetutica.* Aristotle assumes that sentences on the past and the presence are true or false. In his opinion, the assumption that sentences on the future are true or false is sufficient for a construction of an argument of determinism. If all sentences on the future are true or false, then events described by these sentences are determined. If all future events are determined, then there are not accidental events and everything is necessary. Therefore, the thesis, that sentences on the future events are true or false implies, that – apart from the past and the presence – the future is also logical determined.

Recently, the problem of the logical determinism was considered by Jan Łukasiewicz[2]. In the article *On determinism* he gives the following interpretation of determinism:

> If an object A has a property b in a some moment of time t, then in every moment earlier than t, it is true, that the object A has the property b in the moment t.

[1] Apart from the logical determinism the physical determinism is considered. The **physical determinism** is a point of view, that every fact has immemorial causes in other earlier facts. The physical determinism is connected with the principle of causality.

[2] J. Łukasiewicz, *On determinism*, Selected Works, edited by L. Borkowski, Warszawa, 1970.

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It is, so called, the principle of determinism formulated semantically. Zbigniew Jordan wrote, that this principle not run on events, but it run on sentences which describes events. Jordan also wrote, that this principle says about particular properties of true sentences. In accordance with this principle predicative word "true" is an absolute predicative word. If a sentence is true, then it is true irrespective of a person, a place and a time in which says about it, it is true. True sentences are atemporal, but events described by these sentences have temporal component.

Constructions of proofs of arguments on determinism from the principle of bivalency and the excluded middle law were considered in [10]. However, the argument on determinism may be reconstructed from the principle of causality, too.

The principle of causality

Each event Z in a moment t has a cause in a some event Z_1 in a moment t_1, earlier than t. In each moment later than t_1 and earlier than t, there is an event, which is a consequence of Z_1 and a cause of Z.

Therefore, if there is a cause of some event, then it is inescapable. But actual inescapability of some event, does not mean, that actual exists a cause of it. For example, the event *a man X will be dead* is inescapable, but perhaps not exists immediate cause of the man X death.

Some events creates causal-effect association with other events in a set of events. Usually, we assume, that this association is transitive. It means, if an event Z_1 is a cause of an event Z_2 and the event Z_2 is a cause of an event Z_3, then Z_1 is a cause of the event Z_3. Therefore, events Z_1, Z_2, Z_3 are creating a causal-effect chain. In a set of events, ordered with causal-effect association, every event has a cause in other events, which are preceding it in a causal-effects chain. Then, causal-effects chains are infinite. The infinity of causal-effect chains is not adequate condition, that each event has immemorial causes (is determined). Causal-effects chains may be infinite and limited in time. A necessary condition of infinity and limitation of causal-effect chains is density of causal-effect association. Each event is located in a some moment of time. The principle of causality postulates the existence of cause for each event. Therefore, each event in a causal-effect chain is located in a moment of time different from remaining moments of time. If we assume, that time is a measure of change (from cause to effect), then necessary condition for existence of infinite and limited in time causal-effect chains is density of time. Them, if we assume density of time, the principle of causality not implies of the thesis of determinism.

It was proved by Jan Łukasiewicz. We usually assume, that the physics time is continuous. This assumption implies that time is dense. The thesis of determinism is not a result of the principle of causality if we accept properties of time usually assumed. For prove, that the thesis of determinism is the result of the principle of causality, the assumption *time is discreet* is necessary. If we assume, that time is discreet, then causal-effect chains are infinite in the past. It is interpreted, that there are immemorial causes for each event. If each event has immemorial causes, then all events are determined.

Determination should be considered in a temporal context. It is realized in a some systems of tense logic.

The precursor of tense logic was A. N. Prior. One of the basic Prior conception was a temporal interpretation of modal operators. The tense operators are interpreted in the usual way:

F – at least once in the future,
G – it is always going to be the case,
P – at least once in the past,
H – it has always been the case.

The basic deduction system of tense logic is K_t[3]. K_t is the system of tense logic based on classical logic. It is a minimal tense logic. Formally it means, that ever other system of tense logic (based on classical propositional logic) is richer than K_t. Semantical considerations in K_t are based on point structure of time[4].

The system K_t was intended as a formal system coding aid of operators G, H, F, P reasonings about world taking temporal aspects of world into consideration There are no assumes for time structure in K_t semantics.

However, we usually assume, that the real time is linear, continuous, non-ending and non-beginning. If we accept such structure of time, a lot of philosophical and physics considerations are simpler. Of course, not all considerations are more simple. Some considerations are more complicated. For example the rejection of arguments on determinism. Some sentences, which not express of determinism in minimal tense logic, express

[3] This is equivalent of minimal deduction system K for modal logics. See. J. F. A. K. van Benthem, *The Logic of Time*, D. Reidel Publishing Company, Dordrecht, Holland, 1983.

