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MANY-WORLDS THEORY OF TRUTH

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Abstract:
The logical world is a set of propositions, united by common principles of establishing their truth. The many-worlds theory asserting that the truth of any proposition in any given logical world is always established by comparing it with standard propositions in this world – directly or via the procedure of transferring the truth. Existing theories of truth (correspondence, authoritarian, pragmatic, semantic of Tarski and all other possible) are not theories of truth in the full meaning of the word; they must be considered merely as methods of fixation and generation of true propositions in one or another logical world.

First of all, I want to draw special attention to the fact that we consider here only, and solely the concept of truth as a characteristic of proposition in one or another language (‘His testimony is true,’ ‘True solution of problem’), but not as teleological-epistemological category (‘Knowledge naturally strives for truth,’ ‘Truth dwells in man’s heart’ etc.).[1] The concept of truth is understood here in its usual etymological meaning: true means correct, veritable, certain. That is to say, the true proposition is a correct, valid, indisputable proposition.

The principle starting the point of this theory is the assertion of truth’s relativity: the truth of proposition can be established only and exclusively within the boundaries of one of the many more or less closed systems, which I propose to call logical worlds. It is clear that the proposition ‘Snow is white’ is true in the logical world of English language and it is not true in the world of Amazonian tribe’s language. The proposition ‘In two dimensional space, for any given line R and point P not on R, there is exactly one line through P that does not intersect R’ is true in the logical world of Euclidean geometry, but it is false in the world of Lobachevskian geometry. The proposition ‘Reincarnation exists’ is not valid in logical worlds of different religions.

The term “world” is chosen to designate areas in which the truth of a proposition is fixed, because such areas are sufficiently autonomous and self-consistent – they have a comprehensive mechanism to determine the truth of propositions formulated in any of the languages adopted in these areas. It should also be noted that the term “logical world” is not quite accurate, it would be more correct to speak about “logical-worldview-linguistic worlds.” However, for shortness sake and keeping in mind the problem of statement area, I will use the term “logical worlds.”[2]

Now, combining an etymological understanding of truth and the notion of logical worlds, it is possible to give a more precise definition: a proposition should be considered true in one or another logical world, if it is correct (valid, indubitable) in this world. Admissibility is understood here in the broadest sense, as a compliance with logical, worldview, linguistic norms, rules and traditions of the given world.

Taking into account this definition of truth, we can concretize the concept of logical world. In essence, the logical world is a set of propositions, united by common principles of establishing their truth. So the logical world of Euclidean geometry consists of true propositions (theorems), obtained through inference (according to the laws of formal logic) from a specific set of axioms. At the same time, the world of Lobachevskian geometry includes a set of other propositions received by the same rules, but based on a modified set of axioms. Logical world of any religion is formed of propositions that are true from standpoint of the Scriptures and dogmas.

At the same time, we can also formally specify the concept of truth: asserting the truth of a certain proposition necessarily means that it belongs to one or another logical world. If we assert the truth of proposition without mentioning any logical world, it is automatically assumed that we have in mind the logical world of language, in which this proposition was formed (‘Snow is white’), or the logical world of the speaker (‘Angel is flying above me’). The individual logical world of man has its own implicit and explicit rules for determining the truth of a proposition, sometimes they pretty much differs from those in the worlds of other people.

Further, it should be noted that logical worlds are not isolated, they largely overlap. It is clear that logical world of the individual person – the set of true for him propositions – is formed at the intersection of linguistic logical world and other worlds: worldview, political, perhaps one of religious, etc. Therefore, the true proposition made without reference to its belonging to one or another logical world, may be interpreted as poly-true: it can be true simultaneously in the language world, some religious worlds and, of course, in the worldview logical world of the speaker. Consequently, when we affirm the truth of the proposition, we should always specify, belonging to what logical world is in question.

It is noteworthy that initial tenets of the theory of truth, which I suggested to call relativistic or many-worlds, were formulated without mentioning of the so-called “reality,” “actual state of things,” any “real situation.” It was enough to understand that there are non-finite sets of logical worlds with their own mechanisms (methods, rules, norms) of identifying and generating of acceptable propositions, and that a statement of proposition’s truth merely indicates its belonging to one of these worlds (or to several simultaneously). From this understanding and from given examples it clearly follows that there is no and cannot be one, universal for all worlds mode of ascertainment of proposition's truth. In religious worlds the dogmatic way of establishing the truth has a priority – everything that fits with Scripture is true. In theoretical-scientific, mathematical logical worlds the absolute priority is given to the method of logically deducing true propositions from established axioms – although the truth of axioms in each of such worlds is accepted by agreement, assumption. In individual logical worlds a statement of truth of spoken propositions is usually based on immediate personal conviction (often without understanding the reasons for this conviction) and on the norms of language practice.

But I cannot avoid talking about the relationship between truth and reality, because a lot of people, including logician-philosophers, interpret the truth as an immediate correspondence to the actual state of things. Although such interpretation brings forth an unsolvable problem: how can we compare, relate with each other proposition and real situation? Proposition is a set of words, but reality is objects, facts. How they can be compared? Should we put objects to the lines of a book? Or should we make grammatical and stylistic analysis of facts? It's like comparing a price of sausage with a length of shopboard. Indeed, it is quite clear that correspondence can be established only between similar objects.

The many-worlds theory of truth has no problem of correlation between proposition and reality (in the traditional sense of the word) – “real” here is understood merely as an indication that a proposition belongs to one or another logical world. So, the ascertainment of the truth of proposition boils down to comparing it with the set of true propositions. Thus, we obtain another one crucial point of the many-worlds theory: the *truth of proposition in a specific logical world is established only and solely by comparing it with other propositions of this world*.

This assertion is trivial for majority of already mentioned logical worlds and mechanisms of determining the truth. The stated truth of proposition, belonging to any religious or political logic world, is indicative of the fact, that it coincides with the dogmata, absolutely true propositions of this world, or that it is compatible with the dogmata logically, through a chain of correlated propositions. In geometry (and other theoretical logical worlds) axioms play the role of “dogmas” accepted as true propositions, and establishing the truth of theorems consists in proving of their unambiguous logical connection with the axioms (in fact, it is the coherent method of determining the truth). Well, it is clear that the proposition ‘Snow is white’ uttered by me is true for me because in the logical world of English language, in which I was trained, propositions like ‘Snow is white,’ ‘Grass is green,’ ‘Wheel is round’ are initially true. To establish the truth of proposition ‘Snow is white’ I do not need to look out the window, I need only to correlate it with a valid (true) proposition in the language.

Another trivial mechanism for determining the truth of proposition is to compare it with a statistically sufficient set of similar propositions. So we accept as true the results of votes, sociological polls, testimony of the accused in court, confirmed by evidences of number of witnesses. As a special method of determining the truth of proposition we can mark out its comparison with authoritative propositions. Among these must be reckoned the texts of dictionaries, encyclopedias, manuals, handbooks, answers at the end of problem book, and the like, as well as statements of recognized authorities in a given logical world. Although it is clear that the authoritative method of determining the truth of proposition represents *per se* a mixture of dogmatic and statistical methods: it is externally perceived as a reference to dogmatic proposition, though its truth is established statistically (through multiple verifications, approvals, elaborations). In many logical worlds there exist propositions, whose truth is determined by convention, by agreement, that is, if there is a coincidence of propositions made by some set of people. These are propositions of legal laws, road traffic regulations, etiquettes, sports games, etc. The truth of many propositions in science, particularly those that fix terminology, is also established conventionally.

The essential moment in the procedure of determining the truth is the difference in status of correlated propositions: any particular proposition, for the determining of its truth, should be compared with some standard proposition which truth some way or other is already fixed in a logical world. It is clear that in different worlds such standards are dogmatic, authoritative, authoritarian, statistic, conventional propositions: texts of the Scriptures, encyclopedias, handbooks, statements of national leaders and famous personalities, legal laws, various conventionally established rules. In logical worlds of theoretical systems the role of standard true propositions play axioms and rules of transferring the truth from axioms to other propositions of a logical system (theory). Thus, *the truth of any proposition in any given logical world is always established by comparing it with standard propositions in this world – directly or via the procedure of transferring the truth.*

As I noted above, in logical worlds of individual languages the confidence in the truth of man’s spoken proposition is based on matching it with an identical proposition from the world of general (historical) Language, the truth of whose propositions is standard. Having introduced the concepts of individual language (l) and general language (L), we can write down: the proposition ‘Snow is white’ in (l) is true because ‘Snow is white’ is true in (L). This method of forming true propositions, when the truth of some particular proposition follows directly from a set of language rules, can be called linguistic. The linguistic truth does not require any mediation, any introducing of additional conditions; it is justified by tradition of using the stable language forms: ‘Wheel is round,’ ‘Fire is hot,’ ‘Winter comes after autumn.’ In fact, the linguistic truth should be considered as a variation of statistical. If there can be found some connection of the linguistic truth with the real situation, it is only indirect connection – through the long genesis of Language. To determine the truth of proposition ‘Winter comes after autumn’ we have no need to observe a change of seasons for several years – the truth is guaranteed by the long experience of such observations, which is fixed in the Language as proposition ‘Winter comes after autumn.’ It is clear that speaking about the identity of propositions in language and Language, I fix only the external, formal aspect of the problem – actually such linking of propositions takes place in the process of upbringing and learning and afterwards is perceived as natural a priori truth of the individual language.

It is remarkable that the formal record of determining the linguistic truth of individual language proposition (p) – "p in (l) is true if and only if p is true in (L)" – coincides with the T-scheme of Tarski, revealing its trivial sense: *proposition in the individual logical world is accepted as true, if in the logical world of general Language there exists identical true proposition.*

Of course, the introduced many-worlds theory asserting that the truth of proposition can be established only and exclusively in the procedure of comparing it with other propositions in concrete logical world, is faced with the problem of fixing the truth of propositions stating some facts, some situation in reality (for example, 'It's snowing outside the window'), i.e. with the problem of determining the so-called *correspondence* truth.

First of all, it should be noted that propositions asserting some facts, are formed as true in the *individual logical world* – in the individual language and with reference to individual actuality of man. One may ask, why it is important to take this into account? The answer is simple: we must consider all variety of facts about which people can speak – not only such propositions as 'It's snowing outside the window,' but also such as 'The angel flies over me,' 'The President said the truth,' etc. And with this general approach it becomes clear that the truth of proposition about a fact in individual logical world is partly linguistic and partly statistical. That is, proposition fragments, but *per se* independent sentences – 'It's snowing,' 'The angel flies,' 'The President said' – should be regarded as linguistically true: their obvious validity is built into the language traditions. If a man doesn't know words "angel" or "president," the propositions mentioned will not be true in his individual logical world. As true, he will accept, for example, the propositions 'Alien flies,' 'The guy on the telly said.' The truth of full propositions about facts is based on a statistical comparison of them with the experience of using propositions in speaker's life. For instance, if a man has never seen snow, then such a fact as 'It's snowing outside the window' just will not exist in his actuality. And a man with, for example, an opposition experience of "political speaking" as a true description of the same "fact from telly" will say, 'The President told a lie.' That is, in the individual logical world any fact is just uttered proposition itself. And the truth of this proposition is fixed only as its statistical consistency with many other facts – propositions of the individual language and rules of the general Language.

Thus any proposition in the speaker's individual world is considered by him as obviously true because of a priori consistency of his language and his actuality. But since his actuality is in principle unavailable to others, as a fact, fixed in one or another proposition, we must accept only and solely this proposition, which in and of itself (outside the individual world) is neither true nor false. And naturally, if we want to transcend the individual truth, to speak about the truth of concrete proposition-fact in some supra-individual logical world (social, religious, political), then we must introduce other methods of determining the truth, other ways of comparing propositions. For instance, if several people looking out into the street will utter one and the same proposition 'It's snowing outside the window,' then this proposition must be recognized statistically true. Although we usually do not need such a statistical method (e.g. it is not necessary for us to look out the window ourselves to check whether it is snowing), if the person who pronounced the proposition about snow is sufficiently authoritative, i.e. if we know that so far most of his propositions coincided with ours. In much the same way, propositions 'The angel flies over me' and 'The President told the truth' can be automatically accepted as true in the respective logical worlds (religious and political), if the speaker is deemed indisputably authoritative in these worlds – saints and spiritual leaders always utter only true propositions. If the speaker is not the authority, then again just statistics – up to determining the truth of proposition by simple voting.

I must once again stress that seemingly so obvious connection of propositions like 'It's snowing outside the window' with the so-called reality is substantiated only by consistency of individual languages and actualities of various people on the subject-physical level. At this level, due to unified upbringing and education and unity of Language, we all belong to one logical world. But as soon as at other levels we disperse to different political, religious, ethic, aesthetic worlds, the illusion of relationship between the propositions' truth and some single reality immediately disappears. Also, it becomes evident that the truth is relative and that it can be established only through procedure of comparing propositions within separate logical worlds.

Also, it is necessary to consider the concept "scientific truth," i.e. to analyze the purpose and methods of determining the truth of propositions in logical world of science. First of all, it should be stated that this world is initially separated into two sufficiently autonomous subworlds, each with its own principles of determining the truth: theoretical and experimental. The truth of propositions in the world of theories (theoretical truth), we have already considered. As it should be in the many-world concept, it consists in the belonging of proposition to one of logical systems (theories). In theories themselves the truth is established coherently – via comparing the proposition with a set of initially true axioms and rules. The truth of proposition in the world of the experiment (empirical truth), in effect the truth of instrument readings, is established statistically. To the statistic here contribute both reproducibility, repeatability of propositions-results, and a long experience of using instruments – concrete and all previously employed in scientific practice. It is worth noting that establishment of the empirical truth of proposition (measurement result) does not at all imply its correspondence to "reality," to "natural facts." A fact here is only a proposition itself (instrument readings), the truth of which is extremely statistical in nature.

Further, according to the developed theory, we must accept that determining the truth of scientific proposition consists in comparing theoretically true propositions and empirically true ones. But in this case the question arises, whether we have the right to compare two propositions formulated in different languages – theoretical and experimental? Do we have confidence that, for example, the concept "temperature" in theory, its concrete calculated value, is comparable with the numbers on thermometer scale? The answer to this question gives many-worlds theory of truth: the comparability of theoretical and experimental languages is conditioned by the existence of the single logical world of science, which historically includes in itself both theoretical and experimental subworlds, theoretical and experimental languages. That is, the truth of scientific proposition, determined by comparing identical theoretically and empirically true propositions (for instance, the coincidence of theoretically predicted temperature with instrument readings) is statistically guaranteed by all previous scientific experience.

It should be noted that practically in science there is established not some abstract truth, but the truth either of theoretical or of empirical propositions. In other words, there either takes place the testing of predictions of theory – and then the theoretical truth is determined by the fact of coincidence of experimental data with statistically reliable empirical truth. Or, contrariwise, the truth of experimental data is verified by comparison with theoretical predictions of recognized, authoritative scientific theory.

Finally, it would be useful to correlate the introduced concept with other existing theories of truth. Though I think that conclusion, which I will draw now, is already obvious to many: existing theories and subtheories of truth (correspondence, authoritarian, pragmatic, semantic of Tarski, consensus, coherence and all other possible) are not theories of truth in the full meaning of the word; they must be considered merely as independent, but certainly not the only ones, not universal methods of fixation and generation of true propositions in one or another logical world. In some logical worlds (religious, political) dominates authoritarian method, in other (theoretical) coherence method, and in third (social) prevails consensus form of determining the truth. Such an approach frees these "theories" from accusation of incapability to determine the truth of all possible propositions – now, when they moved to the rank of particular method, it is not required from them. (The task of presenting a complete classified list of all mentioned here methods of fixing the truth, was not set as yet.)

It should be emphasized that from the position of the offered concept, it is incorrect to accept the correspondence theory as method of forming true propositions. This theory is rather to be regarded as aberration, though relevant to common sense. Earlier I tried to show that so obvious to ordinary common sense correspondence between the uttered and the seen is only a result of complex historical coordination between linguistic and other practices, the consequence of unified upbringing and education.

It remains to mention as well the so-called "deflationary theory of truth", which states that the notion "truth" is altogether needless. This conclusion it substantiates by such examples as following: the statement 'It's true that snow is white' adds nothing to the meaning of proposition 'Snow is white.' First, deflationary theory says nothing about the nature of truth, it does not answer the question: why proposition 'Snow is white' is true? Therefore, it cannot be classified as theory or method of determining the truth. Second, within the scope of many-worlds theory the content of deflationary theory boils down to banal point: if proposition is recognized as true because of the fact of its belonging to one or another logical world, then within there is no need to repeat each time that it is true. For example, when proving theorem it is not necessary each time to point out the truth of axioms. However, if we consider the belonging of some proposition to different logical worlds, then we necessarily must say, in which of them it is true and in which it is false. Thus, the Euclidean axiom about parallel lines is certainly false in the logical world of Lobachevskian geometry. And in such cases we cannot do without predicates 'true' and 'false.'

Instead of summing up, I'll give some recommendations to those who have amassed objections to the described here many-worlds theory of truth or to some of its basic assumptions. If you want to disprove the relativity of this theory, you must show that for any proposition there is only one truth value (for example, that the proposition 'The President told the truth' is always true). If you do not agree with the thesis that truth of proposition can be established only and exclusively by comparing it with other propositions, then offer a mechanism of comparing a proposition (a sequence of words) with, let us say, rain or snow. Or there is another option: specify, please, to what actual state of things, to what reality correspond true ethical, religious, political propositions. Those who are against the introduction of the concept "logical world" must convince everyone (and, more importantly, themselves) in the existence of universal rules of fixation and generation of true propositions – one and the same for all possible cases (all worlds).

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Notes

1.The notion "truth" in this text I associate only with affirmative propositions – the problem of truth of other types is not considered here. Also, for the present without analysis the problem of correlation between sentences and propositions will remain – it is assumed that in any case the proposition is a sentence in a more abstract language than the ordinary one. It should be noted also that for understanding of this text it is not essential, whether the truth is treated as a predicate of proposition or as its value. Though within the scope of the offered concept it is more appropriate to speak about predicativity of the truth – the assertion of truth of proposition states it belonging to some set of propositions.

2. The concept “logical world” can be correlated with the known in logic concept “possible world” only very remotely – and only with “possible word” in its latest interpretations as some context, the situation in which asserting proposition’s truth is possible, but not in its original meaning of conceivable world as opposed to actual. Logical worlds as sets of true propositions are not hypothetical, but they are ontological, we can even say real, if reality is understood as something similarly manifested in personal realities of some set of individual subjects.

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THE DISCUSSION ON THE PRIVATE LANGUAGE ARGUMENT: LINGUIST VERSUS PHILOSOPHER

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Abstract:
The article is devoted to the discussion on the private language argument. I want to support the position already expressed by G.P. Baker and P.M.S. Hacker, adding to it yet one argument which may, in my opinion, make this view more intelligible and clearer. The position consists in affirming that the so-called community view does not contradict the opposing view on the nature of language. The point is that these views consider language on different levels. One may compare these different levels of investigation of the subject with different questions that a linguist and a philosopher may try to decide when they analyze the language. In this case the researcher who shares the community view can be represented as a linguist and his opponent as a philosopher.

Introduction

If the rule-following problem for the first time formulated by later L. Wittgenstein is held to be one of the significant problems in contemporary philosophy of language then the discussion on the private language argument is often considered as the kernel of the problem. The debate has been conducted in numerous publications; many well-known philosophers have expressed their views on the question. It is notable that the dispute continued on very high level of polemic. Philosophers produced webs of arguments, replies to them and replies to the replies. Indeed, as for sharpness of confrontation the bibliographic references remind one of reports on war operations.

In 1976 R. Fogelin (1976) for the first time connected the rule-following problem with the question about the existence of a private language and asserted the impossibility of one. Later C. Wright (1980) and C. Peacocke (1981) endorsed this conception. Kripke's book *Wittgenstein on Rules and Private Language* brought this viewpoint into prominence. Kripke also insisted that a private language is impossible (1982). In 1984 C. McGinn (1984) expressed doubt whether Wittgenstein connected the theme of rule-following with communicative practice and the denial of the possibility of a Crusoe's language, and G.P. Baker and P.M.S. Hacker in their two works *Skepticism, Rules and Language* (1984) and *Wittgenstein: Rules, Grammar and Necessity* (1985) advanced powerful criticism against Kripke's interpretation of Wittgenstein's private language argument as well as against philosophers who held a language of a solitary speaker to be impossible. In 1986 N. Malcolm (1986) expressed views that were similar to Kripke's position in respect of the essential social nature of language. In a review of this work Hacker (1987) averred Malcolm's interpretation to be mistaken. Malcolm (1989) responded and in his article *Wittgenstein on Language and Rules* criticized Baker and Hacker's viewpoint. The Oxford philosophers countered with new critical arguments in their paper '*Malcolm on Language and Rules*' (Baker & Hacker 1990). Other 'heavyweights' also found themselves on different sides of this barricade. D. Davidson (1992), although on different grounds from Kripke, asserted that a community of speakers is necessary for a language to exist. H. Putnam (1996), by contrast, expressed confidence in the logical possibility of a Crusoe's language. An impressive group of other thinkers participated in this long drawn out discussion. Among them: S. Blackburn (1984), M. Budd (1984), D. McDowell (1984), P. Moser (1991), E. Savigny (1991), T.S. Champlin (1992), G. Robinson (1992), D.G. Stern (1994), J.V. Canfield (1996), P. Frascolla (1994), F. Benjamin, J. Armstrong (1984), H.O. Mounce (1986), P. Horwich (1990), S. Mulhall (2006), M. Kusch (2006) and others.

In this article I want to support the position already expressed by Baker and Hacker, adding to it yet one argument which may, in my opinion, make this view more intelligible and clearer. The position consists in affirming that the so-called community view does not contradict the opposing view on the nature of language. The point is that these views consider language on different levels. One may compare these different levels of investigation of the subject with different questions that a linguist and a philosopher may try to decide when they analyze the language. In this case the researcher who shares the community view can be represented as a linguist and his opponent as a philosopher.

But to demonstrate the correctness of this thesis I must first present the different viewpoints on the private language more carefully.

Kripke's position

The originality of Kripke's viewpoint on the private language argument consisted in the fact that he read the appropriate paragraphs of the *Philosophical Investigations* otherwise than the majority of Wittgensteinians. Usually one considered that the private language argument is contained in the paragraphs following §243. But Kripke insisted that the core of the argument is contained in earlier passages. He suggests that the conclusion regarding the private language problem has already been drawn in §§201-2 in which, in his opinion, the main point of the *Philosophical Investigations* has been presented too. The passages following §243 were rather consequences of these more fundamental considerations. We can see how Kripke describes this situation in his book:

> *A common view of the 'private language argument' in* Philosophical Investigations *assumes that it begins with section 243, and that it continues in the sections immediately following. This view takes the argument to deal primarily with a problem about 'sensation language.' Further discussion of the argument in this tradition, both in support and in criticism, emphasizes such questions as whether the argument invokes a form of the verification principle, whether the form in question is justified, whether it is applied correctly to sensation language, whether the argument rests on an exaggerated scepticism about memory, and so on. Some crucial passages in the discussion following §243 – for example, such celebrated sections as §258 and §265 – have been notoriously obscure to commentators, and it has been thought that their proper interpretation would provide the key to the 'private language argument.'*
>
> *In my view, the real 'private language argument' is to be found in the sections preceding §243. Indeed, in §202 the conclusion is already stated explicitly: 'Hence it is not possible to obey a rule 'privately:' otherwise thinking one was obeying a rule would be the same thing as obeying it.' I do not think that Wittgenstein here thought of himself as anticipating an argument he was to give in greater detail later. On the contrary, the crucial considerations are all contained in the discussion leading up to the conclusion stated in §202. The sections following §243 are meant to be read*

in the light of the preceding discussion; difficult as they are in any case, they are much less likely to be understood if they are read in isolation. The 'private language argument' as applied to sensations is only a special case of much more general considerations about language previously argued… [16]

The originality of this interpretation consists in the fact that Kripke connected the private language argument and the rule-following problem immediately, and so the argument changed its usual content. According to Kripke this argument is not merely about individual personal sensations, as it was previously held to be, for example by Ayer (1954) and Rhees's (1954) discussion. Now the concept of an individual's language has turned out noticeably wider. Not only sensations but things in the external world, abstract objects, and different events could be held to be putative references of private language expressions. The main point consisted in the suggestion that a private language is used by a speaker who applies linguistic expressions rather like a peculiar Robinson Crusoe isolated from any linguistic community. In respect of such a private language, a problem arises which Kripke formulates as a rule-following problem. In point of fact, the impossibility of a private language follows for Kripke from the presence of the rule-following problem in the formulation which he presented in his book.

Recollect what Kripke's skeptical argument regarding the use of a linguistic expression consisted in. At the present moment, when I use the sign '+' in the expression '2 + 2,' I find myself in trouble regarding the question: what kind of meaning should I ascribe to the sign? It might seem that plus, the arithmetical function of addition, must be considered as the meaning of the sign. But the point is that because of the finiteness of my cognitive experience, I cannot grasp the function entirely. On this account, we may consider the following epistemological situation to be possible. In a particular case I may use '+' to denote some other function which is different from plus with regarding to arguments larger than 1000, but here with regard to arguments less than 1000, the function coincides with plus. A radical ambiguity arises. In this particular application of '+' my use of the expression falls under at least two rules at once and I have nothing in my consciousness that could be resolve the trouble.

Thus, when we use a word, we do not grasp its meaning. The only thing that we can use is some illusion of meaning constancy, an impression that we understand what our words mean. In Kripke's opinion, an isolated Robinson Crusoe who uses language in solitude is not capable of producing and supporting the illusion. Like Buridan's ass, he will constantly be in a situation of radical indeterminacy of choice between alternative rules of application of his own linguistic expressions.

There is only one thing, according to Kripke, that could help Robinson Crusoe to avoid this predicament. It is a linguistic community. A linguistic community is indeed able to generate and support the illusion of constancy of meanings that is so necessary for the successful use of language. The illusion of constancy is generated in a person by a shared linguistic practice with other speakers in a community. In the process of communication, a speaker gets approval for his linguistic acts from other persons of the community and these facts reinforce his confidence in the correctness and consistency of interpretation of rules of application of linguistic expressions in different speech situations. In this way, an impression of stable and clear use of linguistic expressions is generated. Kripke holds that in this way we do not generate real meanings and genuine rules. Rather we generate mere illusions of meaning for our shared communicative practice does not give us, in some miraculous way, a capacity to grasp a rule in its infinite expansion as a whole, for example, the rule of addition that must be conceived as determining the meaning of the sign '+'. Nevertheless prolonged mutual agreement between members of a community fulfills an important task. It gives confidence to speakers regarding the correctness of their utterances. This language game is the one possible way to avoid the situation of radical indeterminacy of meaning.

This event – the generation of an illusion of meaning by a linguistic community – turns out to be a necessary and a sufficient condition for the successful functioning of language.

On this account, a private language is not possible at all. The necessary condition of the functioning of a language is not realized in it, for even the illusion of constancy is absent.

G.P. Baker and P.M.S. Hacker: a Robinson Crusoe is possible

The Wittgensteinians from Oxford – G.P. Baker and PM.S. Hacker – chose a different route. Contrary to the so-called 'community view' regarding a private language, they asserted that the idea of a Robinson Crusoe, i.e. a person who follows his own rules for the use of expressions is intelligible. In the second volume of their *An Analytical Commentary on Wittgenstein's Philosophical Investigations* they pay attention to a quite different aspect of the rule-following practice and the successful functioning of language:

> *The concept of following a rule is here linked with the concept of regularity, not with the concept of a community of rule-followers* [3].

And in *Scepticism, Rules and Language*, which was devoted to criticisms of Kripke's position, they insist on their different view too:

> *Note that nothing in this discussion involves any commitment to a multiplicity of agents. All the emphasis is on the regularity, the multiple occasions, of action (cf. §199). What is here crucial for Wittgenstein's account of the concept of following a rule is recurrent action in appropriate contexts, action which counts as following the rule* [2].

In point of fact, Baker and Hacker's position can be presented as follows. A speaker does not need any community for the identification of rules and the following of rules. He needs a regularity taken as a norm of a behaviour, repetition of his action, and this is a necessary condition for the successful functioning of his language. When the linguistic acts of a Crusoe are regular, when he uses certain linguistic expressions to refer to certain things, facts, events, when this use is a custom for him then we have no reasons to doubt, first, the constancy of a Crusoe's language use and, second, the fact that he understands the meanings of his words, i.e. the rules of application of the words which he uses. The personal regularity of linguistic acts will discipline a Crusoe. He will be able to compare his present uses of words with past ones and estimate as correct more regular uses that he treats as standards correctness.

Another important argument against the community view, in Baker and Hacker's opinion, is that a rule and what accords with it must be internally related whereas the community view presents this as an external relation:

> *The pivotal point in Wittgenstein's remarks on following rules is that a rule is internally related to acts which accord with it. The rule and nothing but the rule determines what is correct. This idea is incompatible with defining 'correct' in terms of what is normal or standard practice in a community. To take the behaviour of the majority to be the criterion of correctness in applying rules is to abrogate the internal relation of a rule to acts in accord with it* [3].

Adoption of community agreement as a criterion of correctness for the application of rules means adopting something independent in the process of using of linguistic expressions by reference to which a speaker may orient himself in his linguistic acts. This is different from appealing to Platonic ideas because community agreement does not exist in an independent metaphysical world but in finite worlds of linguistic communities only. Nevertheless it still has a series of features which are similar to Plato's universals. Namely, it is constant, independent of personal experience, abstract in the sense that it is unconnected with any particular case of a concrete speech act and general inasmuch it covers each particular communicative situation in a linguistic group.

All of this, Baker and Hacker consider, is contrary to the main strategy of thought of the later Wittgenstein. Rather, a rule and its application are internally related, i.e. the identity of each is dependent on the other. A rule can be understood only on the basis of the concrete acts which are its application. A rule is defined by reference to what counts as accord with it. The rule of the number series '+2' gets its meaning in concrete steps which a person makes in the process of construction of the series, i.e. when he writes down '1000' after '998' and so on. The rule is not defined from the very outset as some constant entity in which all possible steps of its application have already been coded.

From Baker and Hacker's point of view an isolated Robinson Crusoe is able in his behaviour to exhibit and sustain this specific internal relation between rule and its applications. He can use language in his personal way in a regular and uniform sequence of acts, viewing the regularity as a uniformity which he treats as a norm.

N. Malcolm's counterarguments

If we try to classify Malcolm's critical arguments regarding the position of the Oxford philosophers we can find at least three important ones. I shall examine them sequentially.

A Crusoe by birth and a Crusoe by accident

First, Malcolm points out that we can talk about Robinson Crusoe in different ways. He introduces a distinction between an isolated Crusoe who lived in a human community for most of his life but once by accident found himself in prolonged solitude, and a primordial isolated Robinson who lived all his life in solitude and no contact with people. For short, I will call these persons 'Crusoe by accident' and 'Crusoe by birth.' One can imagine different cases of Crusoe by accident. For example, as Defoe does it in his novel, the isolation of a human being may happen as the result of a shipwreck. When Baker and Hacker assert the possibility of a Crusoe's language they imagine a catastrophe too, but on a universal scale:

> *Since Robinson Crusoe could talk to himself, keep a diary, follow rules, would he cease to be able to do so if, unbeknownst to him, the rest of mankind were destroyed by a plague? Obviously not* [2].

Malcolm imagines a monastic order:

> *It is easy to supply a background which does not imply that those people had spoken only in monologue for their entire lives. For example, after a normal upbringing, they might have become members of a monastic order that forbade its members to speak to one another* [19].

H.O. Mounce thinks up an even more mysterious event:

> *…suppose that some terrible affliction has fallen on a whole population, so that people speak only to themselves, having lost all interest in one another* [23].

The one important circumstance that is invariable in these situations is that a man lived among people, used common language for communication with others, and suddenly these connections are broken.

Criticizing the Oxford philosophers' position, Malcolm, at first, remarks that it is not clear whether they understand the distinction between Crusoe by accident and Crusoe by birth or not. Further Malcolm concedes that Crusoe by accident is possible. We should not enter into discussion regarding this subject, for it is quite clear that a speaker can use language in this situation. Indeed, anyone can find himself in isolation every day. But in these situations we do not lose the capacity to speak intelligibly. For example, I can repeat my speech to a forthcoming scientific conference, a schoolboy can do his homework in mathematics, calculating in accordance with arithmetical rules. All these acts we can do in solitude. We should talk, as Malcolm suggests, about a different scenario. Can a Crusoe by birth use his own personal language? That is the question. Malcolm directs our attention to a fragment from Baker and Hacker's book:

> *Wittgenstein was aware of the danger that his remarks about agreement might be misinterpreted in this way. He quite explicitly took care not to exclude the possibility that a solitary individual could follow a rule or speak a language to himself* [3].

and Malcolm reacts to it in the following way:

> *It is far from clear what the issue is here. Can a 'solitary individual' follow a rule? Most of us follow rules when we are alone. I calculate my income tax alone. I write letters, read, think, when I am alone. I was brought up in the English language and carry it with me wherever I go. If I were shipwrecked, like Robinson Crusoe, on an uninhabited island, I would retain (for a time at least) my knowledge of English and of counting and arithmetic. It is normal for people to do calculations, carry out instructions, prepare plans, in private. In this sense, all of us are 'solitary individuals' much of the time.*
> *Of course all of us have spent many years in being taught to speak, write, calculate. We grew up in communities of language-users and rule-followers. The philosophical problem about 'solitary rule-followers,' should be the question of whether someone who grew up in total isolation from other human beings, could create a language for his own use. Could there be a Crusoe who (unlike Defoe's Crusoe) was never a member of a human society, yet invented a language that he employed in his daily activities? And does Wittgenstein concede such a possibility?* [19].

First, Malcolm gives a negative answer to the philosophical question and, second, he asserts that Wittgenstein denied the possibility of a Crusoe by birth too. The common agreement of the linguistic community is a necessary background on which a language and a practice of rule-following can arise at all. A Crusoe by birth is excluded from a community in principle, and his attempts to develop his own language and follow rules are doomed to failure inasmuch as he is not able to find any criteria of correctness for his linguistic acts. A Crusoe by birth, in Malcolm's opinion, is not able to differentiate between his conviction that he follows rules and genuine rule-following, i.e. the distinction Wittgenstein speaks of in §202 *PI*:

> *And hence also 'obeying a rule' is a practice. And to think one is obeying rule is not to obey a rule. Hence it is not possible to obey a rule 'privately:' otherwise thinking one was obeying a rule would be the same thing as obeying it* [31].

On Baker and Hacker's observation that Wittgenstein included the theme of a Crusoe in his writings explicitly:

> *It is noteworthy that Wittgenstein explicitly discussed Robinson Crusoe* [3].

Malcolm replies in following way:

> *This would be noteworthy only if Wittgenstein had conceived of a 'Robinson Crusoe' who (unlike Defoe's invention) had never encountered other people, yet in his life-long isolation had created a language. But of course Wittgenstein did not conceive of such a Crusoe. He imagines a Crusoe who talks to himself [Here Malcolm refers to Wittgenstein's manuscript MS 165, 103]; but there is no indication that he is conceiving of anyone other than Defoe's Crusoe* [19].

Shared rules and rules as such

Malcolm directs our attention to the fact that Baker and Hacker try to make a distinction between rules of our shared language that we use every day in a linguistic community and rules of language as such, irrespective of whether the language is shared by a community or not. Baker and Hacker consider that an agreement in speech acts of different members of community is necessary for the functioning of a shared language when it is used for communication. On the basis of the agreement in the use of words, we elaborate common concepts, ways of action, meanings of expressions. Our common concept of addition was elaborated on the basis of numerous cross references to calculations of different community members, on the basis of a correlation between them in following the arithmetic rule. Nevertheless, the concept of a shared language, according to Baker and Hacker, does not exhaust the concept of language as such. There are cases of the functioning of language in which agreement in communication is not a necessary condition. The case of Robinson Crusoe by birth is one of them. This person, being isolated from other people all his life, could still logically speaking generate his own language, and follow rules on the basis of the regularity of his own linguistic acts.

Malcolm disagreed. He asserted that Wittgenstein, in that passage where he remarked on the important role of agreement in communication meant language in general, and not a shared language of some actual community. The distinction that Baker and Hacker tried to make is not relevant to Wittgenstein's thought, for Wittgenstein never makes it. Wittgenstein, in Malcolm's opinion, always talked about language as such, about conditions of the possibility of linguistic activity at all:

> *Referring to the imagined case in which people no longer agreed in their actions according to a rule, and could not come to terms with one another, he says that the upshot would be that there would be 'no language' [in this*

> *place Malcolm means the manuscript MS 165, 94 from Nachlass] —not 'no "shared" language'* [19].

Malcolm stresses this point more than once, pointing to the fallacy in the Oxford philosophers' interpretation:

> *…Baker and Hacker allow a limited importance to 'agreement', when they say that 'if there were no agreement, there would be no common concept of addition …'[Here Malcolm refers to Baker G.P., Hacker P.M.S. Wittgenstein: Rules, Grammar and Necessity, Vol. 2 of an Analytical Commentary on the Philosophical Investigations. – Oxford: Blackwell, 1985. P. 243]. An unwary reader might think they were interpreting Wittgenstein as I do. But the emphasis here should be on 'common.' Baker and Hacker think that without agreement there could be concepts but not common concepts, rules but not shared rules, language but not shared language. This is their gloss on Wittgenstein.*
> *But Wittgenstein himself does not employ these qualifications of his theme. He says, for example, that 'If there was no agreement in what we call "red," etc., etc., language would come to an end' [Here Malcolm refers to Wittgenstein L. Bermerkungenüber die Grundlagen der Mathematik, revised and expanded edition, G. E. M. Anscombe, Rush Rhees, and G. H. von Wright (eds). – Frankfurt am Main: SuhrkampVerlag, 1974. S. 196 in his own translation] — language, not 'shared' language. Quiet agreement 'belongs to the framework in which our language works' [Ibid. S. 323] — our language, not our 'shared' language. 'The phenomenon of language rests on regularity, on agreement in action' [Ibid. S. 342] — no 'shared' here. 'The phenomena of agreement and of acting according to a rule, are inter-connected' [Ibid. S. 344] — rule, not 'shared' rule* [19].

'The external relation' between rule and its application

As we have already noted, one of the important critical arguments of Baker and Hacker against the community view consisted in affirming that the requirement of community agreement in the process of following a rule presents the relation between a rule and its application as external whereas the essence of the relation consists in its being internal. Malcolm disagrees with such an interpretation of the community view and asserts that the appearance of an external relation between a rule and its application is an illusion. We may have the impression that the requirement of community agreement severs the internal connection, but this does not happen really, for a language game is organized in such way that the agreement is hidden, it is only the background of linguistic practices. This point, Malcolm insists, was stressed by Wittgenstein himself:

> *This view, as it was meant by Wittgenstein, does not presuppose that rules and what accords with them are 'externally related.' For if 'externally related' means that a general agreement is 'inserted between a rule and what accords with it,' or means that one determines whether this action accords with that rule, by canvassing the opinions of people — then of course Wittgenstein does not hold that a rule and what accords with it are 'externally related.' His position is stated concisely in Z 430 [Here Malcolm refers to Wittgenstein L. Zettel. – Oxford: Blackwell, 1967. P. 75]: our language-games of following rules in arithmetic, of colour judgments, of*

measuring, etc., etc., would not work except in the framework of general agreement — but a canvassing and testing of agreement does not enter into the actual operation of the language-games [19].

Who is right?

In this section I shall determine the replies to Malcolm's objections that the Oxford philosophers give or could give from their point of view, and present my own reflections on the question: who is right in this dispute on Crusoe?

The replies on Malcolm's objections

The first of Malcolm's critical arguments consisted in the assertion that it is not clear whether Baker and Hacker understood the distinction between Crusoe by birth and Crusoe by accident. Possibly, the Oxford philosophers did not explicitly draw this distinction but one should not hurry to draw the conclusion that if the distinction is not explicit, then it is not understood. Another conclusion is more probable, namely that Baker and Hacker considered the distinction so banal that they did not see any sense point in giving it special consideration and in their reply to Malcolm they talk about the triviality of the distinction [14].

The second of Malcolm's objections concerned the point that Baker and Hacker acknowledge the necessity of common agreement only for the functioning of shared rules and concepts which are elaborated in actual communication, while Wittgenstein always talked about language in general, about a principled possibility of linguistic activity, not only about languages of actual communities. As for this point, the Oxford philosophers acknowledge that Malcolm presented their position correctly, but they do not agree with Malcolm's interpretation of Wittgenstein. In their opinion, Malcolm, in trying to support his thesis by reference to some textual evidence, ignores the context of Wittgenstein's passages. When we take into account the context, things become clear:

> *…we denied that a social practice is logically requisite. Malcolm holds this to be mistaken and an incorrect interpretation of Wittgenstein. He cites a wide range of quotations from Wittgenstein (which we had given) and stresses that in none of them does Wittgenstein say that agreement, community consensus, and so on are presuppositions of a shared language, but says that they are presuppositions of language as such. However, Malcolm disregards the contexts of Wittgenstein's remarks, which are never to demonstrate that concepts, rules, and language presuppose community agreement, but rather that our concepts and our language does so. A few examples will make this clear.*
> *Malcolm cites the remark 'If there was not agreement in what we call "red," etc., etc, language would come to an end' (Remarks of Foundations of Mathematics, 196), stressing that Wittgenstein did not write 'shared language'. But Malcolm does not note that this elaborates on the previous remark – namely, 'In what we accept we all work the same way, but we do not make use of this identity merely to predict what people will accept'. It is therefore perspicuously a comment on a shared language. Similarly, Malcolm emphasizes that quiet agreement 'belongs to the framework in which our language works' (Remarks on Foundations of Mathematics, 323) – 'our' language, Malcolm stresses, not 'shared language.' But this is an amplification of the claim that 'Isis of the greatest importance that a dispute*

> *hardly ever arises between people about whether the color of this object is the same as the color of that,' and that 'No dispute breaks out over the question whether a proceeding was according to the rule of not.' Here it is evident that our language is (obviously) a shared language* [14].

The third Malcolm objection concerned the concept of an external relation. He asserted that a rule and its application do not stand in an external relation in the case of the community view, as Baker and Hacker suggest, if by the term 'external' we mean the necessity of actual explicit testing of correctness of a person's actions in consultation with other members of community. Malcolm insisted that this does not happen. The agreement of the community is hidden. It is an unspoken agreement.

In this case Malcolm's criticism is unsuccessful because he interprets the concept of an external relation in a quite different way than the Oxford philosophers do. Baker and Hacker did not say that the external relation consists in actual and explicit testing of correctness of our following a rule by means of correlations with actions of other people. The external relation between a rule and its application, in their opinion, arises in that case when, accepting the community view, we conceive of a rule as something independent and separate from linguistic activity of an individual.

Community as an individual epistemic subject

The point is that it is not difficult for us to present a linguistic community as a whole in the role of an individual epistemic subject, a collective Crusoe. And in this case we can see that the structure – 'the community as Crusoe' – one finds oneself in the same communicative emptiness as an isolated individual. This community cannot have any external connections with other things that might be conceived as providing criteria of correctness of its linguistics acts. The community itself turns out to be an isolated Robinson Crusoe and the interrelations between its members appear as the only criteria of correctness of linguistic acts. These inner interrelations in a community, in turn, can be considered by analogy with the interrelation between different linguistic acts of an isolated Crusoe. The single criterion of correctness of Crusoe's linguistic behavior, in turn, may be the regularity of his actions taken as a norm for his behavior. Thus we demonstrate that the community view, on more careful consideration, turns out to be analogous to an individualistic view on the nature of language. Baker and Hacker direct our attention to this matter. They say that Kripke's problem does not disappear when we adopt the community view:

> *But does this really resolve the sceptical question? Given that no one previously ever added 57 and 68, how do we know that our present community-wide inclination to answer '125' accords with what we previously meant by 'plus,' i.e. with what we would have been inclined to say, had we previously been asked what 57 + 68 is?* [2].

Nevertheless, the acknowledgment that the community view turns out to be akin to an individualistic view of the functioning of language has not any catastrophic consequences for the stability of a community's life. We have not any functional incorrectness in actions of members of a community in supporting the stability of its linguistics practices. It is enough that the community should be able to be guided by the repetition, the regularity in its actions. If we consider a community as a Crusoe and actions of different members of the community as appearances of the activity of a single isolated person, we should not think that this person will be confused because of absence of agreement between his actions and some external actions of another person. He will successfully be guided by the regularity of his own practice. If proponents of the community view would like to present some argument against to these considerations, they would have to assume the existence of a set of communities which could support each other in an external determination of the correctness of their actions. But, obviously, our individualistic argument could be applied to these communities as whole too.

Consequently, we should admit that the idea of community agreement is superfluous for the determination of the foundations of successful linguistic acts. The constancy of the meanings of linguistic expressions may be completely explained by means of the idea of regularity taken as a norm of a behaviour. But since an isolated individual is able to produce regular actions in the same way as a community, so the difference between a community and an individual regarding possibility of following rules turns out to be irrelevant. Just as a community of speakers will eliminate some non-typical interpretations of linguistic expressions because they do not correspond to the regular practice of use, so a solitary individual will be able to carry out the same semantic therapy founded on the regularity of his own actions. The idea of regularity taken as a norm turns out to be more fundamental for an explanation of following to rules than the idea of agreement and in this point Baker and Hacker are more perspicacious than their opponents.

Reconciliation of the sides

Seemingly, the ponderability of arguments ‘from Oxford’ has already inclined us to admit the possibility of a private language understood as a language spoken by a solitary person in solitude. But now Baker and Hacker produce two important remarks on the question, which alleviates the confrontation between the different sides.

First, and this point looks initially unexpected, Baker and Hacker claim that really they do not acknowledge the possibility of a private language and do not deny the social nature of human languages. And this turn of thought forces us to look at their position still more closely. The Oxford philosophers say that the public nature of language can be discovered in that when we attend to the actions of Crusoe we will be able to determinate regularities in his individual linguistic practices. This precisely demonstrates the public character of the language. We can say that the Crusoe who lives all his life in solitude, nevertheless has only an accidentally private language. He has lived, we suppose, in isolation since birth and used only his private language. However, this characteristic of his linguistic activity is not necessary. This point can be discovered not merely from the fact that he may enter into dialogue with others, but from that fact that other people may observe his uses of language and acknowledge such actions as intelligible.

The basic concepts which Baker and Hacker use to present their position are the concepts of a shared language and of a shareable language. Language need not necessarily be shared, i. e. employed in actual communication between speakers. Note that the language of a Crusoe by accident, of course, is not unshared just because the Crusoe happens be alone. The unshared language is the language of a Crusoe by birth. And, in the Oxford philosophers’ opinion, it is obviously logically possible. However, every language must be shareable. We cannot to imagine the existence of an unshareable language:

> *…disagreement between Malcolm and us turns… on whether the practice that constitutes the framework or presupposition of the existence of the rule must be a shared, community practice, or whether it may be an unshared (but shareable) one* [14].

The shareability means that it is logical possible for others to understand the language. May be the possibility will never be realized. One can imagine that Crusoe, alone from birth, will live in solitude all his life. One can even concede that he might be discovered, his activity and linguistic practice observed, and that the observers will fail to understand in his language because of limitations of time and of possibilities of observations. Attempts to translate the language may be unsuccessful but this cognitive task must be thinkable:

> *…concept-possession, following a rule, mastery of language presuppose, not that these are shared with other people, but rather that they can be shared, that it must make sense for others to understand, agree on what counts as doing the same relative to a rule, follow the rule in the same way* [14].