[4] There are tense logic systems, such that semantical considerations are based on period time structure. (See. J. F. A. K. van Benthem, *The Logic of Time*, D. Reidel Publishing Company, Dordrecht, Holland, 1983, p. 193-218.)

determinism in tense logic of non-ending linear time. For example the sentence:

$$\alpha \Rightarrow HF\alpha.$$

According to understanding tense operators accepted in K_t (and K_t extensions which semantics are based on linear time) the sentence $\alpha \Rightarrow HF\alpha$ is reading as:

If it is true, that α, then it is true, that it has always been the case, that at least once in the future α.

If we assume, that time is linear, then if at present there is event Z, which is a semantical correlate of sentence α, then it has always been, that the event Z will be in the future. Since, in every past moment of time it was known that the event Z will be in the future, then we can to say, that the event Z is determined.

If a tense logic of non-ending linear time is based on classical logic, then

$$F\alpha \vee F{\sim}\alpha$$

is a tautology of this logic.

According to understanding of F tense operator, $F\alpha \vee F{\sim}\alpha$ we read as follows:

At least once in the future α or at least once in the future ${\sim}\alpha$.

Since, for any α, the sentence $F\alpha \vee F{\sim}\alpha$ is a tautology of this logic, then in a language of tense logic of non-ending linear time (based on classical logic) $F\alpha \vee F{\sim}\alpha$ express the thesis of determinism. Its consequence is a statement, that all future events are determined and there are not future accidental events. By means of a language of the tense logic of non-ending linear time is possible a description of only such that world, where future is determined.

Indeterminists usually assumes, that past is determined, but future – not. We should to consider a difference between the past and the future. One of arguments to accentuate of this difference is McTaggart paradox on unreality of time. This paradox is connected with two ways of understanding of time.

From one hand, time is understood as a dynamic process. Events are arranging itself according to distinguish between past, presence and future. This is the dynamic conception of time, called A-theory.

In other, these same events, temporal characteristics of which are changing with reference to the past, the presence and the future, are

arranged in order given by an earlier-later relation. This is the static conception of time, called B-theory. According to the B-theory of time, temporal characteristic of all events exist as data.

It seems, that the A-theory of time should be derived from the B-theory, and the B-theory should be derived from the A-theory. However, apparently the static conception of time is derived from the dynamic conception, but not inversely. Some philosophers says, that this is an argument, that the dynamic conception of time is contradictory and time is unreal and illusory. Followers the dynamic conception of time says, that there are ontological differences between past and future events. Past events already have been real events, whereas future events are only possible. The difference can be explained by – for example – logical asymmetry between the past and the future. All expressions on past events are true or false, whereas some expressions on future events are neither true nor false. We can to say, that each past event is determined, but we can not to say, that determined are all future events.

In a certain state of development of world the past may be unknown, but events, which has been, are not changing. The past of world is unique. However, future development of the world is indetermined and may be a lot of way to occur of it.

This point of view was a base to considerations on branching time structure.

The idea of a tense logic of branching time was given by A. N. Prior[5]. One of the main motivation to construction of the tense logic of branching time was attempt at doing an indeterministic tense logic. In the tense logic of branching time, arguments on determinism are rejected by modification of structure of time. If we consider a branching time structure, then apart from moments of linear time, we have to consider moments of time where time is branching. In these moments there are alternative possibilities of realization of the world. Since, realization of the world taking place according to one of the alternative, then if we do something, then it may be to cause, that future will be realized according to another branch of branching time structure.

In the branching time structure the past has no alternatives, however there are a lot of way of a realization of the future. Alternative possibilities of a realization of the future are called **possible futures**. However, usually we assume, that among all possible futures only one is realized. It is, so called, **actual future**.

[5] A. N. Prior, *Past, Present and Future,* Oxford University Press, 1967.

There are logicians, which do not agree with an opinion, that tense logic of branching time is an indeterministic tense logic. P. Yourgran, for example, says, that regardless of which branch is actual future, events occurs in moments of time such that, these moments creates linear time structure[6]. If we choose actual future, then we have only one branch and real time structure is reduced to linear structure. However, if structure of real time is reduced to linear structure, then all events are determined[7].

It seems, that modalization of temporal operators is a solution of reduction of branching time to linear time. Tense logic system with modal--tense operators was given by R. P. McArthur[8]. There are no "actual futures" in a tense logic system with tense-modal operators. All branch of branching time structure McArthur calls **accessible future**. McArthur gives the following questions: how we can to distinguish "tomorrow will be...", "tomorrow should be...", "it is possible, that tomorrow will be...". In models based on linear time all sentences are equivalent. However, in models based on branching time it does not hold. In these models there is hidden modalization of future tense operators.