Baker and Hacker stress that the problem is to clarify the position of a Crusoe by birth, not to distinguish a Crusoe by accident and a Crusoe by birth:

> *…contrast Wittgenstein is concerned with is not between a shared language that can also be employed in solitude and an unshared language, but between a sharable language and a putative language that cannot in principle be understood by any other person* [14].

A Crusoe who (allegedly) speaks a logically private language is not identical with a Crusoe who speaks an accidentally private language. A Crusoe by birth could use his own language successfully if even he were isolated all his life. Nevertheless, it is sheer accident that his language is not shared with others, for it is thinkable that if his linguistic practices were observed, then the observers might recognize regularities that are treated by Crusoe as standard setting uniformities and behaviour of intentionally following rules. A Crusoe who spoke a supposedly logically private language would be quite different. We would never recognize in his linguistic acts any regularities. But that just means that regularities in his linguistic activity are altogether absent. Baker and Hacker, we can say, assert the necessary character of a conditional: if there are regularities in a language, then they can in principle be discovered. Accordingly, by *modus tollens,* one can conclude: if regularities cannot in principle be disclosed, then there are none. But if in a linguistic practice regularities of using of signs are altogether absent then that means that language as such is absent too. There is just a chaotic conglomeration of phonemes which are not subject to any rules. It seems that is the reason why Baker and Hacker call this 'a putative language.' This sound conglomeration would merely resemble a language but it would not be one. Such a Crusoe would not be able to speak, for an absolutely (logically) private language is impossible.

Second, Baker and Hacker say that actually a human being's linguistic practice in almost every case is social. Malcolm's arguments will be correct in the overwhelming majority of actual speech acts. For themselves, the Oxford philosophers keep in reserve an analysis of *the concept* of language, not *the phenomena* of language:

> *Indeed, one may concede that the phenomenon of language is a phenomenon of shared practices. For no one is arguing that as a matter of fact there are language-using wolf-children, or that some beings are actually born with an innate mastery of a language, The question is whether the concept of language presupposes a community of speakers and shared practices* [14].

Focusing on the analysis of concepts, not on empirical facts, on logical, not empirical researches characterizes philosophical considerations, in contrast to researches of empirical sciences. And in this respect, the methodical aims of the Oxford philosophers earmark their researches as philosophical. One can say that in this discussion, Baker and Hacker conduct themselves as philosophers, while Malcolm and others who share the community view, rather, look akin to linguists. They draw quite correct conclusions regarding actual languages, whereas Baker and Hacker discuss questions which are connected with the *logical possibility* of language as such.

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ON SUBSTITUTIVITY OF PROPER NAMES IN PROPOSITIONAL ATTITUDE CONTEXTS

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Abstract:
The main goal of the article is to argue for admissibility of substitution of coreferential proper names in belief contexts which generally has not been allowed in the logico-semantic tradition that dates back to the works of G.Frege. The main object of criticism is a so-called principle of disquotation, as formulated by S.Kripke, which conditions the prohibition of the aforementioned substitution and which is accepted within the Fregean tradition. Some related issues associated with the works of R.Carnap, W.V.O.Quine, and N.Salmon are addressed.

Two Tenets of Traditional Semantics

The tradition in philosophy of language that goes back to Gottlob Frege has attached much importance to Leibniz's law according to which expressions which have the same meaning are interchangeable in all contexts *salva veritate* (i.e. without a change in truth value) and to the distinction between the so-called extensional and intensional contexts. The latter contexts are divided into modal ones and those of propositional attitudes. The distinction has to do with issues concerning substitutivity of expressions and ultimately with their meaning. It has been believed that while all coextensive expressions (i.e. expressions that refer to the same entities) are interchangeable in extensional contexts, not all of them are interchangeable in the intensional ones. It has also been believed that the meaning of an expression consists of an extension (denotation, reference) and an intension (sense). And the latter has been considered as a mode of presentation of the former.

The works of Ruth Barkan Marcus, Saul Kripke and some other theorists in late 1960's and early 1970's provided strong arguments in favor of a different approach to the analysis of proper names. It has been argued that they refer to their objects directly as tags or rigid designators and not via sense as a mode of presentation of reference. Kripke showed how proper names designating the same object could be interchangeable in modal contexts *salva veritate*. This approach was extended to some other expressions, such as natural kind terms, demonstratives and indexicals. But Kripke and many of his followers still argued against interchangeability of expressions (proper names as well as other expressions) with the same meaning in the contexts of propositional attitudes. Thus in what concerns these contexts the tradition persisted.

To illustrate the traditional approach to these contexts we can consider a sentence

(1) Cicero is a great Roman orator.

According to the traditional approach (1) represents an extensional context and the name “Cicero” can be substituted for the name “Tully” (so long as it denotes the same individual) without a change in the truth value of the original sentence to produce

(2) Tully is a great Roman orator.

However such a substitution is not allowed in a context of a propositional attitude. In other words it is not permitted to transfer from a sentence like

(3) Jones believes that Cicero is a great Roman orator

to a sentence like

(4) Jones believes that Tully was a great Roman orator

since it has been argued that such a transition (unlike that from (1) to (2)) does not preserve the truth value of the original sentence. And if the truth value of (4) can be different from that of (3) then by Leibniz’s law this result precipitates in a conclusion that the meaning of the terms “Cicero” and “Tully” has to be different notwithstanding the sameness of their reference.

It was this kind of analysis that led Frege to introduce along with a notion of reference a notion of sense of an expression as a second part of an expression’s meaning. According to Frege a difference in sense is what distinguishes the so-called informative identities[1] (such as “Cicero = Tully”) from tautologies (such as “Cicero = Cicero”).

Thus a tradition was set up according to which informativeness (or cognitive significance) of identities was considered an indication of (i) nonsubstitutivity of the corresponding expressions in propositional attitude contexts and hence of (ii) a difference in their meaning.

In what follows I will try to argue against these two tenets and show why I believe cognitive significance (whatever it is) has nothing to do with the issues of either substitutivity of expressions in propositional attitude contexts or their meaning to the extent that the discipline of semantics is concerned.

But before I begin I would like to make some necessary clarifications. In my argument I will make a case only for proper names and not for sentences or any other kinds of expressions. Thus the very expression “propositional attitude contexts” introduced by Bertrand Russell[2] and significantly popularized by W.V.O. Quine[3] seems not very appropriate for my purposes since it gives an impression of a need for an analysis of propositions which are most often expressed by sentences. And proper names do not express or denote propositions. At best they can be associated with constituent parts of propositions. So may be it would have been better for me to follow Rudolf Carnap and call these contexts “psychological.”[4] But I will not do that because most of the other theorists that I will cite also refer to these contexts as those of propositional attitudes and discuss them in terms of propositions. And an unnecessary confusion of terminology is something that I would like to avoid here. Moreover whatever I will say about the semantics of proper names in these contexts ultimately does not seem to preclude their analysis in terms of propositions. So my only adjustment in this respect will have to do with the way I will represent propositions that follow after a “that” clause. When most theorists present them simply as “p” I will in addition present them as “Pa” (where “a” will stand for a proper name and “P” for a corresponding predicate) in order to stress my focus on the proper name used.

The Traditional Argument

It has already been mentioned that the tradition makes extensive use of Leibniz's law. As a matter of fact this law lays the foundation of an often used argument that stresses the difference between tautologies and informative identities in what concerns the theory of meaning. This argument can be formulated in the following way:

(A1)
If two proper names *a* and *b* have the same meaning, then it cannot be that "*S* believes that *Pa*" is true and "S believes that *Pb*" is false at the same time.
But as a matter of fact "*S* believes that *Pa*" can be true while "S believes that *Pb*" being false (even if "*a*" and "*b*" denote the same individual object).
Therefore, proper names *a* and *b* do not have the same meaning.

It seems counterintuitive to disagree with Leibniz's law. However this fact by itself does not yet make the whole argument in (A1) nonproblematic since there is the second premise which I believe is the root of all relevant evil. Thus I would like to focus attention on it. And the question that I would like to address here has to do with the reasons why the tradition considers it true (which it does, otherwise the argument is not valid).

The Principle of Disquotation

The answer to this question that is often given (or at least implied) refers to the fact that S, while accepting (assenting to) sentence "*Pa*" might not accept sentence "*Pb*". This in its turn is considered a reason to conclude that S believes that *Pa* and does not believe that *Pb*. The basis for this reasoning is presented in a so-called principle of disquotation which has been implicitly used by many adherents of the tradition but to my knowledge was first explicitly formulated by Saul Kripke.[5] In its rough form this principle states

(DP1) If a person accepts "Pa", then he or she believes that Pa.

This principle can be formulated in a stronger form representing a biconditional instead of a conditional:

(DP2) A person accepts "Pa" if and only if he or she believes that Pa.

It is this principle that the tradition uses in order to justify the truth of the second premise in (A1) and to show that the meaning of "a" and "b" cannot be the same notwithstanding their sameness of denotation.
If we take this principle just at its face value it seems to me quite easy to show that it is false. Consider an example of Frank who accepts "Cicero is Roman" and believes that Cicero is Roman. Now if "Cicero" denoted Plato, then Frank would not have accepted "Cicero is Roman" but he would still believe that Cicero is Roman. On the other hand, if "Cicero" denoted Plato and "Roman" meant Greek, then Frank could accept "Cicero is Roman". But of course in such a case his accepting it would not entail his belief that Cicero is Roman. Frank could be ignorant of the history of the Roman Empire, not know anything about Cicero and not believe that he was Roman. And this is what I would consider a refutation of the principle of disquotation in its rough form.

However the rough form is not exactly the way this principle was presented by Kripke and accepted by many others. In his formulation of this principle Kripke makes a reservation that we are dealing with a normal, sincere and reflective speaker who assents to "p". Kripke formulates the principle in its conditional and biconditional forms in the following way: "*If a normal English speaker, on reflection, sincerely assents to 'p', then he believes that p"* and *"A normal English speaker who is not reticent will be disposed to sincere reflective assent to 'p' if and only if he believes that p"*[6] and calls it "a self-evident truth". In such a form the principle of disquotation seems to avoid the counterexamples mentioned above. But this fact by itself, as I will try to show, does not relieve the principle from its problems.

In order to demonstrate what I think is wrong with this refined version of the principle I will need to analyze Kripke's aforementioned reservations. But before this can be done it seems that the notions of normality and reflexivity used by Kripke need to be clarified in the sense that is relevant for the semantic issues that are being considered here. Normality is a normative concept and reflexivity is psychological. What could be their equivalents that would limit their relevance only to the field of semantics and probably to the related epistemological issues?

It seems quite natural to pick out as alternative candidates for "normal and reflective" the terms "competent and understanding the sentence". First of all such a substitution seems to be suitable because when we are dealing with a speaker who accepts some sentence "p" our characterizing him as normal and reflective cannot exclude his competence in the language and his understanding of the relevant sentence. It can however sometimes presuppose some other characteristics. But at the same time in most cases competence and understanding seem to be quite sufficient on their own to describe a speaker as normal and reflective in such a situation.

This choice also seems to be supported by other theorists (those who are in agreement with Kripke's principle as well as those who are critical of it). Nathan Salmon who supports Kripke on the issue of self-evidence of the principle writes: "*Sincere, reflective, outward assent (qua speech act) to a fully understood sentence is an overt manifestation of sincere, reflective, inward assent (qua cognitive disposition or attitude) to a fully grasped proposition*".[7] Scott Soames who rejects the principle talks of it as follows: "*Sentences S_1 and S_2 may mean the same thing, and express the same proposition p, even though a competent speaker who understands both sentences, associates them with p, and knows of each that it expresses p, does not realize that they express the same proposition*".[8]

So I will use the terms "competent" and "understanding" as alternatives for Kripkean "normal" and "reflective" and reformulate the principle of disquotation (in its biconditional form) in the following way:

> (DP3) A competent English speaker who fully understands "Pa" and is not reticent will be disposed to sincere assent to "Pa" if and only if he believes that Pa.

If I am right then the problem with the principle of disquotation becomes vivid. A *competent speaker* in this case is the one who is aware that "Pa" means Pa when it really does mean it. And a *speaker who fully understands* "Pa" is the one who grasps the proposition that Pa, and only it, when he or she encounters "Pa". "Believing that Pa" in its turn also means grasping that Pa and accepting that Pa (Salmon, for example, calls *traditional* the conception of belief as inward assent to a proposition[9]). Thus a reformulation of Kripke's version of the principle will yield:

> (DP4) An English speaker who is aware that "Pa" means Pa when it really means it, who grasps the proposition that Pa, and only it, when he or she encounters "Pa", and who is not reticent will be disposed to sincere assent to "Pa" if and only if he or she grasps the proposition that Pa and accepts that Pa.

But this seems somewhat trivial (if not tautological) given that “Pa” means Pa and that the speaker is aware of it. In other words when it is stated in our premises that “Pa” is always to be associated with Pa, and only with it, then the conclusion does follow by definition: a speaker cannot assent to “Pa” without simultaneously accepting that Pa. However it has been shown in the examples of Frank above that “Pa” need not always be associated with Pa and, consequently, accepting “Pa” need not always entail accepting that Pa. Such a narrowing down of the scope of possible meanings of “Pa” only to Pa and considering these cases exclusively when discussing assent to sentences and belief in propositions not only remains unjustified but also very much resembles the logical fallacy of accident (*a dicto simpliciter ad dictum secundum quid*).

Thus if my argument above is valid then the principle of disquotation in its rough form is simply false and in its revised form it becomes tautological and based on an unjustified and, in fact, false statement. Neither of these two variants seems satisfactory.

Some theorists however (notably Salmon) argue against denying the principle of disquotation because of its alleged self-evidence and the fact that we cannot successfully construe having a belief in the terms other than those of having some disposition (such as that of an assent) to a corresponding proposition. However being disposed in such-and-such a way to a proposition is not the same as being disposed in the same way to a sentence. It has been shown above that the former can obtain without the latter and vice versa.

Thus it seems that we need to agree that sentences

S accepts “Pa”

and

S accepts that Pa

talk about different things and notwithstanding their frequent match in truth value ultimately have different conditions of truth. This is the reason why we can deny the principle of disquotation without denying the so-called traditional approach to explaining belief that Pa in terms of having some disposition towards the proposition that Pa.

Validity of Substitution

If the above is valid then we can look at premise 2 from (A1) from a different perspective. Let’s consider again

(3) Jones believes that Cicero is a great Roman orator.

We can ask a question: if this sentence is true (and John in fact does have a *de re* belief about Cicero that he is a great Roman orator) then what can prevent us from substituting “Cicero” for “Tully” and stating that

(4) Jones believes that Tully was a great Roman orator

preserves the truth value of (3) and thus is also true?

Now, the traditional answer about Jones’ not accepting

(2) Tully is a great Roman orator

will not do the trick since it is based on the principle of disquotation that has been rejected. Another possible answer can consist in appealing to Jones' denying (4) (which is not an impossible situation). However in this case we need not forget that Jones' accepting or denying (4) is not a condition of its truth or falsity just like anyone's agreeing or disagreeing with some sentence does not make it true or false.

It seems that the only valid truth condition for (4) is Jones' possession of a corresponding attitude of *de re* belief about the concrete individual. But as a matter of fact it turns out that possession of this same *de re* attitude is the reason why (3) has its truth value to begin with. And since we are given that (3) is true it means that this state of affairs does obtain. Hence (4) should be true just in the same way as (3) and a transition from (3) to (4) should preserve the original truth value. This means that the terms "Cicero" and "Tully" can in fact be interchangeable in the contexts of propositional attitudes and that tenet (i) is not correct.

Two morals can be drawn from the above. The first one is that we need not always consult Jones in order to make a transition from (3) to (4) and in other similar cases. And our conclusion here will state that even if "Cicero = Tully" is an informative identity (for Jones or someone else), this fact by itself has nothing to do with the question of substitutivity (interchangeability) of these two terms. The second moral states that we need to distinguish between cases of direct and indirect speech when discussing the issues of reference. Interchangeability of terms like "Cicero" and "Tully" is not permitted in cases of direct speech when these terms are rather mentioned than used. Thus a transition from (3′) to (4′) is never permitted:

(3′) Jones says: "Cicero is a great Roman orator"

(4′) Jones says: "Tully is a great Roman orator".

But cases of direct speech have never by themselves raised any of such problems.

Sameness of Meaning and Quine's Obstacle

After what has been said about the problems with tenet (i) it might be tempting to use Leibniz's law to show that if "Cicero" and "Tully" are interchangeable in all contexts including those of belief ascription then both terms do have the same meaning. However an automatic transition from an argument against tenet (i) to an argument against tenet (ii) with a guaranteed successful result would seem to be somewhat hasty.

The reason for this lies in the fact that independence of the question of interchangeability of expressions from the question of a subject's attitude or disposition towards a corresponding sentence does not by itself make terms like "Cicero" and "Tully" synonymous. In other words in the above examples I only *assumed* that "Cicero" and "Tully" have the same meaning (semantic content) and showed that a subject's attitude to the corresponding sentences should not be viewed as a determinant of the meaning of these terms. But if "Cicero" and "Tully" are not synonyms then a transition from (3) to (4) will not be permitted no matter whether the bearer of the corresponding propositional attitude accepts the corresponding sentences or not. And there is a famous argument by Quine against the notion of synonymy which entails that all informative identities represent synthetic and not analytic truths. In our case this would mean not only that the chosen example of "Cicero" and "Tully" would be a bad one but also that no such example can be found in principle. So before rejecting tenet (ii) an account of Quine's argument against synonymy must be given.

The first thing that must be said in respect of Quine's argument is that it is not really an argument *against* the notion of synonymy. Never in his "Two Dogmas" does Quine say that synonyms do not exist. He merely states that the notion of synonymy is a vague one.[10] In our case this can mean not that there can be no synonyms in principle but that it is really hard to think of an example of two synonymous expressions which would not clash with Quine's criticism of the analytic/synthetic distinction.

If so then we should not treat Quine's argument as implying that interchangeability in belief contexts is never possible. Moreover in our language practice we do have synonyms and we do substitute them in belief contexts. An example of such a use, for example, can be provided by chapter 1 of the Constitution of Russia which in paragraph 2 states that the names "Russia" and "The Russian Federation" mean the same. In other words it is implied that in any sentence in which either of the terms is used (and not merely mentioned) a substitution of one for the other will not result in a change of truth value.

One might say that in this case we are dealing with an example of a postulated synonymy and that such cases are different from those discussed by Quine in his article. But in fact it seems hard to see why the often occurring cases of postulated synonymy should not be considered as cases of synonymy.[11] And the fact that in different idiolects different pairs of expressions are regarded as synonymous should not be considered as problematic since when we are dealing with matters of semantic content of expressions and the truth values of the sentences that contain them we cannot do otherwise but stay within the borders of one chosen idiolect and treat as synonymous only those pairs of expressions that satisfy its corresponding requirements. We can disregard the fact that in some other idiolects the set of pairs of synonyms may not coincide with that in our idiolect. Matters of interidiolectical translation should rather be viewed as matters of a theory of communication and not of a theory of meaning.

So returning to the discussion of tenet (ii) we can admit that there can exist expressions which have the same meaning and at the same time form informative identities. And, again, our language practice shows that such expressions not only can exist but that they do exist. And the fact that we can never agree on one set of synonyms once and for all does not mean that synonymy should not be a notion employed within the discipline of semantics as well as within philosophy of language in general. Thus a refusal to address the issue of interchangeability of expressions in the so-called "opaque" contexts (another name for propositional attitude contexts proposed by Quine) cannot be seen as an appropriate strategy for a semantic theorist.

The Object of Semantic Analysis

The critique that has been formulated above presupposes a semantic theory that is different in many respects from that which is associated with the names of Frege and Russell. A characteristic distinction of this theory consists in the fact that it treats neither epistemological factors (such as cognitive significance) nor psychological (as a subject's mental dispositions to sentences) as elements which determine the meaning of linguistic expressions. This semantic theory rests on an understanding of semantics as a discipline which is focused on a study of designation (a relation between signs and corresponding entities).[12] Such a theory would not consider as objects of semantic analysis the matters of syntax, morphology, pragmatics, communication and many other questions that rise in some of the related disciplines.

Thus such a theory would treat as irrelevant the question which, according to Kripke, generates a paradox which every theory of belief and names is allegedly supposed to address. In connection with his famous example of Pierre Kripke formulates this question in the following way: "Does Pierre, or does he not, believe that London is pretty?".[13]

A semantic theory (and especially a theory of naming) according to the approach indirectly supported here is not oriented on answering questions like this one, i.e. questions about beliefs. A semanticist cannot say what is on Pierre's (or someone else's) mind, she cannot say what his psychological dispositions are, etc. What a semanticist can say in a case like that of Pierre at best consists in something like the following: if two terms “London” and “Londres” mean the same and “Pierre believes that London is pretty” is true then the two terms are interchangeable in all contexts *salva veritate*. Expecting from a semantic theory that it answers what Pierre really believes or which concrete terms are synonyms is just as hopeless and unreasonable as expecting from a theory of logic that it answers which atomic sentences are in fact true.

Concluding Remarks

The ideas discussed in this text are not new. The question of a possibility of substitution of terms in belief contexts had already been raised by Carnap in his *Meaning and Necessity*. An important contribution was made by David Kaplan.[14] And coherent formulation of a theory similar to the one I have been advocating here has been offered by Nathan Salmon.[15] However for Carnap the question of a subject’s assent to (or dissent from) a certain sentence was an important factor guiding him in his formulation of the theory, specifically in his introduction of the notion of intensional isomorphism. Based on similar considerations Kaplan introduced his ternary relation of representation along with the notions of vividness and *of*-ness for names. Salmon also stresses the importance of “the way in which the subject takes the object”.[16] He calls this way a third *relatum* for a theory of belief but specifies that it is “entirely separable from the semantic nature of the relevant sentence”.[17] If so then the main goal behind the argument of this paper is to show that an account of reference and a semantic theory in general can and should do without mentioning this third *relatum*.

I have tried to make a case only for proper names which I viewed as linguistic tags of objects. However similar considerations can be extended to an analysis of other singular and general terms (indexicals, demonstratives, descriptive names, definite descriptions, natural kind terms, mass terms, vague terms, etc.). Hopefully further research can show which specific characteristics such an extended semantic theory will have.

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6.Kripke, S. Op. Cit. Pp. 885, 886.
7.Salmon, Nathan. Frege's Puzzle. Cambridge: MIT Press. 1986. P. 130.
8.Soames, Scott. Reference and Description: the Case Against Two-Dimensionalism. Princeton, 2005. P. 79.
9.Salmon, Nathan. Op. Cit. P. 130.
10.In this way Quine writes: "Just what it means to affirm synonymy, just what the interconnections may be which are necessary and sufficient in order that two linguistic forms be properly describable as synonymous, is far from clear; but, whatever these interconnections may be, ordinarily they are grounded in usage". "Two Dogmas of Empiricism" in *From a Logical Point of View.* Harvard University Press, 1961. Pp. 24-25.
11.Cf. Rudolf Carnap's "Meaning Postulates" (*Philosophical Studies*. 1952 3: 65-73) as a reply to Quine's criticism.
12.In many of its respects such an understanding goes back to the works of Charles Morris.
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PRACTICAL LOGIC FOR ECONOMISTS AND ALL USERS OF TABLES

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Abstract:
In this article I represent in the form of the formalized system that fragment of logic of a natural language which from an antiquity is intuitively used by economists at drawing up of tables which carry an imagery of relations of sets. For this representation I develop existential linearly-tabular diagrams. These diagrams are graphically reduced form of record of the logic information of statistical tables. The fragment of the diagrammatic dictionary of logic forms of attributive propositions is shown. (This full dictionary contains 148 diagrams). The algorithm of a diagrammatic method of drawing and checking of all possible conclusions from *n* any such propositions-premises with compound positive and negative terms is given. Free (consciously controllable) mastering of *all* logic natural language's means is necessary for optimal performance of economic thinking. Logical culture of natural language should be high enough for any scientist, economist, lawyer and simply a businessman.

Logical modes of thinking and inference ability are necessary conditions of rational use of resources, first of all, in the field of economics.

The economic scientific thinking, as well as scientific thinking in general, is impossible without practical logic of tables. The accounting tables were under construction on the papyrus in Ancient Egypt; the editor of tables is one of the most used programs in a computer. Nevertheless, people, acquiring this or that economic profession, don't study Logic as special discipline (as a majority of scientists, which work in other fields of science). What does logic ensuring of economic thinking and economic science represent itself? Without special study of Logic it can be only some realized intuitive logic. And, in this case, it's practical logic. For theory i.e. for accuracy of theoretical inferences, proofs and explanations, for logic systematization of theoretical concepts and propositions deductive logic is necessary. It is the most important section of Logic as a whole. As the result there is a question: what deductive practical logic of economic consciousness should be? If economic faculties included the standard courses of Logic in the curriculum, in these conditions the basic place in them would be occupied by traditional "philosophical" and classical mathematical logic (propositional logic and predicate logic). However, the classical propositional logic and predicate logic is unacceptable as real practical logic even in Mathematics, in view of paradoxes of material implication, i.e. such formulas as $A \rightarrow (B \rightarrow A)$, $A \,\&\, \neg A \rightarrow B$ and others [4]. According to these paradoxical formulas, the true proposition can be proved by any proposition, and the false proposition can be a sufficient reason for any proposition. To recognize these formulas as the laws of logic would mean firstly, a recognition of arguments of the any propositions as competent, irrespective of whether they're pertinent to and whether they're true, and secondly, acknowledgement of competence of any propositions as consequences from the obviously false propositions. If the first fact could bring to arbitrariness in argument, the second, on account of the responsibility for consequences, could bring to some arbitrariness in definition of consequences, which there comes the responsibility for.

Is intuitive, correct use of natural language's logic enough in expression of scientific economic thinking? In a principle it must be insufficient. The science proposes universal recognition of the truth and checkability of evidences. If the accuracy of logic proof didn't become clear for others, also thesis of the proof can't be considered proved, even if the proof is constructed correctly on intuition. Besides, the intuitive logic doesn't relieve of mistakes, which could be eliminated at the conscious control. Here of follows, that for economic consciousness is useful not only intuitive, but also realized practical logic, i.e. system of the scientifically developed means and methods of the conscious logic control of reasoning.

The subject should draw correct conclusions from general scientific positions to follow the scientific recommendations consciously. The practical logic, which serves as basis for it, is a system of rules, norms, and in case of need it is possible to get a new true knowledge from initial veritable knowledge using them. The fact is to realize and to improve practical logic, used for clear perception of economic relations.

The scientifically realized practical logic is system of control over expression of ideas and purposeful formation of thinking logicality and its general basic principle is accordance of logic knowledge to users' interests. Derivative principles are the following: interrelation between verbal component of thinking, images and practical actions; the most full mastering of logic forms of concepts, propositions, inferences and language expression of these forms; original, not imparted to the machine with the purpose of intuitive logicality forming, accomplishment of logic operations.

The principle of accordance to consumers' interests demands a choice of optimum means and methods, which give possibility to attain the certain result with the minimal expenses and to attain the maximum of results with the certain expenses. This principle is recognition that extreme principles work in economic activity as a whole and in its mental component in particular. For any mass logical economic thinking the realized logic, which is extremely relieved of unnecessary complications, is extremely approached to intuitively used logic.

In the first place, the practical logic of economic thinking is natural language's logic. Firstly, people use this language in comprehension of the economic reality and when they carry on business negotiations. Secondly, natural language is a language of economic science.

Free (consciously controllable) mastering of *all* logic natural language's means is necessary for optimal performance of economic thinking. But it doesn't exclude, of cause, that an artificial language can be used in addition to this in an economic science, for example, in sphere of mathematical modelling. Therefore, the practical logic of economic thinking can't be limited just by mastering of those forms of attributive propositions, which traditional logic courses offer.

In these courses traditional syllogistics is the closest discipline to logic of natural language. It considers just universal affirmative (***A***), particular affirmative (***I***), universal negative (*E*) and particular negative (***O***) propositions about properties and it establishes rules of formal inference only for them. In these courses the presence of allocating and excluding propositions is admitted, in spite of syllogistics. The traditional logic courses don't still instruct to supervise the information, transmitted by *all* logic forms of natural language's propositions.

It goes without saying, that intuitive thinking uses all natural language's logic means worse or better, but their development, use and connection with the certain meanings mostly are spontaneous and they aren't subject to the conscious control. There are 304 forms of attributive propositions about subjects only in the dictionary, developed by the author. Also the similar dictionaries of the forms of propositions about cases, places, times, points of view are offered. In these dictionaries the existential linear-tabular diagrams (ELTD), which give the information about existence or non-existence of elements with some attributes, are given.

It is not enough just to establish a fact that the classical logic doesn't observe to "intuition of logic consequence". It is necessary to clear up, what intuition is based on, what is considered to be such intuition, if the accuracy of inference or substantiation can be consciously proved to other people and, in turn, be realized by them, to rely on this intuition. And it's in condition that neither scientific economist nor, moreover, business partners for the most part didn't study any syllogistics or, and what is more, any symbolical logic. Nevertheless, opportunity of such proof in such condition exists. But this opportunity isn't created with presence of traditional syllogistics' rules or with methods of symbolical logic. It's created with people's ability to imagine pictorially, what actually speech is about, what information about the object is reported and, accordingly, what information can be taken from this not formalized basis in the consequence. On this basis Johnson-Lard, the representative of cognitive Psychology, opposed a semantic method to Logic [1]. However, this method, according to its subject, as a method of the correct construction of argumentations, is the logic method, and namely it's method of pictorial practical logic (method of pictorial logic semantics). Without doubt, it isn't a method only of the symbolical transformation of ones propositions in others, but it's a method of logic processing of the information, transmitted by image of meanings of these logic propositional forms. Johnson- Laird writes, that there is alternative theory, which is much easier, than the theory, offered by Newell, ("the theory based on the same general lines, which depends on the close relation between the Venn diagrams and the table of truth") and gives an example of application of this theory [1]. The quoted words in themselves specify a connection between his method and logic methods; and the example, resulted by him, reveals analogy of this method and the method of ELTD, offered us.

In offered pictorial practical logic [7], [8], [9] the conclusions are made on the basis of the direct account of the transmitted information; but it doesn't mean at all, that the rules of conclusion aren't used in them. These rules were written down symbolically by L. Carroll, and it doesn't contrary to the fact that people use these rules, but not formulas of them. The symbolical expressions in pictorial practical logic have only those meanings, which the appropriate images give them. Such image can be a perception of reality, a qualitatively similar representation about it, and a qualitatively dissimilar diagrammatic image of the relations between sets. The economic consciousness is connected with events in macrocosm. They are direct appeared in macro forms or, at least, they indirectly contact with sensual perception of their conditions, reasons and sequels. Otherwise, they can't be neither proved nor regulated.

The economic consciousness is called up to adjust such activity, where an image of desirable result and image of action leading to this result are necessary. In this activity the symbols should be connected with images.

Law of sufficient logical reason should be observed in economic thinking: just information, presented in reason, can be taken in consequence. Such understanding of the law of sufficient logical reason can be expressed by formula: $A \leftarrow A \vee (A \wedge B)$, where A and B – information, irrespective of the fact which propositional forms it is transferred by. The formula means: consequence with information A follows from the reason, which contains just the same or the same and additional information. All what is supposed by paradoxes of true and false propositions in classical logic is forbidden by such understanding of the law of sufficient logical reason; it is forbidden to see the logical reason in those propositions, which don't carry information contained in thesis, subjected to substantiation; it is forbidden to make any conclusions from false information, given by some subject; but it is allowed to allocate false information for refutation. The practical logic doesn't substitute the conjunction "If..., then...." by material implication, but namely this substitution conducts to paradoxes.

Sentences of some text by themselves at all don't contain information about object, which is real logical reason for conclusions. These sentences are informative propositions (i.e. propositions about reality) if only they due an image of valid object or indirect displays of its existence in details or as a whole. But also the images are true, merely if there is only that information in them (form liable to reflection), which is in reflected. It means, in the end, that only perception of object's details, kept as pictorial idea (besides, perception common for different subjects) is the original final logical reason for conclusion.

The transformation of the information into commodity value demands an avoidance of losses and distortion of information, transmitted by natural language's logic means. For achievement of unequivocal understanding of these means by different people in different conditions a proved normative pictorial definition of these means' meanings is necessary.

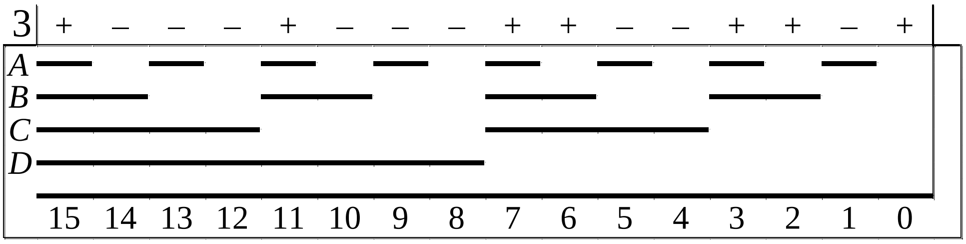
The interpretations, given to propositions (***A, E, I, O***) in the majority of formalized symbolical syllogistics, are different and mostly obviously artificial. As a result of adjustment under conformity to such artificial figure, as logic square there are such interpretations in language of predicate logic, which include material Implication. With such interpretation a simple categorical proposition ceases to be categorical, turns to complex conditional proposition or in proposition with logic "or" and it ceases to carry certain information about discussing case. For example, «Some goods (S) have no demand (P) ", actually, it is supposed to interpret so: «There are no goods (S) or there are goods which haven't a demand (*S* not-*P*)" {Writing down in the language of predicate logic: $\exists x\, S(x) \supset \exists x\, (S(x)\ \&\ \neg P(x))$, what's equivalent of $\neg\exists x\, S(x) \vee \exists x(S(x)\ \&\ \neg P(x)$ [6]}. Hardly any economist will consider it as the categorical proposition about presence or absence of goods. But what interpretation is not artificial? How other propositional forms must be interpreted? There is a need to answer these questions using sociolinguistic researches, because the problem at issue is a natural language as a mean of *mass* intercourse. It is possible to learn about words' meanings got owing to historical spontaneous process, only from people. To attribute to the words artificial meanings (by virtue of gnoseology or other reasons) means transformation of natural language in more or less artificial one.

It's hardly effective to impose the artificial meanings of words to mass of people. A validity criterion of ***A***, ***E***, ***I***, ***O*** and many other interpretations of propositional forms should be not arbitrary formation of syllogistic systems, even if they satisfy to these or those criteria of Symbolical logic, but the reference, firstly, to the practice of mass dialogue in natural language, and, secondly, to that level of thinking, on which illogicality is corrected by impracticability of actions, appropriate to wrong interpretation. At such reference the rules of logic act as component of symbol-representational models, appropriate to sensual experience and validity. Conformity to validity, as it is given in practical experience, becomes thus criterion of validity of logic constructions.

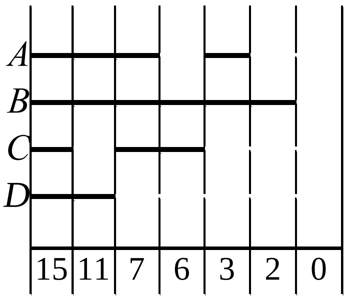
To find out meanings attached to logic means of natural language, the technique of research with using of questionnaire was developed and applied by author [9]. In this questionnaire the logic means of language correspond with all probable (allowable) variants of meanings, appropriate or inappropriate to these means, i.e. sentences with these means. Form of accordance of these meanings given in this questionnaire doesn't demand any special training. A respondent should answer what combinations of presence or absence of attributes correspond to the specified sentence, and what combinations don't correspond.

Reveal of character of relations between sets of discussed elements correspond to some language expression of logic proposition form, allows to find out, what information this expression carries, and to transfer this information with one existential linear-tabular diagram. (It's required more than one of Euler diagrams for demonstration of all opportunities corresponding to this or that language expression of the logic proposition form). Euler diagrams mostly are offered in textbooks, but using of tables as logic diagrams is considerably more productive. Any economist and overwhelming majority of population are accustomed to use and understanding of tables. However, construction of both Euler diagram and table is a result of some kind of precomputation. For such ELTD are optimal (see: the diagram 1 in the following complete set). It's possible to have a preparation of such diagram's linear part in electronic variant or in the form of logic rule. The sample of the diagrams of relations between concepts is given below: *A* – money; *B* – value; *C* – paper product; *D* – rouble.

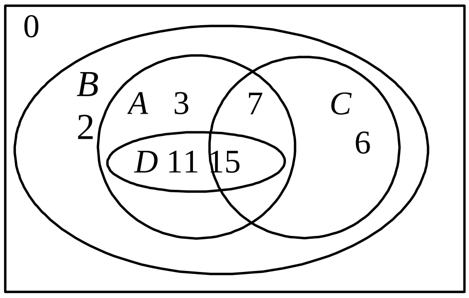
1.



2.



3.



4.

All discussed						
B						*not-B*
A				*not- A*		*not-A*
C		*not-C*		*C*	*not-C*	*not-C*
D	*not-D*	*D*	*not-D*	*not-D*	*not-D*	*not-D*
15	7	11	3	6	2	0

5.

All discussed						
B						*not-B*
A				*not- A*		
C		*not-C*		*C*	*not-C*	
D	*not-D*	*D*	*not-D*			
15	7	11	3	6	2	0

Fig. 1.

Diagrams in this complete set on Fig.1 are: 1 – ELTD, linear-tabular diagram with image of obviously empty sets (completed with a logic ruler). {With the ruler the diagram is more convenient to draw on a clean sheet. The scale if this logical ruler is like a logical Marquand rectangular drawings which S. Lushchevska-Romanova improved and T. Kotarbinsky used [2] and [3]}; 2 – linear-tabular diagram without the image of obviously empty sets; 3 – Euler diagram complemented with image of all discussed (universe), i.e. with rectangle; 4 – obvious combination of the linear diagram and table; 5 – table. Usual, but basically not obligatory, difference between registration table and linear diagram of existence is: on the diagram not numbers are put, but marks of existence (for example, "+" instead of nonzero number, or "-" instead of zero).

The definition of common acceptable meanings of a language's logic means allows concluding, with a higher degree of probability, that the certain form of proposition carries such-and-such information. The fragment of the diagram dictionary of logic propositional forms about discussed subjects looks with use of ELTD like this:

B		*not-B*		
A	*not-A*	*A*	*not-A*	
+				There is *A B*. Some *A* is *B*. Some *B* is *A*.
+	+			Not only *A* (*not-A*) is *B*. Not each (every, all) *B* is *A* (not-*A*).
–				There isn't *A B*. No *A* is *B*. No *B* is *A*.
–		–		There isn't *A*. There isn't *A B* and there isn't *A* not-*B*.
+		–		Each (every, all...) *A* is *B*. // Only *B* is *A*.
+	–	–		Only each *A* is *B*. By definition, *A* is *B*.
–	+	+	–	Each, except *A*, is *B*. Each, except *not-A*, is *not-B*.
.		.		There is *A* (*B* or *not-B*).

Fig. 2.

Diagrammatic dictionaries of forms of propositional about cases, places (loci), times and points of view have an analogous kind. In such forms terms are the propositions (for example: "Always, when all, except *A*, is *B*, then any *C* is not *A*".). In representational construction of logic of natural language the proposition "If *A*, (then) *B*" ("In a case if *A*, (then) *B*") is interpreted as equivalent to proposition "there are no cases in which there is *A*, but there isn't *B*". It eliminates paradoxes of implication.

The diagrams of meanings of separate propositions and information of these diagrams (tables) can be combined in one diagram (table) of the reason. Such combination can make the new information. For a wide class of tasks the tabulation is the confirmed century practice, optimum on a ratio of availability and simplicity method of demonstration of logic following in economic reasoning. Linear diagrams are only graphic reductions of tables.

Below the inference rules are given. These rules of transformation are formulated in graphic (partially pseudo-symbolical) language of existential tables in which the linear diagrams are inscribed by fat lines. The existential tables for transfer of the economic and not only economic information in with the designation of existence (non-emptiness of sets) **is** «+»and the designation of non-existence (emptiness of sets) **is** «-» are usual enough and easily understood.

The inference rules of this tabular method are:

I. Rules of carrying of information from a partial *table-premise, or ELTD,* in the summary table, or ELTD, with additional discussed properties and additional splitting of columns:

1. If and only if there is *A*, then there is *A B* or *A not-B.*
2. If and only if there isn't *A*, then there isn't neither *A B*, nor *A not-B.*

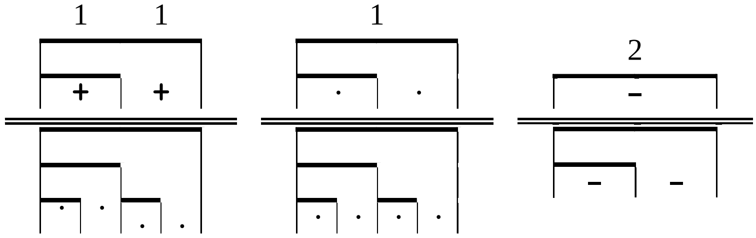


Fig. 3.

1. $\exists x\, A(x) \leftrightarrow \exists x\, ((A(x) \wedge B(x)) \vee (A(x) \wedge \neg B(x)))$
2. $\neg\exists x\, A(x) \leftrightarrow (\neg\exists x\, (A(x) \wedge B(x)) \wedge \neg\exists x\, ((A(x) \wedge \neg B(x)))$

II. Rules of association of information taken from partial tables-premises in the diagram of reason in the summary table:

3. If and only if there is *A*, then there is *A*.
4. If and only if there is *A* and there is *A*, then there is *A*.

5. If and only if there is *A* or *not-A* and there is *A*, then *A* is present.
6. If and only if there is *A* or *not-A* and *A* is not present, then there isn't *A*, and *not-A* is present.
7. If and only if there are *A*, *B*, or *C*, and there *A* is not present, then *A* is not present, and there is *B* or *C*.
8. If and only if there isn't *A*, then there isn't *A*.
9. If and only if there isn't *A* and there isn't *A*, then there isn't *A*.
(6 – 7. If and only if according even to one table-premise there is no *it*, in a result: *it* is not present).
10. If there is *A* and there isn't *A*, this is the contradiction which it is necessary to eliminate.
11. If there is *A* or *B*, and both *A* and *B* are not present, it's contradiction.
(10 – 11. If according to one premise it is present, and according to another it is not present, the data about its presence are contradictory).
12. If and only if there is *A* or *B* and there is *B* or *C*, then there is *A* or *B* and there is *B* or *C*.

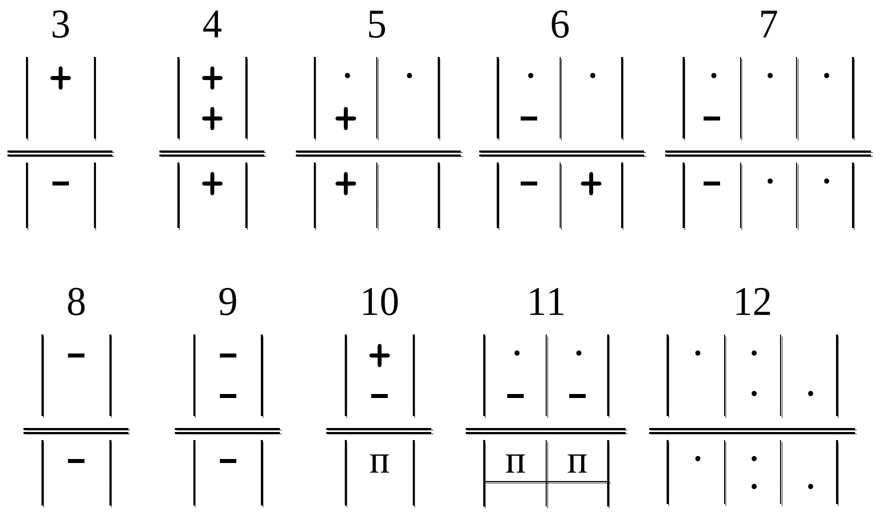


Fig. 4.

3. $\exists x\, A(x) \leftrightarrow \exists x\, A(x)$
4. $(\exists x\, A(x) \wedge \exists x\, A(x)) \leftrightarrow \exists x\, A(x)$
5. $(\exists x\, (A(x) \vee \neg A(x)) \wedge \exists x\, A(x)) \leftrightarrow \exists x\, A(x)$
6. $(\exists x\, (A(x) \vee \neg A(x)) \wedge \neg\exists x\, A(x)) \leftrightarrow (\neg\exists x\, A(x) \wedge \exists x\, \neg A(x))$
7. $(\exists x\, (A(x) \vee B(x) \vee C(x)) \wedge \neg\exists x\, A(x)) \leftrightarrow (\neg\exists x\, A(x) \wedge \exists x\, (B(x) \vee C(x)))$
8. $\neg\exists x\, A(x) \leftrightarrow \neg\exists x\, A(x)$
9. $(\neg\exists x\, A(x) \wedge \neg\exists x\, A(x)) \leftrightarrow \neg\exists x\, A(x)$
10. $\exists x\, A(x) \wedge \neg\exists x\, A(x) \rightarrow$ *contr.*, or $(\exists x\, A(x) \wedge \neg\exists x\, A(x) \leftrightarrow$ *contr.* $(\exists x\, A(x) \wedge \neg\exists x\, A(x)))$
11. $\exists x\, (A(x) \vee B(x)) \wedge \neg\exists x\, A(x) \wedge \neg\exists x\, B(x) \rightarrow$ *contr.*
12. $\exists x\, (A(x) \vee B(x)) \wedge \exists x\, (B(x) \vee C(x)) \leftrightarrow \exists x\, (A(x) \vee B(x)) \wedge \exists x\, (B(x) \vee C(x))$

On these diagrams "+" corresponds to any number, which greater of zero, and "**-**" corresponds to zero. The symbol of point can mean, for example, that it is known, how many *B* subjects are present, but it is not known, how many *C* and *not C* are among them. At numerical filling of the tables the numeric data can contradict itself only partially. For example, if according to one document there are 5X, and according to another there are just 3X about the same object, place, time and relation, the information only about 2X is contradictory:

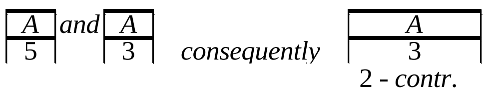


Fig. 5.

III. If the information of basis is not interesting, it is necessary to take out important information by transformation of initial table to the table – conclusion. It is made by the following rules:

13. Only if there isn't *A B* and *A not-B* is not present, *A* is not present. (If it isn't present neither *such* nor *not-such* [other], so it is not present).

14. If and only if there is *A B* or *A not-B*, then there is *A*. (If it is, such or not such, *it is*.)

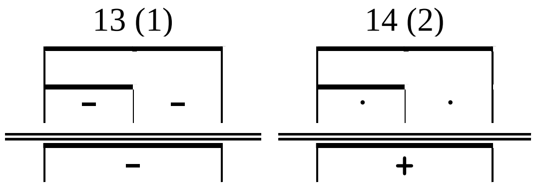


Fig. 6.

13. $\neg\exists x\,(A(x) \wedge B(x)) \wedge \neg\exists x\,(A(x) \wedge \neg B(x)) \leftrightarrow \neg\exists x\,A(x)$

14. $\exists x\,((A(x) \wedge B(x)) \vee (A(x) \wedge \neg B(x))) \leftrightarrow \exists x\,A(x)$

15. If there is *A B*, then there is *A*.

16. If at transformation "+" and "." get in one column, the above mentioned rule 5 works: if there is *A* or *not-A* and there is *A*, then there is *A*.