McArthur introduces the following modal-tense operators: $F^{\diamond}$, $F^{\square}$, $G^{\diamond}$, $G^{\square}$. Operators $F^{\diamond}$, $F^{\square}$ are interpreted as follows:

$F^{\diamond}$ – it is possible, that at least once in the future...,

$F^{\square}$ – it is necessary, that at least once in the future...

Aid of operators $F^{\diamond}$, $F^{\square}$ are defined the operators $G^{\diamond}$ and $G^{\square}$:

$$G^{\diamond}\alpha \equiv \sim F^{\square} \sim \alpha,$$

$$G^{\square}\alpha \equiv \sim F^{\diamond} \sim \alpha.$$

Axiomatization of minimal tense logic[9] with modal-tense operators was given by W. A. Smirnow[10]. However, if we would like to reject of arguments on determinism, we need semantics based on branching time. Branching time tense logic with operators $F^{\diamond}$, $F^{\square}$, $G^{\diamond}$, $G^{\square}$, H, P (operators H and P are interpreted as usually) was created by J. Burgess[11] and called $K_b^{\square}$.

[6] P. Yourgran, *On the logic of indeterministic time*, The Journal of Philosophy, Vol. 82, 1985, p. 548-559.

[7] A. Karpienko, *Fatalizm i słuczajność buduszczego*, Moskwa, 1990.

[8] R. P. McArtur, *Factuality and modality in the future tense*, Nous Vol. 8, 1974, p. 283-288.

[9] The are no conditions upon structure of time.

[10] W. A. Smirnow, *Logiczeskije sistiemy z modalnymi vriemiennymi operatorami*, Modalnyje i vriemiennyje logiki, Moskwa 1979, p. 89-98.

[11] J. P. Burgess, *Decidability for branching time*, Studia Logica, Vol. 39, 1980, p. 203-218.

The sentence

$$\alpha \Rightarrow HF^{\diamond}\alpha$$

is a tautology of system $K_b^{\square}$. But it does not express of determinism. We read this sentence as follows:

If it is true, that α, then it is true, that always has been, that α is possible in the future.

Then, in conclusion, α is only possible in the future, but it does not necessary.

In the $K_b^{\square}$, the sentence $\alpha \Rightarrow HF^{\square}\alpha$ express determinism. We read the sentence $\alpha \Rightarrow HF^{\square}\alpha$ as follows:

If it is true, that α, then it is true, that always has been, that α is necessary in the future.

However, the sentence

$$\alpha \Rightarrow HF^{\square}\alpha$$

is not a tautology of $K_b^{\square}$. Because the thesis of determinism is not a tautology of $K_b^{\square}$, we can to say, that the logic $K_b^{\square}$ is an indeterministic tense logic.

In the literature is considered the problem of construction of many--valued tense logic was. It is connected with possibility of the rejection of arguments of determinism from the principle of bivalency. Many-valued tense logic were crated for example by A. N. Prior[12], N. Rescher and A. Urquhart[13], K. Trzęsicki[14]. However some sentences, which express determinism (for example $\alpha \Rightarrow PF\alpha$, $\alpha \Rightarrow H \sim G \sim \alpha$), are tautologies of these logics.

There is a question: is possible a construction of an indeterministic tense logic satisfying the following conditions:

- there are two logical values,
- there are no condition upon structure of time,
- there are no specific operators apart from tense operators.

It seems, that all conditions are fulfilled by an intuitionistic tense logic. In the system of tense logic based on intuitionistic propositional logic

[12] Por. A. N. Prior, *Time and Modality*, Calendron Press, Oxford 1957.

[13] N. Rescher, A. Urquhart, *Temporal Logic*, Wien New York 1971, p. 219-224.

[14] K. Trzęsicki, *Logika operatorów czasów gramatycznych a problem determinizmu*, Białystok 1986, p. 298-328.

is rejected the excluded middle law. Then, in the intuitionistic tense logic, we can not to reconstruct of argument on determinism based on the excluded middle law. Intuitionistic tense logic systems were considered in a literature[15].

The sentence

$$\alpha \Rightarrow HF\alpha$$

is a tautology of intuitionistic tense logic. However, the meaning of tense operators is such that, the $\alpha \Rightarrow HF\alpha$ does not express of determinism.

Moreover, the sentence $F\alpha \vee F{\sim}\alpha$ (which express determinism) is not a tautology of intuitionistic tense logic.

And finally, the thesis of determinism is not a tautology of intuitionistic tense logic, even if we assume, that time is linear and non-ending[16].