17. If there is (are) *A B*, then there is (are) *A B* or *A* not-*B*.

18. If there isn't *A*, then there isn't *A B*.

Below in diagrams 19 is shown action of rules 17 and 18 in the same times.

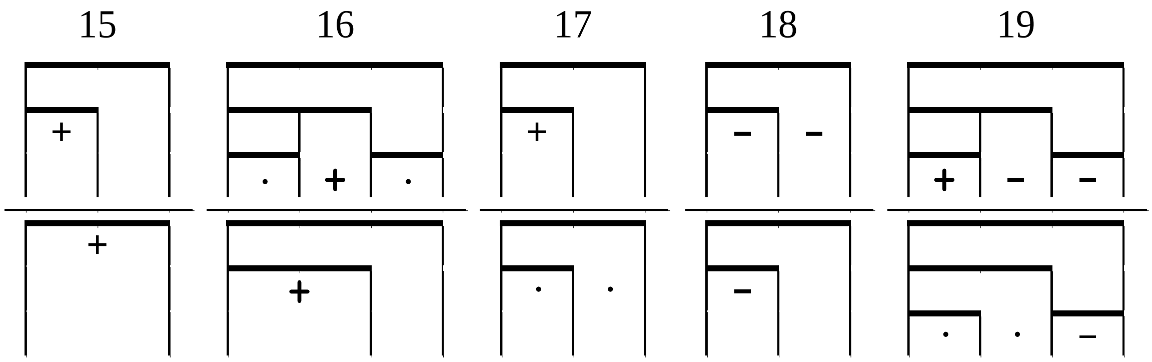


Fig. 7.

15. $\exists x\,(A(x) \wedge B(x)) \rightarrow \exists x\,A(x)$

16. See 5.

17. $\exists x\,(A(x) \wedge B(x)) \rightarrow \exists x\,((A(x) \wedge B(x)) \vee (A(x) \wedge \neg B(x)))$

18. $\neg\exists x\,A(x) \rightarrow \neg\exists x\,(A(x) \wedge B(x))$

Mainly, action of rules 6, 7, and also association of the information about not-existence give *the new* information. The full information which contains in the diagram of the reason on fig. 8, does not contain neither in any of premises, nor in their combination without application of inference rules. Deduction serves as a method of theoretical cognition.

Rules 1-9, 12, 13, 14 provide a conclusion without information loss such conclusion which is equivalent to the reason. Rules 15, 17, 18 provide a conclusions with a part of the information of the reason. Rules 10, 11 fix the information about what simplest propositions are contradictory. It in scientific thinking can be rather significant, as, for example, knowledge of that which denying of a postulate of Euclid's geometry differ Lobachevski's and Riman geometry. In this connection the rule 10 can be transformed to equivalence: $\exists x\,A(x) \wedge \neg\exists x\,A(x) \leftrightarrow$ *contradiction* ($\exists x\,A(x) \wedge \neg\exists x\,A(x)$).

Addition to the rules of tabular method: conclusion about subjects is correct, if its premises are propositions describing the same discussed case.

Diagrammatic systems could provide us with rigorous proofs [5]. Optimal construction of the logic of statistical tables is carried out in the language of linear-tabular diagrams. We illustrate the resolution of the method ELTD for example A.

Example A.

Below are four propositions-premises (the letters *A, B, C, D, E* – product names, or others) and proposition-conclusion:

Not each *C*[1]not-*D* is [3]either *A*[2]or *C*.
Each not-*B*[4]not-*D* is [6] neither *A*[5]nor *C*.
There is only[7]not-*D E*.
All *E*, except not-*B*[8]not-*C*, are[10]*C*[9]*B*.

There are *B C* not-*D E*, which not each are *A*, and there is not-*A* not-*B* not-*C* not-*D E*, and there is nothing else.[11]

This is the inference-equivalence. We must prove that the reason (a combination of premises and applied rules of inference) is equivalent to a conclusion.

Separate diagrams for each operation are given to facilitate understanding of the solution in Fig. 8. They are obtained by substituting the required terms in those diagrams dictionary connectives and propositional forms, which determine the value of these operations.

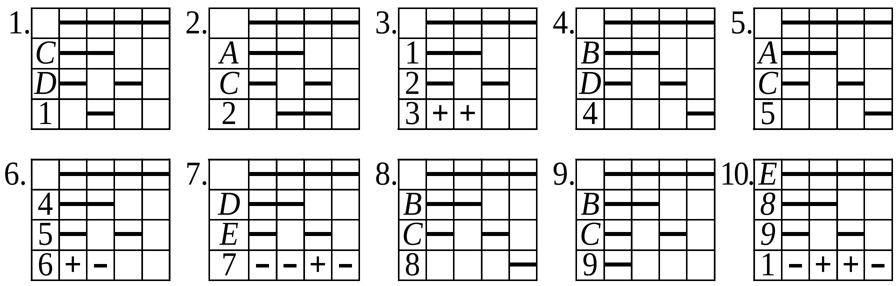


Fig. 8. Separate diagrams for operations.

Information of these diagrams at the following inference rules is transferred to the appropriate lines of the diagram in Fig. 9. This is a combined diagram premises, as well as reason and conclusions.

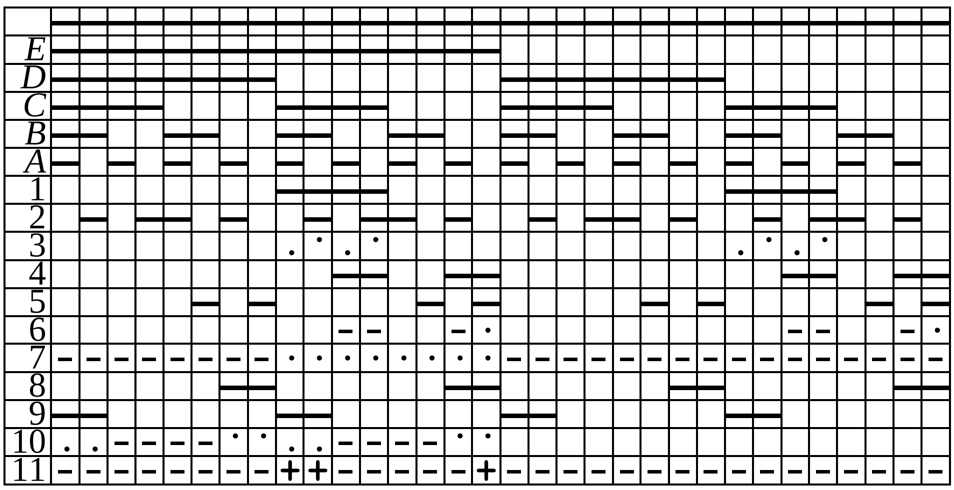


Fig. 9. The combined diagram premises, reason and conclusions.

The diagram in Fig. 9 is enough to record the construction and testing of such inferences. Even faster diagram is drawn, if the part with the letters *A*, *B*, *C*, *D*, *E* transferred to the blank which printed by printer or applied onto logical ruler

Below the above inference is written in the language of predicate logic with this interpretation of propositional forms that is fixed in the diagram dictionary:

$$\exists x\,((C(x) \wedge \neg D(x)) \wedge (A(x) \vee\vee C(x))) \wedge \exists x\,((C(x) \wedge \neg D(x)) \wedge \neg(A(x) \vee\vee C(x))) \wedge \exists x\,((\neg B(x) \wedge \neg D(x)) \wedge (A(x) \downarrow C(x))) \wedge \neg\exists x\,((\neg B(x) \wedge \neg D(x)) \wedge \neg(A(x) \downarrow C(x))) \wedge \exists x\,(\neg D(x) \wedge E(x)) \wedge \neg\exists x\,\neg(\neg D(x) \wedge E(x)) \wedge \neg\exists x\,(E(x) \wedge (\neg B(x) \wedge \neg C(x)) \wedge (C(x) \wedge B(x))) \wedge \exists x\,(E(x) \wedge (\neg B(x) \wedge \neg C(x)) \wedge \neg(C(x) \wedge B(x))) \wedge \exists x\,(E(x) \wedge \neg(\neg B(x) \wedge \neg C(x)) \wedge (C(x) \wedge B(x))) \wedge \neg\exists x\,(E(x) \wedge \neg(\neg B(x) \wedge \neg C(x)) \wedge \neg(C(x) \wedge B(x))) \leftrightarrow \exists x\,(E(x) \wedge \neg D(x) \wedge C(x) \wedge B(x) \wedge A(x)) \wedge \exists x\,(E(x) \wedge \neg D(x) \wedge C(x) \wedge B(x) \wedge \neg A(x)) \wedge \exists x\,(E(x) \wedge \neg D(x) \wedge \neg C(x) \wedge \neg B(x) \wedge \neg A(x)) \wedge \neg\exists x\,\neg\,((E(x) \wedge \neg D(x) \wedge C(x) \wedge B(x)) \vee (E(x) \wedge \neg D(x) \wedge \neg C(x) \wedge \neg B(x) \wedge \neg A(x)))$$

[$\vee\vee$ - “either... or…”(See Fig. 8, diagram 2); $\downarrow$ - “neither ... nor…”(See Fig. 8, diagram 5)]

As far as I know, there is no alternative theory of inference, which would suggest that the method allows for 10 minutes a man without a computer to prove or disprove this equivalence. A conclusion from several complex premises in natural language can be done by constructing a diagram more successfully than any symbolic methods. The condition of this success is building a new logical system, the theory of inference evidence. The basis of this theory is a new pictorial language.

Two-letter diagrams of individual operations can be not drawing, but necessarily to keep in mind.

If all the information of reason diagram is not reading in the form of relatively easy proposition as in this case, for each of the individual propositions of a complex conclusion we are building separate connective lines in an integrated diagram. We do not need to pencil such diagrams of the partial conclusions, and usually remember the areas from which the information is considered and extracted in conclusion, it is not difficult. As you develop solving skills to these problems an increasing number of operation scan be performed in the mind, and write the solution can be shorten. First detailed external models of doing any graphics, gradually more and more executed in his mind, become internal mental models. The diagrams as external representation become freely used internal mental representation.

Individual control of reasoning is necessary condition of subject’s independence, freedom and personal responsibility for substantiation of accepted decisions. Also it is training of intuitive logicality. Economic activity is documented in tables. In them the economic thinking has reliable enough logic means, which deserve to be studied regularly, to be improved, to be used consciously and to be passed to the next experts.

In some publications of last time an economic science is refused in status of exact science, in knowledge of the objective laws, and, therefore, in accomplishment of prognostic function, which is based on such knowledge. Other question is whom, where and when such withdrawal of economic science of its status is advantageous. Each subject of knowledge and any system of knowledge are doomed to imperfection and falseness of information about cognized object. In this sense there isn’t any distinction in kind between economic science and other sciences. There can be claims on the part of used empirical methods of reception of initial information and on the part of methods of the further theoretical information organization. One of the most important methods of theory is deductive logic with its semantic methods.

At the faculty of economics in BSU the practical logic is successfully read as a course for choice. It was developed by author, and it is oriented to professional training of economists. The last ten years, the author of this article gave a course of logic of choice for economists do not feel necessity to pass to reading of ‘informal logic’ or etc. In fact, I teach a course of image-bearing practical logic of natural language. Logical culture of this language should be high enough for any scientist, economist, lawyer and simply a businessman.

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TOWARDS OWL-BASED KNOWLEDGE REPRESENTATION IN PETROLOGY

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Abstract:
This paper presents our work on development of OWL-driven systems for formal representation and reasoning about terminological knowledge and facts in petrology. The long-term aim of our project is to provide solid foundations for a large-scale integration of various kinds of knowledge, including basic terms, rock classification algorithms, findings and reports. We describe three steps we have taken towards that goal here. First, we develop a semi-automated procedure for transforming a database of igneous rock samples to texts in a controlled natural language (CNL), and then a collection of OWL ontologies. Second, we create an OWL ontology of important petrology terms currently described in natural language thesauri. We describe a prototype of a tool for collecting definitions from domain experts. Third, we present an approach to formalization of current industrial standards for classification of rock samples, which requires linear equations in OWL 2. In conclusion, we discuss a range of opportunities arising from the use of semantic technologies in petrology and outline the future work in this area.

1. Introduction

Petrology, a branch of geology studying rocks and their formation, plays an important role in describing Earth's crust structure, which is essential for revealing patterns in distribution of mineral resources. Similar to other natural sciences, a wealth of knowledge requiring a proper management (especially with regard to consistency) and integration has been accumulated in petrology. These tasks could be approached more efficiently, if the knowledge had been machine processable, in particular, if a *formal theory of petrology* (i.e. a system of axioms, definitions and theorems [11], p.33) had been available. Ontologies, especially OWL ontologies, are well suited for playing the role of a cornerstone of such theory, as they have been remarkably successful in other sciences, e.g., bioinformatics, chemistry, and health care.

This paper describes our steps towards developing a formal theory of petrology. We focus on identifying basic terms, providing definitions to other commonly used terms i.e., terms used in industrial standards, and namely, rock types such as rhyolite or harzburgite, and formalizing the basic set of axioms. We use OWL as a main formalization tool enabling us, in particular, to automatically check our representation for consistency.

It is only natural to start developing a theory by identifying the important terms to be later used for representing facts, e.g., knowledge about specific rock samples. Such facts are typically stored in relational databases in modern petrology, so relational databases can be used as a source of terms. We describe the conversion of one such database, namely Proba [5] (*Sample* in Russian), to a collection of OWL ontologies containing facts expressed using an initial set of currently undefined terms in the 2 section.

Once the terms have been identified, we proceed to their formalization, i.e., writing their definitions in OWL. First, it is essential to define the basic terms, which can be used to define all other terms. Currently available definitions are usually stored in a semi-structured form in natural language thesauri. Besides other issues, this often leads to contradictions, especially given differences between schools in petrology. We use one such thesaurus, namely the Glossary of Igneous Rocks [7], to define petrological terms and relationships in an OWL ontology. In addition, we develop a webProtege-based tool to enable domain experts to work collaboratively on term definitions, in particular, to agree upon them. See the 3 section for details.

Finally, we complement the ontology by using another rich source of term definitions – internationally adopted scientific recommendations describing rock sample classification methodologies, e.g. *Igneous Rocks: A Classification and Glossary of Terms* [10]. The 4 section describes an approach to extracting definitions from the standard and expressing them as OWL axioms. As it stands, OWL 2 is insufficient for a complete capture of terms semantics (as specified in the standard), but this would be possible if *path free linear equations* were adopted.[1] We conclude the paper by summarizing our experience from the described work and outlining plans for the future.

2. Formalizing Facts: From Database to OWL

A considerable amount of important information is saved in databases, but in the form of data, which, unfortunately, is not a knowledge and requires an essential and laborious processing to obtain knowledge. This section describes a direct way of getting knowledge from the data: database conversion to the traditional form of knowledge, i.e. knowledge in a natural language. The natural language is limited to CNL to make this knowledge machine processable. We follow *T. Kuhn: CNLs are subsets of natural languages that are restricted in a way that allows their automatic translation into formal logic.* p.5 [9]. We consider CNL as a universal tool for representing a formal ontological knowledge.

The original database.

Proba DB [5] contains data from 1,174 scientific articles (Bibliography table) about 49,285 samples of igneous rocks (Measurements table). Samples are collected all over the globe, which is reflected in the Localities, llocal, lglobal and lgroup tables. The samples are assigned a rock type (Rocks table), a genesis type (Erruptypes table), age (ages table), and, which is the main thing, weight percentage (Concentrations table) of chemical substances and isotopes (list in the Elements table).

This brief description alone already shows that table and column identifiers can only approximately match the terms used by petrologists to exchange sample data. The transition to CNL also solves the problem of converting the data saved in RDB to knowledge in a form directly understandable to experts in the subject domain.

CNL sentences.

List 1 includes examples of all types of CNL sentence required to present all facts contained in the Proba DB. Local (internal) proper names required to name various objects within the knowledge base are used in the sentences. So, PUB5633 is the name of article number 5633 (from bibliography.id) in the DB. SAM32994 is the name of sample number 32994 (from measurements.id) in the DB, etc. Words are connected by letter “ _ ” in compound terms. The text also contains well-known global proper names, for example, Iceland, Atlantic _ Ocean.

List 1. Example of CNL sentences.

PUB5633 is a publication. A title of PUB5633 is "A CONTRIBUTION TO THE GEOLOGY OF THE K...". SAM32994 is a sample. SAM32994 is a rhyolite. PUB5633 describes SAM32994. PLC32994 is a place. PLC32994 is a part of Iceland. A gathering_place of SAM32994 is PLC32994. SUB469812 is a substance. SAM32994 includes SUB469812. WPC469812 is a weight_percent. A value of WPC469812 is 73.95. A component of WPC469812 is SUB469812.

The sentence structure is very simple. A very limited natural language is actually required to record all facts contained in a RDB if RDB is normalized. But RDB Proba is normalized not everywhere. Completing normalization is one of the tasks of reorganizing a DB to enable automatic conversion to knowledge. Rules of mapping the RDB content to CNL have been developed. These rules are the specification for SQL-scripts dumping RDB to CNL text [15].

OWL ontology: getting and analysis.

All generated sentences are ACE language [3] sentences, and are selected so that a concrete APE compiler[2] could compile them to OWL. A portion of the knowledge contained in each article is separated as a text (ACE file) to be converted to an independent ontology (DL species is AL(D)). Thus, the DB will be converted to 1,174 ontologies. Columns values mainly form attribute values, but also class names (rhyolite, harzburgite) and individual names (Iceland). Let's consider the ontology obtained for an article with a DB number of 5633. The obtained classes, properties and individuals are listed below.

Classes: place, publication, **rhyolite**, sample, substance, weight _ percent.

Object properties: component, describes, gathering _ place, includes, mixture, part.

Data properties: authorial _ number, chemical _ formula, first _ page, journal _ reference, last _ page, latitude, longitude, reference, title, value, year.

Individuails: Atlantic _ Ocean, Iceland etc.

All the terms used except rhyolite refer to contexts outside of petrology and even geology. These are the contexts of geography (place, etc.), scientific publications (publication etc.), solid state physics (sample, substance, weight _ percent etc), chemistry (chemical _ formula). The rest of the report focuses on obtaining rock type definitions, including that for rhyolite.

3. Formalizing Terminology: From Natural Language to OWL

The ontology of the facts specifies that the part of names used for classes, relations, individuals belongs to a different ontology (vocabulary). This dictionary ontology is supposed to provide term definitions, and the author of the article has exactly this understanding in mind. Such scientific terms are normally already collected in a dictionary, for example, Petrographic Dictionary [12], Dictionary of Geological Terms [4], Dictionary of Igneous Rocks Terms [7], Glossary of Geology [1]. The dictionary represents a very important and specific type of knowledge. It is based on subject domain terms and informal definitions of these terms. Example: harzburgite rock type article from [10], p.88:

> HARZBURGITE. An ultramafic plutonic rock composed essentially of olivine and orthopyroxene. Now defined modally in the ultramafic rock classification (Fig. 2.9, p.28). (Rosenbusch, 1887, p.269; Harzburg, Harz Mts, Lower Saxony, Germany; Troeger 732; Johannsen v.4, p.438; Tomkeieff p.247)

We have converted a specific dictionary [7] initially presented by authors as an html page to an OWL ontology. We begin the formalization of relations between terms (for example, synonymy) and term properties (for example, become outdated).

Converting the dictionary text to ontology.

We took the Dictionary of Terms of Igneous Rock Types compiled by the Interdepartmental Petrographic Committee in the Department of Earth Sciences of the Russian Academy of Sciences [7]. The dictionary contains 1,567 articles, the overwhelming majority of them being rock names. The dictionary structure and conversion procedures required to get the ontology are described in [13] and most important below.

Vocabulary: Words are connected by letter " _ " in compound terms.

Article title: The dictionary article title contains a Russian term and its English equivalent in a simple case, but its both Russian and English synonyms are often specified as well. Each term present in the title generates an ontology class. Thus, the ontology will contain classes in Russian and in English. All terms from one title are considered synonyms, i.e. their classes are declared equivalent. These conversions resulted in 3,179 classes and 1,659 class equivalence axioms having appeared in the ontology.

The text of the article: The basic dictionary article text parts are: term definition, comment, list of links to references (normally at the end), term origin description (normally located on the list of references after the article, in which the term was introduced). Comments and a list of links to references located in some parts of the ontology in the form of separate annotations are supposed to be selected from the text of the article.

The dictionary ontology (DL species is ALUF(D)) is published[3] and can be viewed using any ontology browser at this moment.

Collective management of scientific term definitions.

Another copy of the ontology is accessible by means of webProtege[4] installed on the Geology portal.[5] The dictionary ontology is 'dic' there.

It is important that a prefix and a namespace be assigned to each dictionary. We have for terms of the ontology itself, terms from the Moscow State University Geoweb portal, terms from the Petrographic Code of Russia [8], and terms from the [7] dictionary, respectively:

prefix dic: <//earth.jscc.ru/ontologies/dic.owl#>
prefix gwr: <//wiki.web.ru/wiki#>
prefix pgcc: <//www.igem.ru/site/petrokomitet/code#>
prefix pgc: <//www.igem.ru/site/petrokomitet/slovar#>

A formal term meaning definition is critical for developing a formal theory. For example, the current version of the dictionary provides a formal definition of the abessedite rock type (see Portlet Axioms for dic:abessedite), and namely

peridotite and minerals_mixture and
contains_mineral only (olivin or hornblende or phlogopite)

This formula is written using the Manchester OWL syntax. It is important that petrologists are able to read it. The process of obtaining a formal (mathematical) definition, especially in a form clear to experts, is described further, and is one of project's main ultimate goals. The [13] report contains details of the work done.

4. Formalizing Rock Classification

Rules of rock type assignment to samples are described in [10] and consist of a description of initial-classification algorithm and diagrams of final classification by percentage of essential minerals. We begin with a specification of all parts of the algorithm, sample data being its input and term (word combination) representing sample rock type its output. The algorithm is written as a set of functions in the form of a flowchart clear to petrologists.

The algorithm uses some real-valued functions and unary predicates. These functions and predicates are supposed to have value on any solid [2]. Some of these functions and predicates have been given definitions, definitions should be found for other ones, and some will probably remain without definitions and will enter in the formal theory as primary ones. The algorithm and necessary definitions are given for ultramafic types of plutonic rock as an example. It is shown then how to get formal definitions of some types of rock from the algorithm.

VPC means mineral Volume Percentage Content of the sample and is also known as "volume modal data".

We name an algorithm function (for example, ultramafic_rock_type) receiving sample data at its input and returning a sample rock type name *classifying*.

Quantitative and Qualitative Characteristics.

We need unary real-valued functions returning the volume percentage of minerals in a solid. The full set of minerals required for the algorithm will be gradually clarified.

The following functions of one argument returning a real number were required till now: VPC_melilite, VPC_kalsilite, VPC_leucite, VPC_Ol, VPC_Opx, VPC_Cpx, VPC_hornblende, VPC_garnet, VPC_spinel, and VPC_biotite. These functions are primary and may be measured.

We also need the VPC of groups of minerals (see [10] p. 4, [6] p. 6): VPC_Q, VPC_A, VPC_P, VPC_F and VPC_M. It is clear that these functions have definitions. The VPC_M definition is given below.

The following unary predicates will be required to describe the sample: pyroclastic, kimberlite, lamproite, lamprophyre, charnockite, plutonic, and volcanic. All of these predicates are supposed to have definitions. The definition of pyroclastic is given below.

Definitions.

All the definitions currently available can be found in a technical report [14]. We show typical examples here. All definitions are based on two sources: "Igneous Rocks: A Classification and Glossary of Terms" [10] and "BGS Rock Classification Scheme" [6], and are confirmed by petrologists.