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LEGAL REASONING AND LOGIC

In this paper I discuss relations between rules of legal reasoning and formal logic. I state that some rules of legal reasoning can be arranged as formal systems. To prove my thesis I construct a formal system of the kind in question.

1. In the process of legal argumentation two kinds of rules of reasoning are used. The rules of the first kind are the well-known rules of classical logic. The rules of the second kind are usually called "the rules of legal reasoning".

2. The rules of legal reasoning can be divided into five groups:

a) the rules of the first group (so called "rules of interpretation") are used to reconstruct the meaning of legal expressions; the famous rule *clara non sunt interpretanda* is of this kind,

b) the rules of the second group (so called "rules of inference") are used to infer consequences from legal norms; the rules of reasoning: *per analogiam (a simili), a contrario, a fortiori (a maiori ad minus, a minori ad maius)* are of this kind,

c) the rules of the third group (so called "rules of collision") are used to solve collisions of legal norms; the rule *lex posterior derogat legi priori* is of this kind,

d) the rules of the fourth group are used to determine factual circumstances; the rule *in dubio pro reo (in dubio pro libertate)* is of this kind,

e) the rules of the fifth group are the rules of procedure; the rule that a judge should consider arguments of both parties is of this kind.

3. The system of rules of legal reasoning is called "legal logic". How can we define the relation between legal logic and formal logic? Chaim Perelman opposes legal logic to formal logic in two ways. First, he maintains that legal logic is a heuristic logic, whereas formal logic is just the logic of justification. Second, he maintains that legal logic is possible only as "material logic",

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"nonformal logic". This peculiarity of legal logic is connected – according to Perelman – with the fact that many rules of legal reasoning depend on subjective valuations.

4. *Ad hoc* I can agree that formal logic is not a heuristic logic. In fact, it doesn't offer us rules effective in **all** cases of legal argumentation. However, I admit that in **some** cases rules of formal logic could be effective (for example, it seems that rules of inductive logic are used in legal argumentation as a heuristic method).

5. It is reasonable to assume that legal logic is a heuristic logic. Having this assumption we can consider legal logic as a part of methodology of law: the part which deals with problems such as which legal norm should we use in legal argumentation and how should we use it? Legal logic helps us with finding the solution of a legal problem whereas formal logic (which includes both kind of rules: deductive and inductive) helps us with justification of this solution.

6. However, I can not agree that legal logic is **necessarily** nonformal. Indeed, many rules of legal logic are based on subjective valuations. Quite often this fact makes it difficult or even impossible to formalise such rules. For example, I don't know how we can formalise the rule *clara non sunt interpretanda*. However, on the other hand, many rules of legal reasoning can be formalised quite easily. For example, I formalise the rule of reasoning *a contrario* in the following way:

$$\frac{(x)\{P(x) \Rightarrow Q(x)\}}{(x)\{-P(x) \Rightarrow -Q(x)\}}$$

Moreover, sometimes it is possible not only to formalise a single rule, but also to build a formal system of rules of legal reasoning.

7. Let us consider the following rules of legal reasoning (these rules are called "rules of collision"):

- *lex posterior derogat legi priori* (later norms suppress earlier norms),
- *lex superior derogat legi inferiori* (superior norms suppress inferior norms),
- *lex specialis derogat legi generali* (particular norms suppress general norms),
- *lex superior prior derogat legi inferiori posteriori* (earlier superior norms suppress later inferior norms),

- *lex superior generalis derogat legi inferiori speciali* (superior general norms suppress inferior particular norms),
- *lex prior specialis derogat legi posteriori generali* (earlier particular norms suppress later general norms).

The first three of these rules are called "the first order rules of collision". The last three are called "the second order rules of collision". Whenever the use of first order rules leads us to a contradiction, we employ a second order rule. Respectively, a third order rule of collision would be defined and employed in the case of contradiction between second order rules.

8. Let us build a formal system for the above relations. We add some two-place predicates to the vocabulary of a system of predicate logic: $ESup(\ldots,\ldots)$, $ESpec(\ldots,\ldots)$, $EPost(\ldots,\ldots)$, $Sup(\ldots,\ldots)$, $Spec(\ldots,\ldots)$, $Post(\ldots,\ldots)$, $Der(\ldots,\ldots)$. The definitions: of term, of atomic formula, of formula and the definition of sentence are standard. The axioms of the system are: all sentences of the language of the system which are constructed according to the schemas of valid formulas of predicate logic and some axioms which describe the properties of $ESup(\ldots,\ldots)$, $ESpec(\ldots,\ldots)$, $EPost(\ldots,\ldots)$, $Sup(\ldots,\ldots)$, $Spec(\ldots,\ldots)$, $Post(\ldots,\ldots)$:

AXIOM 1 $(x)ESup(x,x)$,

AXIOM 2 $(x)(y)\{ESup(x,y) \Rightarrow ESup(y,x)\}$,

AXIOM 3 $(x)(y)(z)\{ESup(x,y) \,\&\, ESup(y,z) \Rightarrow ESup(x,z)\}$,

AXIOM 4 $(x)Sup(x,x)$,

AXIOM 5 $(x)(y)\{Sup(x,y) \,\&\, -ESup(x,y) \Rightarrow -Sup(y,x)\}$,

AXIOM 6 $(x)(y)(z)\{Sup(x,y) \,\&\, Sup(y,z) \Rightarrow Sup(x,z)\}$,

AXIOM 7 $(x)(y)\{-Sup(x,y) \Rightarrow Sup(y,x)\}$,

AXIOM 8 $(x)ESpec(x,x)$,

AXIOM 9 $(x)(y)\{ESpec(x,y) \Rightarrow ESpec(y,x)\}$,

AXIOM 10 $(x)(y)(z)\{ESpec(x,y) \,\&\, ESpec(y,z) \Rightarrow ESpec(x,z)\}$,

AXIOM 11 $(x)Spec(x,x)$,

AXIOM 12 $(x)(y)\{Spec(x,y) \,\&\, -ESpec(x,y) \Rightarrow -Spec(y,x)\}$,

AXIOM 13 $(x)(y)(z)\{Spec(x,y) \,\&\, Spec(y,z) \Rightarrow Spec(x,z)\}$,

AXIOM 14 $(x)(y)\{-Spec(x,y) \Rightarrow Spec(y,x)\}$,

AXIOM 15 $(x)EPost(x,x)$,

AXIOM 16 $(x)(y)\{EPost(x,y) \Rightarrow EPost(y,x)\}$,

AXIOM 17 $(x)(y)(z)\{EPost(x,y) \,\&\, EPost(y,z) \Rightarrow EPost(x,z)\}$,

AXIOM 18 $(x)Post(x,x)$,
AXIOM 19 $(x)(y)\{Post(x,y) \& -EPost(x,y) \Rightarrow -Post(y,x)\}$,
AXIOM 20 $(x)(y)(z)\{Post(x,y) \& Post(y,z) \Rightarrow Post(x,z)\}$,
AXIOM 21 $(x)(y)\{-Post(x,y) \Rightarrow Post(y,x)\}$.

The only rules of the system are the rules of predicate logic and the following rules describing the properties of $Der(\ldots,\ldots)$:

RULE 1 $$\frac{\begin{array}{c}Sup(x,y)\\ -ESup(x,y)\end{array}}{Der(x,y)}$$

RULE 2 $$\frac{\begin{array}{c}ESup(x,y)\\ Spec(x,y)\\ -ESpec(x,y)\end{array}}{Der(x,y)}$$

RULE 3 $$\frac{\begin{array}{c}ESup(x,y)\\ ESpec(x,y)\\ Post(x,y)\\ -EPost(x,y)\end{array}}{Der(x,y)}$$

9. According to the axioms: AXIOM 1 – AXIOM 21, the predicates: $ESup(\ldots,\ldots)$, $ESpec(\ldots,\ldots)$, $EPost(\ldots,\ldots)$ denote some equivalence relations and the predicates: $Sup(\ldots,\ldots)$, $Spec(\ldots,\ldots)$, $Post(\ldots,\ldots)$ denote some linear order relations. According to intuition, the above relations order the set of legal norms. So, we read: $ESup(\ldots,\ldots)$ – "the norm … is neither superior nor inferior in relation to the norm …", $ESpec(\ldots,\ldots)$ – "the norm … is neither general nor particular in relation to the norm …", $EPost(\ldots,\ldots)$ – "the norm … is neither later nor earlier in relation to the norm …", $Sup(\ldots,\ldots)$ – "the norm … is not an inferior norm in relation to the norm …", $Spec(\ldots,\ldots)$ – "the norm … is not a general norm in relation to the norm …", $Post(\ldots,\ldots)$ – "the norm … is not an earlier norm in relation to the norm …".

10. The rules for $Der(\ldots,\ldots)$ can be called: RULE 1 – "the rule of derogation of inferior norms", RULE 2 – "the rule of derogation of general norms", RULE 3 – "the rule of derogation of earlier norms". These rules describe the order of derogation defined by the rules of legal reasoning introduced in the point 7 – so called "rules of collision". The construction of the rules: RULE 1 – RULE 3 determines their "hierarchy": RULE 1 is the strongest rule (in the sense that one needs only two premises to use this rule) and RULE 3 is the weakest rule. So, in the above system the second order rules of collision are needless.

11. The above system is a formal system as well as a system of legal logic. So, legal logic is not necessarily nonformal.