VPC_Px: the modal content of pyroxenes (required to classify some plutonic rocks):

$$\mathrm{VPC\_Px(x)} =_{\mathrm{def}} \mathrm{VPC\_Opx(x)} + \mathrm{VPC\_Cpx(x)}$$

Where $=_{\mathrm{def}}$ means by definition [16].

VPC_OOC and VPC_OPH: VPC of mineral groups. We need these definitions to formalize the diagrams on Fig. 2.9, p. 28 of [10].

$$\mathrm{VPC\_OOC(x)} =_{\mathrm{def}} \mathrm{VPC\_Ol(x)} + \mathrm{VPC\_Opx(x)} + \mathrm{VPC\_Cpx(x)}$$

$$\mathrm{VPC\_OPH(x)} =_{\mathrm{def}} \mathrm{VPC\_Ol(x)} + \mathrm{VPC\_Px(x)} + \mathrm{VPC\_hornblende(x)}$$

VPC_M: returns volume percentage of group M (mafic) minerals in the sample (p. 4, 28 see [10], and especially [6] p. 6). Following the direct instructions given in [6] p. 6:

M = mafic and related minerals, that is all other minerals apart from QAPF;

we obtain the definition:

$$VPC\_M(x) =_{def} 100 - (VPC\_Q(x) + VPC\_A(x) + VPC\_P(x) + VPC\_F(x))$$

pyroclastic: We mainly rely on the 2.2 PYROCLASTIC ROCKS AND TEPHRA section [10], p. 7.

$$pyroclastic(x) =_{def} clastic(x) \wedge (\quad \forall y clast(y) \wedge part\_of(y,x)$$
$$\rightarrow volcanic\_eruption\_result(y))$$

This can also be represented in DL:

$$pyroclastic \equiv clastic \prod \forall (part\_of \circ id(clast))^{-}.volcanic\_eruption\_result$$

Algorithm.

Our algorithm is a further formalization (and elaboration!) of the classification rules provided in the [10]. The algorithm is written as a set of function flowcharts, the main function being the classifying rock _ type function. This function should be invoked to classify a sample. We have also created flowcharts for the ultramafic rock classifying function and two diagrams on Fig.2.9 [10], p. 28: OOC _ diagram _ field (the upper triangle) and OPH _ diagram _ field (the lower triangle). The IUGS diagram flowcharts are deliberately presented as a chain of if-nodes, each one being responsible for one specific diagram area. Each if-condition represents a system of linear inequalities. The set of such conditions has important mathematical properties:

• Any two conditions are incompatible, since areas corresponding to them are mutually disjoint

• The union of all conditions gives inequalities for a triangle, since conditions cover the entire triangle

It is important that the described properties can be checked automatically if definitions are loaded in a reasoner working with linear inequalities.

Rock type predicate definition.

The classification algorithm implicitly contains definitions of all types of igneous rock. Definitions can be obtained from the algorithm in the form of formulas one free variable formulas of predicate calculus of first order with numbers. The formula structure shows the complexity of the concept behind the term, and also specifies all the concepts underlying a term. This is extremely important for finding the primary concepts. We have quite formally, i.e. using mathematical conversions, obtained formulas for the harzburgite and dunite predicates.

Harzburgite: when applied to the sample, the harzburgite predicate should give “true” if the sample is harzburgite, and “false” otherwise. Flowcharts have to be tracked from top to bottom, and conditions leading to a OOC _ diagram _ field flowchart node producing the “harzburgite” value collected, to get a predicate. These conditions should be connected by the logical operation “and”. The conversions will give the following formula:

harzburgite(x) = def plutonic(x) $\wedge$ $\neg$ (pyroclastic(x) $\vee$ kimberlite(x)
$\vee$ lamproite(x) $\vee$ lamprophyre(x) $\vee$ charnockite(x))
$\wedge$ VPC_carbonates(x) $\leq$ 50 $\wedge$ VPC_melilite(x) $\leq$ 10 $\wedge$ VPC_M(x) $\geq$ 90
$\wedge$ VPC_kalsilite(x)=0 $\wedge$ VPC_leucite(x)=0 $\wedge$ VPC_hornblende(x)=0
$\wedge$ 0.4*VPC_OOC(x) $\leq$ VPC_Ol(x) $\leq$ 0.9*VPC_OOC(x)
$\wedge$ VPC_Cpx(x)<0.05*VPC_OOC(x)

Thus, a precise definition of the harzburgite igneous rock type consists of three parts:

• Qualitative characteristics (lines 1, 2).
• Absolute restrictions on modal data (lines 3, 4).
• Relative restrictions on modal data (lines 5, 6).

Now we can compare this definition with the informal definition quoted in Section 3: the formal definition is more complete. It does not suppose anything and does not refer to the diagram. It contains the necessary part of the diagram.

5. Lessons Learnt, What is Next?

This paper describes our experience of converting the petrological information stored in databases, glossaries, and classification standards to a formal OWL-based representation. A similar approach, i.e. one based on providing unambiguous and consistent definitions for all terms, can be used in developing a formal theory for virtually any scientific area. We will now briefly summarize the results and outline plans for the future.

From data to knowledge. Moving from a database of petrological facts to a knowledge base is beneficial from multiple perspectives. Firstly, the new representation is richer and enables generation of sentences in a controlled natural language, which, in our experience, are understandable to geologists. They can be used not only as an interface to the KB, but also to annotate publications, which should lead to increased amounts of machine-processable metadata. Secondly, the KB (equipped with a CNL-based interface and a SPARQL endpoint) can be integrated with the ontology that provides the vocabulary. This is important for ensuring a consistent use of the terminology across all information systems using the KB. The stored knowledge can be further integrated with other available datasets, e.g. those provided by the EarthChem consortium.[6]

Centralized vocabulary. Providing a controlled vocabulary is essential for managing the knowledge. In our case, it was most important to collect the terms used in the database in a single OWL ontology, and give them unambiguous definitions along with human-readable annotations. This is a substantial improvement compared to the previous situation where terms were defined informally and in multiple, often contradictory sources. The resulting system can be used both as a dictionary (for people and applications i.e., via SPARQL) and as a tool for collaborative work on terminology.

Rock classification. The formal definitions of the terms captured in standard OWL are not detailed enough to support automated rock sample classification, which is one of the most important use cases in petrology. To this end, we have investigated the possibility of complementing the definitions with quantitative restrictions on their mineral composition. Such restrictions can be defined using linear equations, a possible extension to the current data ranges in OWL 2.

Similarly to databases and glossaries, the classification recommendations, namely [10], are sometimes ambiguous and incomplete as well, so their formalization requires collaboration with petrologists from the Subcommission on the Systematics of Igneous Rocks of the International Union of Geological Sciences. However, we managed to identify some predicates and functions requiring definitions, which can be used as building blocks of a formal theory. Following the methodology described in the 4 section, we have obtained detailed definitions for two types of rock as well as for some auxiliary terms. We plan to extend this work to cover all rock types in the classification.

Our work enables answering questions like Is a current object a sample of a certain rock by performing instance checking, a standard reasoning task in OWL. However, this can be extended to query answering to find all possible rock types for a specific sample or to find all samples of a specific type in the KB. This, however, requires reasoning with linear inequalities, which is not supported at large scale at the moment (some reasoners are available, e.g. RACER).

Finally, we would like to stress that our approach to formalization differs from what can be seen in many biological and chemical ontologies. They are often deep class hierarchies with numerous asserted subsumptions between class names and with relatively few definitions. We focus on providing detailed definitions (using standard OWL and linear equations) instead, and plan to rely on automated reasoners to build and maintain the hierarchy. This may enable use of the ontologies in a broader range of situations as illustrated by rock sample classification.

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Notes

1. The details of the proposed extensions are available at http://www.w3.org/TR/owl2-dr-linear/. Our work on petrology may be viewed as a use case for supporting linear equations in future OWL versions.
2. Attempto Parsing Engine http://attempto.ifi.uzh.ch/site/tools/
3. http://earth.jscc.ru/ontologies/dic.owl
4. http://protegewiki.stanford.edu/index.php/WebProtege
5. http://earth.jscc.ru/webprotege/
6. EarthChem is a community-driven effort to facilitate the preservation, discovery and visualization of and access to the broadest and richest geochemical datasets possible: http://www.earthchem.org.

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PROGRAM STRUCTURE

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Abstract:

A program is usually represented as a word chain. It is exactly a word chain that appears as the lexical analyzer output and is parsed. The work shows that a program can be syntactically represented as an oriented word tree, that is a syntactic program tree, program words being located both in tree nodes and on tree arrows. The basic property of a tree is that arrows starting from each node are marked by different words (including an empty word). Semantics can then be directly specified on such tree using either requirements or additional links, and adding instructions to some tree nodes enables program execution specification.

Section 1 contains a summary of the approach and an example of program text and its syntactic tree. The syntactic schema described in Section 2 is used to specify a family of syntactic trees. As a result, the language proves to be a family of trees specified by the schema. Extra requirements depending on programming language semantics are set forth for the syntactic tree of a program. In addition, extra arrows, that is semantic links, are drawn on the syntactic tree of the program. Semantics depends on the programming language, and it will be reviewed in terms of the Turingol language [1] in Section 3. The structure of the external data existing irrespective of the program requires a separate consideration. Section 4 describes data for Turingol alone with program using a tape. A program presented in the form of a syntactic tree with semantic links has to be initialized. For example, the external data to be used by the program has to be connected to it. In addition, instructions are entered for the Executor in nodes corresponding to executable statements. Initialization is described in Section 5, and execution in Section 6.

Appendix 1 describes bringing author's grammar Turingol to a form giving a schema, and Appendix 2 describes tools required to work with finite labeled graphs.

1 Introduction

We will review Turingol [1] as an example throughout the paper. It is a simple programming language with formal semantics described since its birth, and notably by the author. However, a closer examination shows that translation of a Turingol program into Turing machine programs, that is a translation verifying the Turingol program, is described. As regards the language itself, the author says that it is clear as it is [1], p.138, lines 1-3. Thus, the requirements for the Turingol program are hidden in translation. However, a couple of requirements are mentioned in express form [1], p.139:

"...programs are malformed if the same identifier is used twice as a label or if a **go to** statement specifies an identifier which is not a statement label."

We will not review Turing machine programs, and will focus on Turingol programs themselves. Some Executor is supposed to execute a Turingol program using a tape. The tape structure will be accordingly described.

A program is a graph of words, and description of its properties and rules of use is the main objective of the paper. An oriented tree of language words, that is a syntactic tree of the program (or its part), underlies the graph. A syntactic schema similar to Wirth [2] syntactic diagrams in a sense is introduced to specify a family of trees. However, while syntactic diagrams specify rules for building a word chain, a schema specifies rules for building word labeled trees. There exist only two ways of creating sub-trees: a sequence of nodes (connected by arrows) and a node with a set of arrows specified by the schema starting from it. The fact that a particular schema generates trees with differently labeled outgoing arrows has to be proved, which will be done for Turingol.

The fact that the program itself is a tree enables a new view of the description of its semantics. Various attributes may obviously be assigned to tree nodes, including assignment for compilation [1].

The obtained tree has to meet some requirements providing a well-formed Turingol program [1], p.138. We will assume for simplicity that there exists a separate verification phase for these requirements, although any verifications can undoubtedly be carried out in the course of building up a tree.

If the tree is good, then additional arrows expressing syntactic and semantic relationships facilitating Executor's work will be drawn between some of its nodes. Tree completion (to a graph) is highlighted as a separate phase for simplicity as well.

To completely understand a program, one has to specify its execution procedure. An abstract Executor moving across the program graph and executing instructions located in nodes corresponding to statements is supposed to exist for this purpose similar to [6]. This approach corresponds to how a programmer thinks of his program. Placing instructions in nodes is included in the program initialization phase. It also includes program connection to external data, which consists of connecting a tape to the program in case of Turingol.

The Executor executes the program by moving from some of its nodes to other and executing the instructions specified there (including operations on the tape). This is exactly what the programmer had in mind when creating the program. The idea is that the programmer can basically execute his program on his own without using a machine (for example, Turing machine). Moreover, it is exactly this understanding of how an Executor executes the source text of the program on the part of the programmer that obliges such Executor (aka debugger) to interact with the programmer as if it (Executor) were executing the source text of the program.

1.1 PROGRAM EXAMPLE – TEXT

Let's review program 4.1 [1], p.137. Some word combinations are written through a dash for simplicity, which makes them lexically one composite word. The dash has to be accordingly added to the language alphabet.

```
tape-alphabet is blank, one, zero, point;

print "point";

go to carry;
```

test: if the-tape-symbol is "one" then

{print "zero"; carry: move left one-square; go to test};

print "one";

realign: move right one square;

if the-tape-symbol is "zero" then go to realign.

1.2 SYNTACTIC TREE OF A PROGRAM

Syntactic tree of program 4.1 is given in Fig. 1.

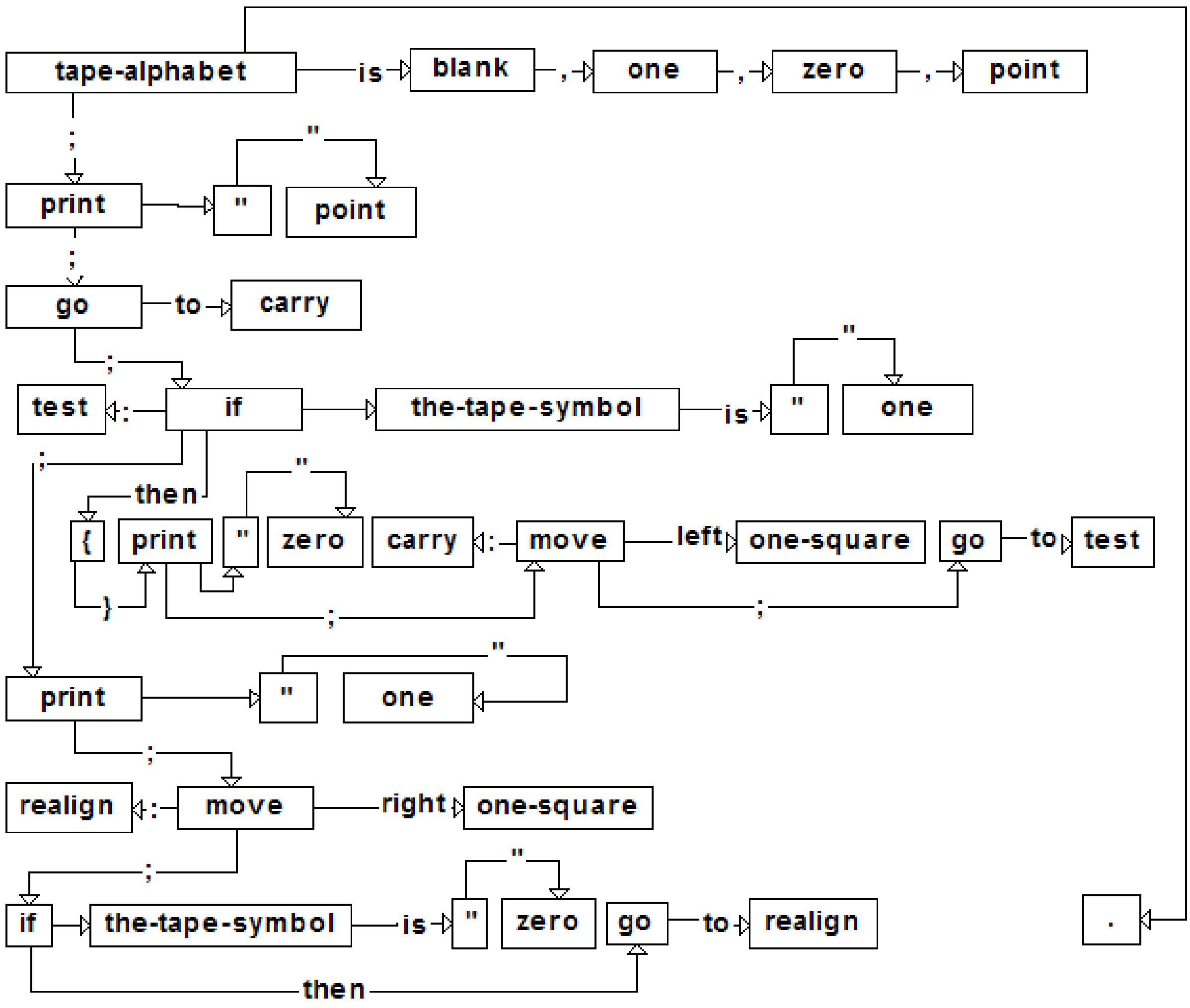

Fig. 1 Syntactic tree example

Each program word (including special words such as ';', ',') goes to a specific label (of a node or an arrow). The arrow label and generally node label as well may be an empty word.

One may see for oneself that arrows starting from any node are differently labeled.

Let's highlight node chains by a separator placed on arrows:

First row: 'blank' ',' 'one' ',' 'zero' ',' 'point'.

Fifth row: 'print' ';' 'move' ';' 'go'.

Vertically: 'print' ';' 'go' ';' 'if ' ';' 'print' ';' 'move' ';' 'if'.

The first chain is uniform in that its members are of identical structure. This is simply a word in this case. The second and third chains are non-uniform, as their component statements have differently structured trees.

Note, too, that the program tree is not ordered, i.e. it contains no child node order, an order that could be used to specify tree drawing (position on a plane or line).

We will see that such tree contains enough information for the entire program semantics. Accordingly, one may assume that drawing rules are only used for convenience of recognizing parts of the tree.

2 SCHEMA. SPECIFYING TREE FAMILIES

A syntactic schema is used to specify families of labeled trees.

Regular expressions are used to specify permissible values of a word in a node or on an arrow of a tree. However, a specific word only is usually permissible. We will need only two regular expressions: left|right and [a-z]+ for Turingol, the last one specifying a word over an alphabet of lower-case English letters.

An explanatory comment will be further added in square brackets to the text of definitions, which will not be a part of the definition formally.

Let PLA and MLA be two non-overlapping alphabets.

PLA is a programming language alphabet, and MLA is a metalanguage alphabet.

Definitions

Syntactic tree (*sytr* in abbreviated form) is oriented labeled tree such that the node label together with arrows are a word from PLA*.

Uni-labeled tree is sytr with arrows starting from each node labeled differently.

Sentential tree (setr) is oriented labeled tree such that:

1.Node label is a word from PLA* or MLA+
2.Arrow label is a word from PLA*.

Nodes labeled by a word from MLA+ are named *auxiliary* and play a role similar to non-terminals in context-free grammars.

Syntactic schema is oriented labeled graph with 2 types of arrow (*AND arrows* and *OR arrows*), AND arrows being divided in two (*mandatory/optional*) subspecies.

Labels are regular expressions over PLA [including words from PLA*].

A node with no outgoing arrows is named an *atomic node / A node*.

A node with outgoing OR arrows only is named a *OR node*.

A node with outgoing AND arrows only is named a *AND node*.

Each schema node can be assigned a unique name (*schema name*), which is a word from MLA+. Such schema is named a *completely named schema*.

End of definitions

2.1 TURINGOL. SCHEMA

Let us review a completely named Turingol schema as an example (see Fig. 2).

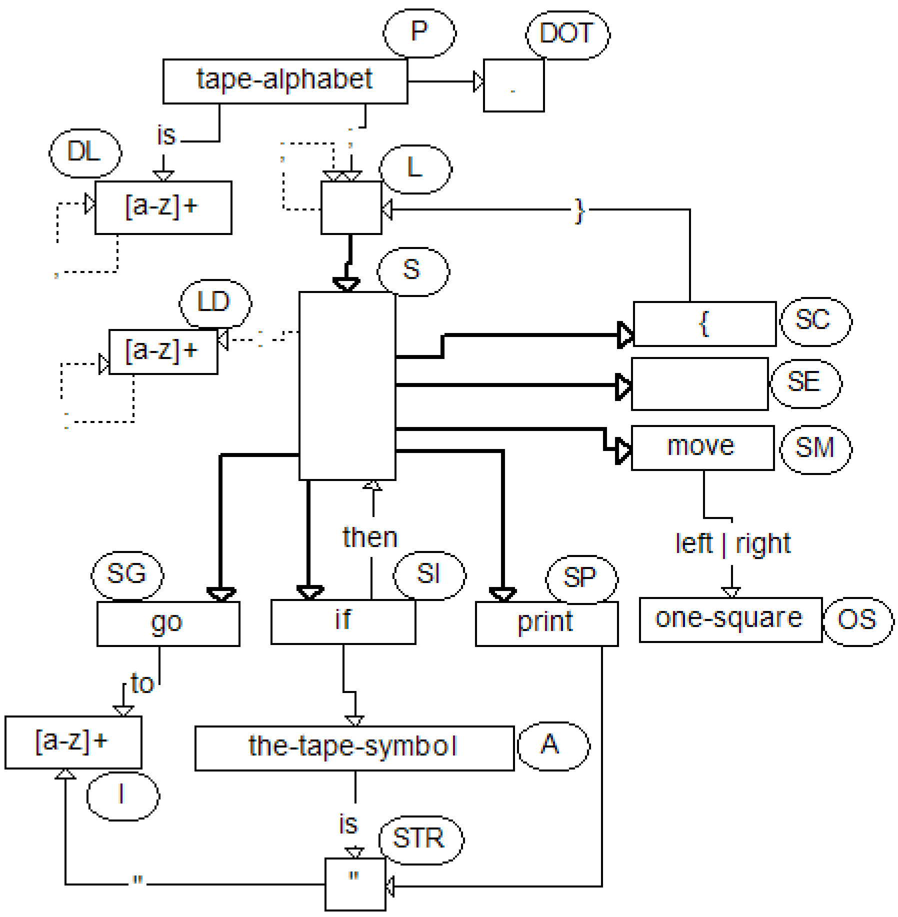

Fig. 2 Syntactic schema for Turingol

Nodes are rectangles. Ovals contain schema names. OR arrows are marked by heavy lines. Optional arrows are marked by dash lines. The majority of nodes and arrows are labeled by specific words. DL, LD, I nodes are labeled by the regular expression [a-z]+. An arrow from SM to OS is labeled by the regular expression left|right. L, S, SE nodes are labeled by empty words. P-DOT, SI-A, SP-STR arrows are labeled by empty words too.

Let us now describe how a syntactic schema specifies a sytr family.

Building a sentential tree from a schema node: Let the schema be somehow completely named, and a schema Y0 node specified. To create a sentential tree from the Y0 node,

Create an isolated N1 node (a Y0 node copy).

Draw copies of all mandatory and some optional Y0 node outgoing AND arrows from the N1 node, and label arrow copies by words permissible by regular expressions of their samples in the schema. Create a node with a schema name of the corresponding schema node at the end of each arrow. If an Y0 node has OR arrows, then take a node name at the end of one of them, and enter it in N1; otherwise, label N1 by a word specified in the regular expression of the Y0 node.

The instruction becomes significantly simpler for an OR node:

Create an isolated N1 node.

Take a node name at the end of a Y0 node outgoing OR arrow and enter it in N1.

The instruction for an atomic node is simple as well:

Create an isolated N1 node.

Label N1 with a word specified in the regular expression of the Y0 node.

Thus, the schema enables assignment of a set of sentential trees to each schema name of a node. We actually have a context-free grammar of trees in a compact form where the left part of the rule contains an isolated node labeled by a schema name, and the right part, a setr or sytr.

For example, two setr are generated from L (because of an optional arrow): one is simply S, and the other is S with a ';' arrow in L; and 12 setr are generated from S: six isolated nodes (SG, SI...) and six such nodes with a ':' arrow in LD.

OR arrows (including one as in L) enable specification of setr with a non-terminal in the root.

A tree is inserted in another tree by substituting the root of the other tree for a node of the first one.

The schema is convenient due to the fact that it is a connected graph, and so enables an association between some graph properties and properties of the family of generated trees.

Building a sytr using a schema: Let a schema be completely named. To obtain an initial setr, it suffices to take any schema node, and to create an isolated node labeled by the schema name of this schema node.

Let there be given a C1 setr with a N1 node labeled by any schema name. Then a C2 setr can be created from a schema node with a schema name equal to N1 label and substituted for N1 in C1.

If no node labeled by a word from MLA+ is found after several substitutions in C1, then we have obtained a sytr.

Schema properties: one can readily see that:

1. No OR arrow label is used
2. Parallel OR arrows are redundant
3. The label of a node with an outgoing OR arrow is not used.

Thus, we can assume without loss of generality that:

1. The label of an OR arrow or a node with an outgoing OR arrow is an empty word
2. There are no parallel OR arrows on the schema.

Single AND loops (i.e. when a node has a single loop) play an important role, as they specify chains. Parallel AND loops specify a tree, i.e., a structure that apparently does not occur in programming languages. Note that if an AND arrow of a loop is mandatory, then the building process will never come to an end, and this part of the schema is useless for finite trees. Thus, all AND loops can be considered optional.

2.2 UNI-LABELED FAMILIES

So, a schema specifies a family of trees. We can try to prove that only uni-labeled family trees exist by analyzing the schema. The following schema property is required for a family to be uni-labeled.

AND condition: regular sets of regular expressions of AND arrows starting from each node do not overlap.

The AND condition can be easily checked on a schema. But it is insufficient, as if both AND arrows and OR arrows start from a N1 node, then AND arrows from nodes at OR arrow ends, etc. may be added to N1 AND arrows. For example, a ':' AND arrow from S (*statement label*) can propagate onto any statement by following any OR arrow in case of a Turingol schema.

The most important property of 'interesting' schemas is presence of cycles on the graph. The cycle can be characterized by the node types it includes.

OR loop possesses the following property: if it is located in an OR node, then it is redundant, otherwise it results in a non-uni-labeled sytr, as AND arrows will be drawn from the node in the course of building, and the same 'non-terminal' will remain in the node, which means that the same arrows can be drawn from it again. An OR cycle (OR arrow cycle on a schema graph) gives non-uni-labeled trees as well. Therefore, we will be interested in schemas with *AND cycle condition* satisfied: each cycle includes an AND node. It also means that the schema has no OR loops.

If the AND cycle condition is satisfied, a simple algorithm of AND arrow label propagation on the schema works: we create a <node schema name, arrow label> pair for each AND arrow, and place it in the node located at the origin of the AND arrow. Each pair so created follows all OR arrows and 'settles' on AND and A nodes.

Sufficient condition: If all of the pairs accumulated in AND and A nodes (including those initially existing) after 'advance' have pairwise disjoint regular sets (no conflict), then the schema generates only uni-labeled trees.

The Turingol schema satisfies the AND condition and the AND cycle condition, as there are only two cycles with OR arrows on the schema – SC-L-S-SC and SI-S-SI, each containing an AND node (SC, SI, respectively).

The propagation algorithm results in <L,';'> and <S,':'> pairs arriving in nodes at the ends of OR arrows from S, but without conflicts.

Thus, the sufficient condition is satisfied, and the schema specifies a family of uni-labeled trees.

Clearly, an arbitrary schema may generate exotic trees, including infinite ones. Appendix 1 discusses the relationship between the schema and context-free grammars, including demonstration of the Turingol context-free grammar required to obtain a schema.

2.3 General case of a programming language

Following [5] and exercise 2.4.28, "2.4 Context-Free Languages" from [4] each context-free language is produced by a grammar with all rules looking like A:aBbC, A:aBb, A:aB, A:a. If the empty word belongs to the language, then the S:e rule is permitted, where capital letters denote some non-terminals, lower case letters (except 'e') – some terminals, and 'e' – the empty word.

It can be easily seen that a sentential tree can be assigned to each type of the right part. If we present the graph as sets of triples <arrow origin, arrow label, arrow end> corresponding to the number of arrows in graph, then we obtain:

- two triples: <a '' B>, <a b C>., i.e. 'a' is a root with an empty word labeled arrow to B and a 'b' labeled arrow to C for the first type of rule.
- one triple: <a b- B>. for the second type of rule. Where '-' after b means here that b should follow B linearization during tree linearization. Without a sign "-" a situation is just reverse.
- one triple: <a '' B>. for the third type of rule.
- The fourth and fifth rules give atomic nodes.

The context-free grammar form given above is named a "standard operator form" [5]. Thus, if we present a grammar as a standard operator form, then we can mechanically go over to uni-labeled trees. Every programming language can be theoretically said to permit a representation of its programs as uni-labeled trees. There clearly exist several such representations, and some of them are likely to be visual and natural. The Author of the language had best to take care of a representation in the form of trees.

3 TURINGOL. PROGRAM STRUCTURE

There exist requirements already for the syntactic tree of the program.

In addition, the following objects converting a sytr to a graph have to be additionally built on the syntactic tree of a program:

- Semantic link: 'is-declared-at' arrows
- Semantic control flow links: 'next', 'yes', 'no' arrows.

Designations: the requirements are named. The requirements that are not critical for program execution have a W (warning) letter in their names.

Appendix 2 contains some ways of handling the graphs that we will need below.

3.1 ALPHABET

Definition: tape word declaration node (*w-declaration-point*) is node in a chain with a **'tape-alphabet'+is** head.

'tape-alphabet'+is is a path formula (see Appendix 2) and means a node at the end of an 'is' labeled arrow starting from a 'tape-alphabet' labeled node, i.e. one is proposed to follow (indicated by '+') the 'is' labeled arrow from a 'tape-alphabet' labeled node.

Definition: tape word use node (*w-usage-point*) is any **print+'"'** node or **if+''+is+'"'** node, **print+'"'** and **if+''+is+'"'** being path formulas here (see Appendix 2).

The fact that these paths are meaningful (passable) can be easily seen from the schema.

AW1, AW2, AW3 REQUIREMENTS

(AW1) Labels must be different in all w-declaration-points.

(AW2) Label value must be equal to the label of a w-declaration-point in any w-usage-point.

(AW3) The label of each w-declaration-point must be used in a w-usage-point.

Note: It is not so in Example 4.1, and it may be a cause for warning.

3.2 'IS-DECLARED-AT' ARROW FOR A WORD USAGE POINT

Once fulfilled, the requirement (AW2) enables drawing an 'is-declared-at' arrow from a w-usage-point to a w-declaration-point.

Note: The Executor does not need such arrows.

They are required for further processing in other languages.

3.3 LABEL

Definition: Label target point (l-target-point) is any ':' arrow destination node.

Definition: Label use place (l-usage-point) is any 'to' arrow destination node.

L1, L2, LW1 REQUIREMENTS

(L1) All labels must be different in l-target-points.

(L2) Every l-usage-point must have an l-target-point with the same label.

(LW1) Every l-target-point must have an l-usage-point.

Otherwise, it is useless.

3.4 CONTROL FLOW GRAPH

Let there be given a sytr.

The word 'go' may prove to be also a label or a tape word. To distinguish, let us introduce the simplest classification on sytr:

Data node: this is a node of a sub-tree starting from a **'tape-alphabet'+is** node as well as the node at the end of the ':', 'to', '' arrows.

S node: this a non-data node labeled by the word 'go', 'if', 'print', 'move', '', '{'.

L-chain member: a ';' arrow destination member.

Let's also introduce a simple S node classification: *control statements* are 'if', '{'`, 'go' labeled nodes. Let us name the other *ordinary* ones: 'print', 'move', '' labeled nodes.

Once the Executor has executed an instruction in the next in turn S node, it must know where to go further, and so forth until it has executed a Stop direction. The next executable node is one and only one for the majority of the nodes, across which the Executor moves. We will draw a semantic 'next' arrow to the next executable node. Only 'if' nodes are exceptions in that two control arrows 'yes', 'no' will be drawn from them similar to the situation described by Post [6]: "(B) Perform operation (e) and according as the answer is yes or no correspondingly

follow direction $j_{i''}$ or $j_{i''}$,".

The Executor starts from the 'alphabet-tape' node, and must syntactically follow the ';' arrow to the first statement. It is a frequent situation when the ('next', 'yes', 'no') control arrow is parallel to the syntactic arrow.

Building procedure: Draw a 'next' arrow in parallel to a ';' arrow starting from the 'alphabet-tape' node.

Program halting is a special act that is only implied in a language such as Turingol, i.e. it has no express statement. Let's introduce a special additional node named *Stop* where the Stop direction will be placed at the program initialization stage (see below) to simplify program structure and to follow Post's ideas [6].

Building procedure: Create a node and label it by the word 'stop'.

The 'tape-alphabet' and '{' nodes have a subordinate statement chain; 'if' node has one subordinate statement. The next statement to be executed after an ordinary statement located last in an L-chain or subordinate to 'if' is specified in the subordinating node, provided that the subordinating node is not a subordinate one, otherwise one must go to its subordinating node. Let's draw an auxiliary 'back' arrow to show the subordination relationship.

Building 'back' arrows: Draw a 'back' arrow from the last member to the node subordinating the chain (i.e. 'tape-alphabet' or '{' node) in each L-chain. Draw a 'back' arrow from each node hanged to 'if' node over 'then' arrow to this 'if' node.

Thus, it follows from the building procedure that one and only one of the situations listed below occurs for each S node:

BACK situation: there is a 'back' arrow starting from it

';' situation: there is a ';' arrow starting from it, i.e. the statement is not the last one in the L-chain.

Building procedure: Redirect the 'back' arrow with an 'alphabet-tape' destination node to the 'stop' node.

The result of building 'back' arrows for Example 4.1 is presented on the Figure 3.

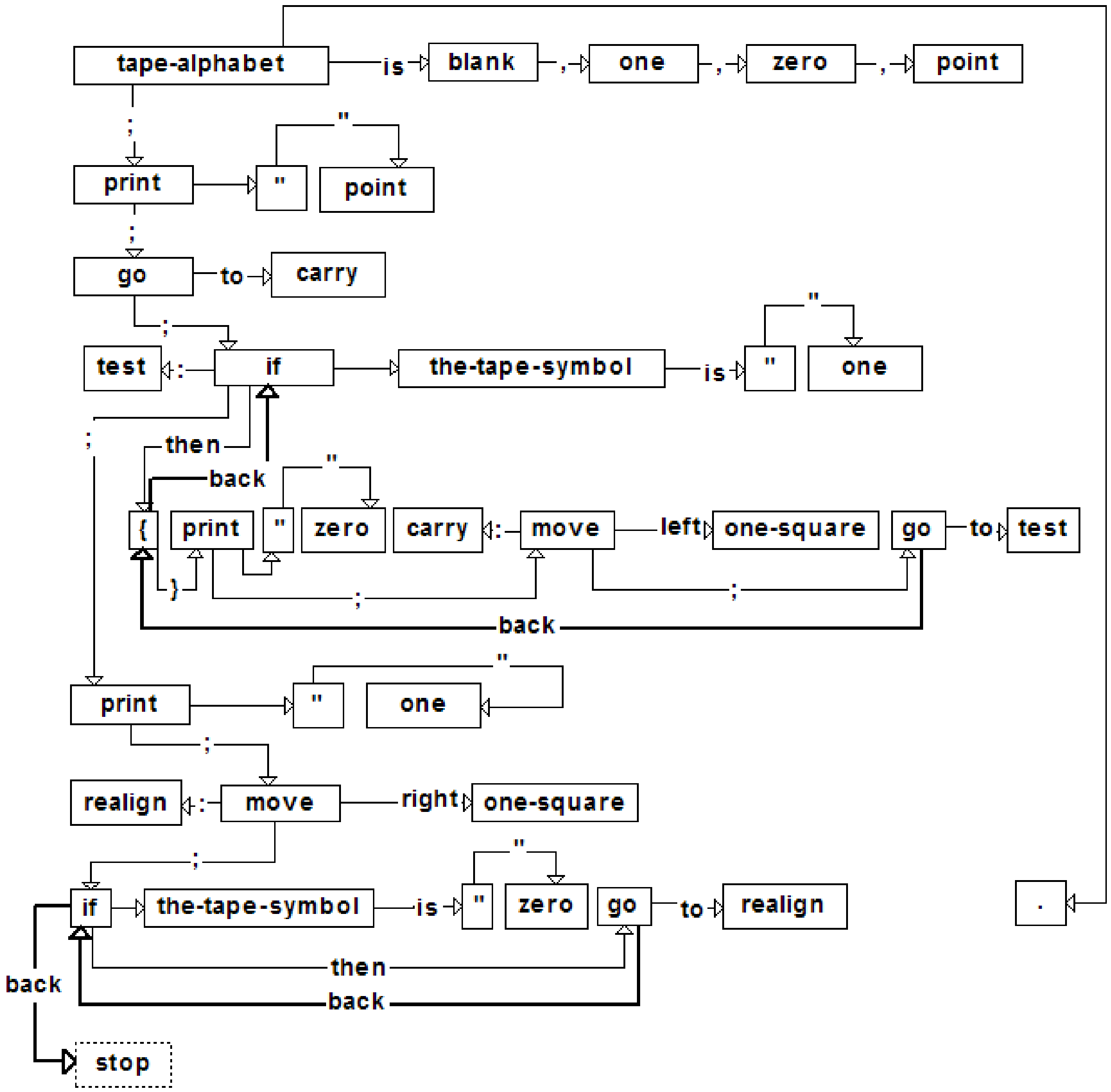

Fig. 3 Syntactic tree with 'back' arrows

Building control arrows that do not depend on the BACK or ';' situation:

Draw a 'yes' arrow in parallel to 'then' arrow for each 'if' node.

Draw a 'next' arrow in parallel to '}' for each '{' node.

For each S node labeled 'go', using its l-usage-point, draw a 'next' arrow to the last node, to which one can rise from an l-target-point with the same label value as the l-usage-point label along ':' arrows.

The possibility of the last building procedure is ensured by fulfillment of the L1, L2 requirements.

Building control arrows for the ';' situation, i.e. a statement that is not last in the L-chain:

Draw a 'next' arrow parallel to the ';' arrow for ordinary statements ('', 'print', 'move').

Draw a 'no' arrow parallel to the ';' arrow for the *if* statement.

Thus, the syntactic ';' arrow is not used to build 'next' arrows for 'go' and '{' nodes in the ';' situation.

Building the control for nodes with an outgoing 'back' arrow:

1. Label all 'back' arrows as unprocessed ones

2. Stop if there are no unprocessed 'back' arrows

3. Select the entire (full length) unprocessed 'back' arrows chain. It either goes to ';' (let's name it a *NEXT situation* and this ';' arrow a *C1*) or ends in the 'stop' node (let's name it *a STOP situation*)

Draw a 'next' arrow from each ordinary node of a 'back' arrows chain with an outgoing 'back' arrow, and draw a 'no' arrow from the 'if' node. Draw this arrow:

- To the same node that is the C1 end node in the NEXT situation
- To the 'stop' node in the STOP situation.

Label all 'back' arrows chain arrows as processed ones. Go to 2.

We have obtained a control flow graph. It is exactly the one, along which the Executor moves when executing a program.

The control flow graph is built on S nodes, 'alphabet-tape' node, which is a start node, and 'stop' node. Thereby, it is built on syntactic nodes (plus 'stop' node) and is simply structured from the viewpoint of outgoing arrows:

- One and only one 'next' arrow starts from each S node (except 'if' node) and 'alphabet-tape' node.
- One and only one 'no' arrow and one and only one 'yes' arrow starts from each 'if' node.

No control arrows parallel to syntactic arrows are drawn on the drawing below (see Fig. 4), which corresponds to Example 4.1, but corresponding syntactic arrows are drawn bold. The label of the corresponding control arrow can be easily restored. So, 'no' arrow obviously corresponds to the ';' arrow from 'if' in line 4.

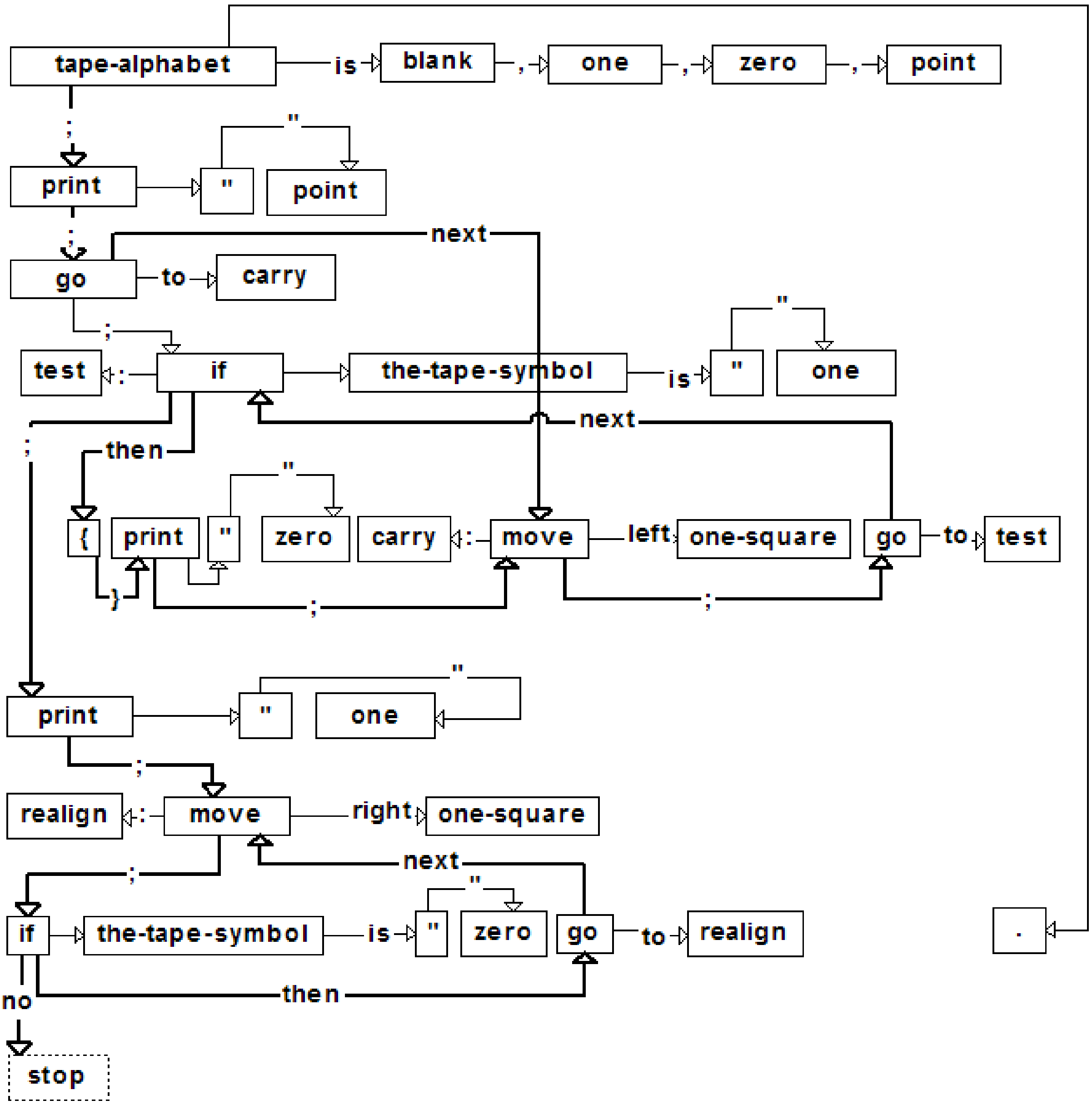

Fig. 4 Syntactic tree with control flow graph

CW1, C2 REQUIREMENTS

The idea of control flow generates a natural requirement:

(CW1) Reachability of any program S node from the 'alphabet-tape' node across the flow graph.

For example: If there is a 'go' node in the L-chain, then the node located at the end of the ';' arrow starting from it must have a label, otherwise it is unreachable. See, for example, a ';' arrow from 'go' node in the third line in 4.1.

The problem of halting a program on data (including any kind of data) is extremely important, as indicates program usability. It has to be solved specifically for each program or a class of programs. However, there are also obvious negative results requiring only an understanding of the idea of control flow.

Requirement (C2): 'next' arrows may not form a cycle.

In particular, "go to" may not point at itself. Note that C2 is no CW1 consequence, as several control arrows may belong to a node.

3.5 Conclusion

The presence of a construct in the form of a tree enables a precise and natural expression of requirements for a program as well as setting rules for building additional links.

'is declared at', 'back', 'next', 'yes', 'no' links as well as 'stop' node are necessary in the majority of programming languages.

Whether any links else are necessary is the subject of ongoing study of Standard Pascal [8]. One of the candidates is the 'has type' link for languages with data types.

The flow graph is easier to 'translate' to a Post machine than a Turing machine.

4 Data. Tape

Tape is a finite chain of nodes labeled by words. Arrows are labeled by empty words.

Tape finiteness necessitates its completion if a step to the left or to the right is required to a non-existing node. It has to be done as required. The node and the arrow are labeled by empty words at creation.

Note: Such tape is obviously no Turing machine tape, as it:

- Contains words, not letters in cells
- Is finite (see [6] p.105, about possible tape improvements)

A family of tapes, which is a syntactic construct, may be formally described in addition to language grammar:

T:: =I | I T

where I stands for identifier non-terminal (see Appendix 1).

The description of a language actually requires two starting symbols – for the program and for external data.

4.1 Operations on the tape

The Executor will:

- Compare labels of some program nodes with labels of tape nodes
- Put labels of some program nodes on tape nodes
- Expand the tape

Tape expansion is based on the following situations and operations.

Let N1 be tape root node name.

Then execution of the "Create a node and an arrow from it to the n1 node." direction (see Appendix 2) will give a tape again with a new root and an arrow from it to the old root.

Let N2 be the last tape node name.

Then execution of the "Create a node and an arrow from the n1 node to it." direction (see Appendix 2) will give a tape again with a new last node.

4.2 Conclusion

The tape used by the Turingol program is a prototype of the programming language file, and the method of representation in the form of a chain may be applied to the file as well.

5 INITIALIZATION

A 'tape' labeled arrow is created from the 'tape-alphabet' node to some node of the tape to be used by the program at the start. The tape node containing the end of 'tape' arrow is named *current tape node*.

The current tape node is usually supposed to be the first tape node 'after tape opening'. However, the last tape node must be the initial current node for the program from Example 4.1, for example.

In addition, instructions for the Executor are entered in some program nodes. Instructions are placed in nodes in a way similar to Post's approach [6] except that programs consist of numbered lines with branches on numbers according to Post, while we assume that programs consist of nodes with branches on arrows.

Note: These instructions may be assigned to schema nodes as attributes and be copied to sytr already at the building stage.

5.1 INSTRUCTIONS IN NODES

Instructions are given below for S nodes, **'**tape-alphabet' node and 'stop' node. Other nodes have no instructions.

Two instruction versions exist for 'move' nodes.

It is important to stress that the nature of the operations performed in instructions is working with labeled graphs by changing node labels, creating nodes and arrows, and reassigning the destination of the 'tape' arrow. If the terminology is not intuitively clear, then see a more precise description in Appendix 2.

Tab. 1 node instructions

node label	**instruction**
tape-alphabet	Follow the 'next' arrow.
stop	Stop.
go	Follow the 'next' arrow.
{	Follow the 'next' arrow.
if	If the **'tape-alphabet'+tape** node label equals **+''+is+'"'** node label, then follow the 'yes' arrow. Follow the 'no' arrow.
"	Follow the 'next' arrow.
print	Label the **'tape-alphabet'+tape** node by a **+''+'"'** node label. Follow the 'next' arrow.

node label	instruction
move	A version of the instruction for a 'move' node with an outgoing 'left' arrow: If no '' arrow exists to a **'tape-alphabet'+tape** node, then create a node and an arrow from it to the **'tape-alphabet'+tape** node. Reassign the 'tape' arrow to a **'tape-alphabet'+tape-''** node. Follow the 'next' arrow.
move	A version of the instruction for a 'move' node with an outgoing 'right' arrow: If no '' arrow exists from a **'tape-alphabet'+tape** node, then create a node and an arrow from the **'tape-alphabet'+tape** node to it. Reassign the 'tape' arrow to a **'tape-alphabet'+tape+''** node. Follow the 'next' arrow.