Quod erat demonstrandum

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DO WE THINK ALGORITHMICALLY?

The title of the paper may suggest my approach towards the basis of recognition mechanisms, providing some arguments supporting the algorithm of thinking process thesis, or refuting it. The problem occurs, because radical decisions are currently under examination concerning recognition functions of the brain/mind, and the scientists studying this issue bring different, often in contrary to each other, theories and hypothesis. Those thesis are based on empirical material, but collected facts are the elements of some model, which shape depends on philosophical assumptions.

That how it is, as in an example of mind functions hypothesis presented by the American neurologist Antonio Damasio, which assumed close correlation between the body and the mind, and its philosophical base is determined by the negation of the Cartesian dualism. In a latest published book entitled "Decartes' Error...", Damasio shows the polemics with the basic assumptions of this kind of rationalism, which exists in the ideas of the West (Damasio 1999).

Damasio writes, that the mind finds its foundation on the determined brain systems, that is why there must be both: functional and anatomical connections between the mind, the feelings and the body. Therefore, we are absorbed by the passion of reasoning, and the impulse produced in the core of the brain, penetrates into other elements of the nervous system and occurs as feelings, or unconscious mechanisms directing the decision making process. The mind – from the practical one to the theoretical one – probably is based on the control of this inborn impulse. When the impulse disappears, you will never achieve mastery. But just the fact of possessing it, does not make a master out of you.

Damasio concentrates mainly on the emotions, questioning the dogma of the contrary between the emotions and the rational decision taking. He also determines the right function of feelings in the human acting, but he

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analyzes as well this part of thinking processes, that we call reasoning. The purpose of the reasoning lies in taking decisions, and the essence of the taken ones states our choice reaction: non-verbal acting, words, sentences, or some combination of those elements, that belongs to a whole collection of the reactions in a given moments. Reasoning and deciding require from a resolving person a certain knowledge a) about the situation, which exacts taking a resolution, b) about different chances of reacting and about the consequences of choosing any of the possible options, according to the close and distant future. Therefore, reasoning and deciding necessitate possession of some strategy by a unit, based on the rules of logic (Damasio 1999, 191).

Damasio discusses the model of the decision making process based on the traditional mind fundament. Rational reasoning cannot be distracted by the passion. According to this approach human gathers various scenarios of the progress of the events and uses the right strategies, making the analysis of the profit and the loss. Taking into consideration "subjectively implied usefulness", which he desires to maximize, he concludes logically, what is good or bad for him. The main part of this calculation is based on the creation of the other possible scenarios of the event development, founded, among other things, on the visual and aural pictures, as well as forming of the verbal narration, necessary to keep the process of the logic reasoning. Damasio states, that if this strategy was the only one, rationality in the given sense would not exist. In the best option, the process of making the decision would have taken extremely lot of time. As a barrier against the effectiveness of this model, there is also a limitation of the attention and the memory, as well as, suggested by the imperfection of our reasoning strategies, unawareness or wrong use of the probability and statistics theories. However, as Damasio notices, our brain often can take the right decision in within a second or a minute, depending on what time will be accepted as a right one according to the aim, we want to achieve. If it is possible, the brain ought to make miracles, not being limited by the "pure mind". That is why, there is the need for an alternative model of its functioning (Damasio 1999, 198).

The option for a pure mind appears in a hypothesis of a "somatic marker". In Damasio's opinion, the somatic markers are the special kind of feelings generated on the base of the former emotions (Damasio 1999, 200). Those emotions and feelings were joined in a process of studying from the predictable future results of some event progress scenarios. The somatic markers do not help us out in reasoning. They go together with the considerations by the relief of some options (especially peculiar or profitable)

and quick elimination of them in a further analyzing progress. We may imagine them as an autonomic system of foreseeing and selection, which works whether we want or not, in order to estimate the extreme possibilities of the event development that may appear in the future.

Damasio proves, that the somatic markers have a close physiological basis. Some part of them came into being in the evolution process, the major part arose in our mind in the process of education and socialization by joining the particular category of the impulses with the categories of the somatic states. Therefore, the somatic markers are, in some sense, an effect of the cultural environment. On the neuron level, creation of the somatic markers is based on the process of system learning, that can gather some categories of existence or events with pleasant or unpleasant states of the body.

Damasio notices that evolution is rather economical one and prefers to improve and complete to build than to create from the beginning. It formed in the brains of many species, based on the body and orientated towards survival resolving mechanisms. Those mechanisms occurred to be effective in different types of ecological niches. The oldest decision mechanisms, from the evolution perspective, refer to the biological regulation, then to the private and social domain, and the youngest ones can operate on the collection of the abstract- symbolic elements, that is connected with the artistic creation, scientific reasoning, development of the language and mathematics (Damasio 1999, 217).

In Damasio's opinion there are three components that have an influence on the process of reasoning. These are: automatic somatic states, together with their directed mechanisms, operation memory and attention. All those elements interact in completing the assignment, that consists of selection of the parallel appeared representation. The problem occurs, because the brain construction allows conscious production of the limited stream of the mental and motive information. The images that compose our thoughts have to be constructed in phrases, which consist of the "sentence structure". The same thing refers to the movements, being exterior reactions, and supposed to bring expected results. The selection of the frames, in which those phrases, sentences of our thoughts and movements are to be created, is based on the parallel preview of the possible options. Therefore, both mind and attention require synchronous transforming, building of those arranged sequences remains uninterruptedly. The preview of the possible options depends on designating their order. Qualifying requires the criteria of establishment (preferences and values). Those criteria are provided by the somatic markers (Damasio 1999, 226).

Lot of decisions, made in a process of reasoning, have an influence on the future of the organism. It is right, in Damasio's opinion, that some of the criteria – directly or indirectly – are rooted in the biological impulses of the organism (that might be understood as its "mind"). The biological impulses may be expressed openly or secretly and can be used as a directed marker system, set in motion by the concentration of the attention on the representations kept by the operation memory in an active state.

Human acquired automatic mechanism of the somatic marker also thanks to creation of the culture and civilization. Although, its roots reach the biological regulation, it assumed cultural norms as well, created to survive in a given society.

The motion of the biological impulses, the states of the body and emotions compose as Damasio claims, a necessary basis of the rationality. Those lower levels control the direct, mutual connections between the brain and the body, positioning the body in the chain of operation, that helps in reaching the summit of the intellectual and creative abilities. Rationality, in another words, is shaped by the signals coming from the body. Damasio asserts outright, that the organism has some kind of the intellect, which has to be used by the mind. Verification of the justness of the taken decisions using the logic tools is a secondary process, decoding the rules of the autonomic preference (Damasio 1999, 229).

Damasio by describing the activity of the mind, assumed the existence of a close correlation of the mental and physiological processes. Intellectual processes are characterized by a high level of an automatic action ability, and this automatism has its biological basis. The same automatic action, in unison with the hypothesis, refers to the motion of the somatic markers, which were produced on the basis of the cultural experience.

Reviewer of the book "Cartesian' Error..." writes, that it became from the authors believe, that the traditional notion on the nature of the mind cannot be right – that is why Damasio questions dualism, explaining the right relations between the body and the mind, on the ground of the biology and culture.

Philosophical fundament of Damasio's theory proclaims a contestation with the Cartesian type of rationalism, a dispute leading to its declination. As an alternative we receive a theory, that can be called a theory of the "incarnated mind", amazingly convergent to the Leibniz's version of rationalism, built on the negation of the dualism rule. However, Damasio does not refer to Leibniz's philosophical system, but Leibniz's ideas are worth mentioning, because they might be a historical and philosophical base of Antonio Damasio's theory.

Leibniz presented an idea of the universe as a harmonious system, in which we experience the existence of unity and variety, coordination and division of parts, and this great order results in the fact, that the nature is the God's clock.

Although his model of nature was based on the laws of the mechanics but, by accepting the beginning Leibniz emphasized that those laws do not depend on the mathematical extension, but on some metaphysical causes. In his opinion, the basic scarcity of the mechanistic physics was not taking into consideration some dynamic factors existing in the nature. By replacing the Cartesian principle of the maintaining motion with the principle of the perfect balance between the cause and the effect, Leibniz educed some metaphysical consequences. Namely, that the power or energy, even measured, its future effect is something real, existing permanently in the substances. (See Święczkowska, 1998, 17).

In the language of Leibniz's metaphysics the term of power means tendency – that is one of the main attributes constituting the elementary unit of the existence, called by Leibniz a substance, or a real atom of the nature. The second attribute, is the perceptivness which is always some consequence of the tendency.

The world of the nature, whose matters are indivisible metaphysical points – substances – is ordered with the rules of God's interference. Admittedly, it has an factual status, but it is the world of the occurrences well grounded.

Endeavour and perception, considered on the level of a substance, stay in a close relationship with the physical world. Leibniz emphasizes in many places, that "all souls and spirits, simple substances are always created in some body, and there are no souls that are completely separated from that" (Leibniz, NE, Preface). The term of the bodily substance introduced by Leibniz corresponds to any living thing. In the letter to Arnaud, he explains it as follows: "Je responds que supposant qu'il y a une ame ou Entelechie dans les bestes ou authres substances corporelles, il en faut raisonner en ce point comme nous raisonnons tout de l'homme, qui est un estre doue d'une veritable unite, que son ame luy donne, non obstant que la masse de son corps est divise en organs, vases, humeurs, esprits; et que les parties sont pleines sans doute d'une infinite d'authres substances corporelles douees de leur propres Entelechies." (GP II, 120).

Leibniz by maintaining, that every substance deserves, in a general meaning, something that he calls endeavour (or tendency) and perception, forms from the cognitive substances the basis of their classification. The general term of monads or entelechies is kept for the substances, whose cognitive activity is close to zero, and whose beginning is the ability of unconscious perceptions. Those monads, in which the perception is connected with the memory or sensation, Leibniz calls the souls; furthermore, those souls are equipped in the ability of perception connected with the memory and consciousness, able to reason, are called the rational souls, or the spirits (Święczkowska, 1998, 24).

Every inspection of the world, every perception stays in a perfect conformity with the perspective of God's possession, however the reality of "God's vision" states the guarantee of authenticity of the perceptions created by God substances.

Cognitive activity of any substances depends on the God's rule of the sufficient propriety. This activity, independent in some sense from the complexity degree of the organism, responding to God's order of the world. Whereas, this order, in Leibniz's opinion, is a mathematical sequence, and in the matter of reminding, when God calculates and makes thoughts, the world is created.

This metaphysical thesis on the algorithm of the perception processes occurs currently in a context of the evolution theory. Some of the scientists (for instance Daniel Dennett) by analyzing the theory of the natural selection claim, that only those species survived, which developed well functioning highly specialized perceptive mechanisms and they behave, as they recognized "propriety freely seized towards them". Referring to such approach Roger Penrose argues: "If we suppose that the action of the human brain, conscious or otherwise, is merely the acting out of some very complicated algorithm, then we must ask how such an extraordinary effective algorithm actually came about. The standard answer, of course, would be "natural selection". As creatures with brains evolved, those with the effective algorithms would have a better tendency to survive and therefore, on the whole, had more progeny. These progeny also tended to carry more effective algorithms than their cousins, since they inherited the ingredients of these better algorithms from their parents; so gradually the algorithms improved – not necessarily steadily, since there could have been considerable fits and starts in their evolution – until they reached the remarkable status that we (would apparently) find in the human brain." (Penrose, 1995, 454).

A question occurs, whether those cognitive processes, proper to all organisms on the different levels of their complexity can be reproduced using the tools of made by the creative mind. In other words, if organism's acting follows some rational plan, it is possible to stimulate it. Damasio speaks about automatic acting of decisive processes, that have the basic influence

on the future of the organism, which is possible by the mechanism of the somatic marker. He claims, that the logical principles, the rules of the pure mind may decide about the additional item of this mechanism, but they do not decide on the effective side of reasoning.

According to Leibniz's plot, rational souls, or the spirits joining the aggregates of the monads are able, in his opinion, to recognize and reproduce the mathematical plan of the creator. This plan, seized in the natural order of the idea, is memorized in everyone's mind. Leibniz assumed, that apperception, in other words thinking that leads to the idea, has an algorithmic character. The natural order of the idea is isomorphic with the universe order, and this order is a consequence of Creator's action, who by choosing the best from the possible worlds, created the one following the internal order of the thoughts. On trusting the power of the creative mind, Leibniz believed, that the human can reproduce this natural order of the idea and can recognize the complexity of universe on all the levels. On the other hand, there are still some essential difficulties in reconciling Leibniz's trust confidence in miraculous force of algorithm with the dynamic structure of cognition unfolded in "Monadology". He wrote: "It must be confessed moreover that perception and all that depends on it are inexplicable by mechanical reasons that is figures and motions". Mechanization of cognition process is therefore only imitating nature, it is the human ability of reconstructing God's order, likewise as "no machine made by human art is not a machine corresponding with the God's one which is a natural authomaton" (Leibniz, Monadology, Paragraph 64).

Therefore, we may repeat for Damasio, that the organism has some kind of the intellect, which must be used by the mind. The first one may be composed of monads' reasoning, that are integrated with the monad joining all the body with the mind, soul, or entelechy.

We can find a surprising resemblance to this idea, in Daniel Dennett's book entitled "The nature of the mind". He maintains that not only we come from macromolecular robots but we are composed out of them. And those collections of billions of macromolecular mechanisms evinces authentic consciousness (Dennett, 1997, 36).

To finish the discussion, we should consider just natural robots, meaning the God's machinery.

On the question stated in the title, it is possible to give at least a partial answer. Many research workers share the belief, that considerable part of the cognition processes and the undertaken actions, proceeds in the body automatically. It is accepted according to the lower organisms.

However, the problem refers to those cognition processes which determine the species' difference between human and animals' world. The issue still remains open.

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