Note: the unique place where a passage against the course of an arrow was necessary and the '-' operation was used is the **'tape-alphabet'+tape-''** path formula, i.e. jump from a 'tape-alphabet' labeled node following a 'tape' arrow to the node located at its end (current tape node), and then jump from the current node against an empty word labeled arrow to the node at its origin. This latter node may be named a *previous node*.

'Code optimization': two complete sets of instructions exist for 'move' nodes. The selection has to be based on the neighboring nodes. In continuation of this subject, one may take the label of a node located at the end of the **+''+is+'"'** path from an 'if' node, and place it on an appropriate place in the instruction itself. 'print' nodes may be handled in a similar way.

5.2 PROPERTIES OF INSTRUCTIONS

Instructions possess properties, which jointly ensure a fault-free execution of any program on any tape.

Let's list some basic properties.

On the whole,

1. Only instructions located in 'move' nodes modify the graph structure through tape expansion. The tape chain structure is not violated.
2. Only instructions located in 'print' nodes modify graph labeling and affect tape nodes only.

In addition,

1. Each instruction takes the Executor away from a node, i.e. the Executor cannot find itself in a DIRECTIONS EXHAUSTED IN THE NODE situation.
2. Each instruction located in an S node takes the Executor to an S node or 'stop' node. So, the Executor always reaches a node where an instruction is present and cannot find itself in a NO INSTRUCTION IN THE NODE situation.
3. Any path formulas are passable in instructions. Some of them are always passable

due to the building procedure, as, for example, the **+''+'"'** path from a 'print' node or the **+''+is+'"'** path from an 'if' node. The paths that include a 'tape' arrow during program execution point at different tape nodes, but always remain passable at the time of use.

6 Execution

The Executor executes a program starting from a node and executes the instructions assigned to the next node. Execution may generally fail. A fail-safety hypothesis exists for Turingol.

Initial state of the program-plus-tape construct:

1. predefined tape node is connected to the 'tape-alphabet' node using the 'tape' arrow.
2. Instructions are entered in nodes.

The Executor starts from the 'tape-alphabet' node.

The Executor moves from node to node in the directions shown by arrows. The Executor executes the instructions contained in the nodes reached.

If a node contains no instruction, then the Executor fails with a No Instructions situation. A similar crash occurs in the Instructions Exhausted in the Node situation.

Each direction has its necessary and sufficient condition of normal execution. The Executor generally must act cautiously and verify the necessary and sufficient condition of normal execution before executing a direction. It will enable it to report a crash.

However, the following *fail-safety hypothesis* exists for Turingol:

Let C1 sytr be built on a schema, meet requirements, and be supplemented with links and instructions.

Let T1 be a tape.

If an arrow is drawn from the 'tape-alphabet' node to any T1 tape node, and the Executor is started in the 'tape-alphabet' node, then the process will never fail.

For example, INSTRUCTIONS EXHAUSTED IN THE NODE SITUATION may not occur, as the last direction in each instruction is FOLLOW THE ARROW form.

Thus, despite the fact that instruction elements (including path formulas) may fail in an arbitrary situation, Turingol computer system (program+data) possess a property that is unusual for programming languages, i.e. fail-safety.

One of interesting tasks is to develop Hoare axioms [7] for operations and to prove the fail-safety hypothesis.

7 Discussion

The program structure obtained seems rather realistic, as the Programmer had links such as 'next' and 'is declared at' in mind when creating the program, and exactly the action that the Executor would later find in a node when writing down a statement.

The Executor is a person under the described approach, as instructions are written in a limited natural language. The Executor must be able to work with labeled graphs and certainly letter strings. The computer system structure itself, i.e. a program graph filled with instructions and connected to data, is acceptable so far as we can prove its properties.

It is interesting that a uni-labeled tree is enough for semantics, i.e. we did not need an ordered tree. Some agreements on drawing are most likely required for recognition on the drawing graph elements (as it is always with graphs).

Several sytr versions obviously exist for a programming language. The author of the language had best provided a version of his own.

The fact that the tape proved to be a graph is a start of a study where both external and internal programs data is supposed to be represented by labeled graphs, which are usually trees.

Optimization and Post machine: Program graph may certainly be control optimized, i.e. many paths may be reduced. As a result, we will obtain an isomorphic Post machine structure.

Knuth attributes (compilation): Attributes may be specified for nodes already on the schema, and a sytr with attributes enables using the Knuth method for calculation of their values – for example, for compilation.

Although regular expressions are mentioned in schema definition, just a few regular expressions are used in programming languages in practice:

- Specific words of the language (including the empty word) or their finite |-combinations,
- Identifiers,
- Numbers (integers, real numbers).

A finite labeled graph as a mathematical construct enables definitions, algorithms and proofs. All these things may be formalized if required, but that was not the purpose of this work.

7.1 Conclusion

The author of the language could have describe the tree structure at once. The author of the language could precisely specify semantic requirements for the program on the tree although additional effort is required to do that. A precise language semantics description must results in high-quality and even intelligent compiling programs.

In studying a phrase of the language, the programmer should probably know not only the order of terms, but also the main one and the one to be put on an arrow. However, a suitable tree editor may hide the tree structure from the programmer.

If a tree editor is created, then building and storing a program in the form of a tree will make the lexical analysis and parsing unnecessary.

One can briefly say that while a program is usually considered to be a word chain, it actually proves to be a uni-labeled word tree.

Appendix 1: Relationship between a schema and a context-free grammar

A1.1 From schema to grammar

A sytr has to be ordered to represent it in the form of a word chain. Node and outgoing arrow numbering is introduced on the schema for that purpose: The node and AND arrows starting from it are numbered by numbers starting from 1. OR arrows remain unnumbered.

If an AND arrow word has to be located after a word chain generated by a node at the end of the arrow, then '-' letter is put after the number.

Algorithm for building a production on the basis of a schema node:

Left part: Node schema name

Right part: List regular expressions on AND arrows and node schema names at their ends in the order of numbers on arrows. If an arrow number contains '-', then the regular expression of the arrow is written after the schema name. If an arrow is optional, then a meta-statement of optionality is assigned to the pair.

The regular expression of the node is put under node number in the event that there are no OR arrows; otherwise, the - |-expression from node schema names on OR arrow ends is put there.

Examples:

Let the numbering for L to be as follows: 1 for the L node and 2 for the ';' arrow.

We obtain:

L::= S (';' L)?

Let the numbering for S to be as follows: 2 for the S node and 1 for the ':' arrow.

We obtain:

S::= LD ':' (SG|SI|SP|SM|SE|SC)

A1.2 TURINGOL. FROM GRAMMAR TO SCHEMA

To obtain a syntactic schema, we will start with the author's version of the grammar, convert it to the necessary form, and show how the grammar should be represented in the form of a schema generating syntactic trees of Turingol programs.

EBNF notation from [3] is used to record context-free grammars.

Author's context-free grammar Turingol (see Table 1 in [1]).

```
A::= [a-z]
I::= A | I A
D::= 'tape alphabet is' I | D ',' I
O::= 'left' | 'right'
S::= 'print' '"' I '"' | 'move' O 'one square' | 'go to' I
    |  '' /*empty word*/
    | 'if the tape symbol is' '"' I '"' 'then' S | I ':' S
    | '{' L '}'
L::= S | L ';' S
P::= D ';' L '.'
```

Some insignificant changes in the language lexicon:

Composite terms are written through a hyphen (for example, tape-alphabet)

So, we will need an additional letter '-'.

Alphabet:

PLA:: = [-a-z:;{}.]

Equivalent conversion of the grammar: We need rules with right parts possessing certain properties – roughly speaking, we need non-terminals to be alternated with terminals.

Transformation:

1. D, O non-terminals are substituted in their places of use.

2. For the L non-terminal equivalent rule is

L::= S (';' L)?

Let's introduce for P

DL::=[a-z]+ (',' DL)?

We obtain:

I::= [a-z]+

S::= I ':' S | 'print' '"' I '"'

| 'move' ('left' | 'right') 'one-square'

| 'go' 'to' I | ''

| 'if' 'the-tape-symbol' 'is' '"' I '"' 'then' S

| '{' L '}'

P::= 'tape-alphabet' 'is' DL ';' L '.'

3. Let's introduce

LD::=[a-z]+ (':' LD)?

Then

S::= I ':' S | Q

can be written as

S::= (LD ':')? Q

which gives

S::= (LD ':')?

('print' '"' I '"' | 'move' ('left' | 'right') 'one-square'

| 'go' 'to' I | ''

| 'if' 'the-tape-symbol' 'is' '"' I '"' 'then' S

| '{' L '}'

)

4. Let's introduce additional non-terminals:

OS ::= 'one-square'

DOT::= '.'

STR::= '"' I '"'

A ::= 'the-tape-symbol' 'is' STR

SG ::= 'go' 'to' I

SI ::= 'if' A 'then' S

SP ::= 'print' STR

SM ::= 'move' ('left' | 'right') OS

SE ::= ''

SC ::= '{' L '}'

5. We finally obtain:

```
I  ::= [a-z]+
OS ::= 'one-square'
DOT::= '.'
LD ::= [a-z]+ (':' LD)?
DL ::= [a-z]+ (',' DL)?
STR::= '"' I '"'
A  ::= 'the-tape-symbol' 'is' STR
SG ::= 'go' 'to' I
SI ::= 'if' A 'then' S
SP ::= 'print' STR
SM ::= 'move' ('left' | 'right') OS
SE ::= ''
SC ::= '{' L '}'
S  ::= (LD ':')? (SP | SM | SG | SE | SI  | SC)
L  ::= S(';' L)?
P  ::= 'tape-alphabet'  'is'  DL  ';'   L DOT
```

A completely named schema for Turingol corresponds to the rules provided.

Appendix 2: Finite graphs. Properties, actions, instructions

We will need some simple propositions for work with finite oriented labeled graphs.

For example, we will need the following proposition:

"There exists a unique 'W1' arrow", where W1 is any word. This proposition is true if one and only one arrow having a word W1 as its label exists on the entire graph.

In addition, we will need actions, including actions changing the graph.

The normal execution condition is graph property required for an action (proposition) to be executed (evaluated) normally. Some combination of normal execution conditions specifies the necessary and sufficient condition of normal execution (evaluation).

Note: Post describes situations where the Executor may be failed. True, Post speaks about inapplicability ("assuming" [6]). However, failure is an important element of programmer's thinking – they do occur, and he must think about fail-safety.

Post describes only two failure situations:

- Attempt to write in a cell already containing a value.
- Attempt to delete a value from a cell containing no such value.

Let's introduce a *path formula* to indicate a node on a graph*:*

Let WORDN mean the label assigned to the unique graph node (let's designate it N1), and WORD mean any word. Then

- WORDN+WORD means the node located at the end of the arrow starting from N1 and labeled by the word WORD,
- WORDN-WORD means the node in the origin of the arrow reaching N1 and labeled by the word WORD.

It means that we follow the arrow in the first case and move in the opposite direction in the second case. If there are several arrows for jumping, or there is no one, then the formula is considered inapplicable, and an attempt to use such formula during execution of the algorithm will result in a crash.

The initial node must be the only node on the graph. For example, if the path formula starts with a 'tape-alphabet' labeled node, then a crash is possible, as there exist more than one specified node or there is no one.

If we are in a node that is considered to be a current node, then +WORD means a node located at the end of the WORD arrow starting at the current node, and -WORD means a node located at the start of the WORD arrow ending at the current node.

If a word contains the '-' letter or is an empty word, then it has to be put in quotation marks, i.e. an empty word will be designated as '' in the path formula.

Path passability: Let P1 be a path formula. We will need an proposition of the type "P1 path is passable" from the current or a given node.

Some elementary propositions using path formulas are provided below. Necessary and sufficient conditions of normal execution are also provided for these propositions.

Let W1 designate a word, P1, P2 be some path formulas.

Tab. 2 propositions. condition of normal execution

proposition	Necessary and sufficient condition of normal execution
The P1 node label equals P2 node label.	P1 path is passable and P2 path is passable.
No 'W1' arrow exists to the P1 node.	P1 path is passable.
No 'W1' arrow exists from the P1 node.	P1 path is passable.

Actions: Some actions required for work with oriented graphs and their necessary and sufficient conditions of normal execution are listed below.

Let W1 be a word, P1, P2 be some path formulas.

Tab. 3 actions. conditions of normal execution

Action phrase	Necessary and sufficient condition of normal execution	Description
label the P1 node by the P2 node label	P1 path is passable and P2 path is passable.	The label of the target node is overwritten.
reassign the 'W1' arrow to the P1 node	P1 path is passable and there exists a unique W1 arrow.	The arrow end is reassigned.
create a node and an arrow from it to the P2 node	P2 path is passable.	The arrow and node created are labeled by an empty word.
create a node and an arrow from P2 node to it	P2 path is passable.	The arrow and node created are labeled by an empty word.
follow the 'W1' arrow	There exists a unique W1 arrow from the current node.	Note: A crash is possible in two cases: 1. There exists no arrow with such label 2. There exist several arrows with the same label

Direction: Any phrase of an action represented as a sentence is a direction.

Let S1 be a proposition, D1 - an action phrase. Then

"If S1, then D1." is a direction.

Note: According to Post, direction is a numbered sentence in English.

He lists three forms of direction [6], p.103-104: "Start at the starting point and follow direction 1. It is then to consist of a finite number of directions to be numbered 1, 2,

3... n. The i-th direction is then to have one of the following forms:

(A) Perform operation Oi [Oi = (a), (b), (c), or (d)] and then follow direction j_i,

(B) Perform operation (e) and according as the answer is yes or no correspondingly

follow direction $j_{i''}$ or $j_{i''}$,

(C) stop."

Note on the starting point: According to Post, a one-dimensional space of boxes is infinite in both directions, and one of the box is selected as the initial one – this is the box where the worker begins to work.

Instruction is a sequence consisting of one or more directions.

Note: Post's directions (A) and (B) forms can be represented as follows:

(A) Perform operation O_i. Follow direction j_i.

(B) If (e) then follow direction j_{i1}. Follow direction j_{i2}.

Then they will consist of two simpler directions.

An instruction is obviously a special case of an algorithm.

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INTERVIEW

SOURCES OF THE ANALYTIC PHILOSOPHY IN SLOVENIA

Andrej Ule (1946) was born 1946 in Ljubljana, Slovenia. He graduated 1971 in mathematics and achieved M.A. (1974) and phD (1981) in philosophy at the University of Ljubljana. He got in 1982-3 the Humboldt grant for the study of logic and theory of science in München, at the Institute for Logic, Theory of Science and Statistics. His current position: Professor of analytic philosophy and philosophy of science at the Faculty of Arts, University of Ljubljana. Fields of interest: philosophy of logic, Wittgenstein's philosophy, epistemology, philosophy of science, comparative philosophy. Some of his recent books are: Operationen und Regeln bei Wittgenstein (Frankfurt/M, 1998); Logos spoznanja (Logos of Knowledge)(Ljubljana, 2002); Dosegljivost resnice (Attainability of Truth) (Ljubljana, 2004), Znanost, družba, vrednote (Science, Society, and Values)(Ljubljana, 2006), Circles of Analysis: Essays on Logic, Mind and Knowledge (Berlin, 2008).

Andrew Schumann: Logical tradition as well as tradition of analytic philosophy has deeply rooted in Slovenia, a small Central European country. How can it be explained? In a word why is it?

Andrej Ule: Analytic philosophy in Slovenia has its roots in the algebraic logic of the late 19th century and in the Meinong school from the beginning of the 20th century. Our first modern logician was Mihael Markič who at the break of the 19th and 20th century developed his own unique system of algebraic logic and grammar. Mihajlo Rostohar and Franc Veber, both widely recognized Meinong's pupils, also wrote on logic and epistemology, Veber actually being our first formal philosophy professor at the newly founded University of Ljubljana. I also have to mention a well-known Meinong's pupil Ernst Mally, who was of Slovene descent, but renounced his Slovenian origin at the wake of the Second World War. Mally was an expert on deontic logic, ontology and epistemology. Unfortunately, in the aftermath of the Second World War the philosophy in Slovenia completely broke off with previous schools of thought and for some time Marxist dogmatism prevailed. Luckily enough, some philosophers and intellectuals maintained the free spirit and this is especially true of my professor of logic and methodology Frane Jerman at the Department for Philosophy at the Faculty of Arts, University of Ljubljana, in the 1960's. Jerman was a real proponent of critical and rational thinking in philosophy, and he strongly supported open and democratic discussions and rejected any dogmatism. He taught (among other subjects) modern logic, methodology of science, Wittgenstein's philosophy and history of logical empiricism. His main interests were Russell's, Wittgenstein's and Schlick's philosophy and the logic of Jan Łukasiewicz. He also translated and commented some basic works of Russell and Wittgenstein in Slovene language. His lectures were characterized by unique and distinctive clearness of thought, substantial argumentation and absence of any ideologization. He inspired several students (myself included) to follow in his footsteps, embracing his philosophical method, analytical approach and critical orientation. At the end of 1970's and 1980's it became possible to study philosophy in the Western countries and some of Jerman's students took this opportunity further their studies of logic, analytic philosophy and/or philosophy of science in Western Europe and the USA. After our return to Slovenia we began with our own teaching and research in different areas of analytic philosophy. Currently, there are some bright, promising students and young researchers working in the broader field of analytic philosophy helping us spread and further develop analytical thought in Slovenia. Analytic philosophy is now one of the three main philosophical schools in Slovenia (phenomenological/hermeneutical, post-modernist/post-structuralist and analytic).

A.Sch.: What Slovenian schools of logic and philosophy of science could you talk about? What are their achievements?

A.U.: In Slovenia we have two main centers for the study of logic and philosophy of science. The first is at the Department of Philosophy at the Faculty of Arts in Ljubljana, and the second is at the Department of Philosophy at the Faculty of Arts in Maribor. The Department in Maribor is mainly analytically oriented, while in Ljubljana there is a rich mixture of different philosophical orientations. It is also possible to study mathematical logic at the Faculty of Mathematics and Physics in Ljubljana, and (a certain amount of) applicative logic at the Faculty of Computer Sciences in Ljubljana. Some of the achievements of "Slovenian analytic school" is the establishment of two analytic journals (the international journal *Acta Analytica* and the national (Slovenian) *Analiza*), Bled's annual international symposiums of analytic philosophy, and a myriad of books and articles in both national and international journals and publishing houses.

A.Sch.: Slovenia is one of the post-Socialist countries that were quite easily democratized and involved into the common life of the European Union. What are the reasons in your opinion? Perhaps it is connected with Slovenian ways of thinking cultured by analytic-philosophical tradition that is rational, objective, and technological in comparison with Marxism?

A.U.: Now, it is debatable whether post-Socialist countries, including Slovenia, are really fully democratized, and not merely partially or even *seemingly* democratized. One only has to take into account numerous examples of nationalism, ethnocentrism and other forms of exclusivism, which are on the uprise in Slovenia (as well as other ex-Yugoslavian countries). However, it is possible to maintain that in some general sense Slovenia did in fact witness a rather rapid decline and decomposition of the Socialist regime and the incorporation of marketing economy and pluralist democracy in its economical, social and political structures. The main reason for this was probably the long-standing and relatively strong tradition of civil right movements present in the Slovenian socialist regime which wasn't as oppressive as it was in other parts of Yugoslavia. The main contribution of the analytic-philosophical tradition to this process was its predominantly scientific approach to economical and political issues. The resolution of the political crisis in Slovenia was therefore significantly de-emotionalized if compared to other Yugoslavian countries. I myself have written extensively on Popper's falsificatory theory of science and his criticism of Marxism already in 1970's and 1980's. Some other analytical philosophers (Miščević, Potrč, Borstner etc.) have pointed out the negative effects of emerging nationalisms, serious problems pestering language ideologization etc.

A.Sch.: Slavoj Žižek is the most famous Slovenian philosopher and in general he is the only philosopher from the post-Socialist world who became a celebrity. Why did Žižek become on the crest of the wave? How is it related to his philosophical ideas, his public image, and his origin?

A.U.: I myself am not particularly fond of post-structuralist/post-modernist philosophy. I have been greatly impressed by Sokal's critique of post-modernist and post-structuralist approaches to philosophy and science, and have even written an article on the topic. In my opinion, the main reason why Žižek has become so popular in the world was the fact that he brought out certain paradoxes and pains of modern society before anybody else did. His unique combination of different philosophical approaches (Lacanian psychoanalysis, Hegelian dialectics, Marxist criticism of Capitalism, and even fragments of analytical philosophy of language) provided him with a very flexible theoretical approach to almost any problem that was especially successful in pointing out all inherent paradoxes of a given issue. Žižek was successful in applying the Lacanian psychoanalytic theory and method on great historical events, e. g. the fall of the Berlin's wall, at the end of 20th century. His extraordinary talent for rhetoric and humor contributed additionally to his world-wide recognition. There is no doubt that Žižek is extremely good at bringing out problematic aspects of a situation, but his philosophy rarely provides plausible, adequate solutions.

A.Sch.: Science has rapidly changed since knowledge began to be reduced to technologies in the late 20th century. How does it influence philosophy of science? What are the new trends? Is the definition of science the same?

A.U.: The increasingly applicative value of scientific research that was reflected primarily in numerous technological achievements was responsible for temporary domination of positivist, pragmatist and operationalist conceptions of science. In this view, a scientific theory was merely a summary (or sum total) of actual and potential empirical evidences and experimental operations. In the last twenty years or so, however, it is possible to notice a revival of interest in some fundamental theoretical, even philosophical questions within the framework of fundamental scientific disciplines. For example: the nature of physical reality, the role of the observer in contemporary cosmology and quantum mechanics, the issue of emergent structures in biology, the mind-body problem in cognitive sciences etc. The answers to these questions demand a profound theoretical framework founded on serious philosophical reflection. This new approach transcends the limits of mere technological and operational consequences, and opens new and interesting horizons in the field of philosophy and theory of science.

A.Sch.: What are the perspectives of development of science? How does the role of logic increase or decrease?

A.U.: The trends suggest a renewed mutual and two-way respect and interest among scientific in philosophical schools of thought. In this process, the role of logic is not limited solely to rational reconstruction of scientific language, but also plays a key role in knowledge production. I believe that, in the future, the development of new scientific theories and even paradigms will be closely and associated with computerized modeling of scientific hypotheses and theories, and of course logics will play a fundamental role in constructing appropriate computer programs and models.

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INTERVIEW
IS LOGIC EVER FOUNDATIONAL?

András Máté studied mathematics and philosophy at the Eötvös University Budapest (Hungary). He began his research in logic and its history as an assistant of Imre Ruzsa. He is currently associated professor of logic at the Philosophical Institute of the Eötvös University. He made his PhD (CSc) at the Hungarian Academy of Sciences about Plato and Frege. His research interests include history of logic and semantics (semantical ideas in Plato's dialogues, Stoic logic, medieval semantics, Leibniz, Bolzano, Frege) and philosophy of mathematics (second-order logic as a framework, philosophical ideas of 20th century Hungarian mathematicians). He wrote four textbooks of logic and its history and several papers about different topics including even aesthetics of music in Hungarian, 14 papers in German and English mainly about the history of logic. He translated works by Plato, Frege, Tarski, Kneale and Kneale.

Andrew Schumann: *Der Wiener Kreis* is one of the most legendary schools of logic and analytic philosophy. How did it come out in Hungary? Which names? Which ideas?

András Máté: Four years ago our department has finished a common research with the Institute Vienna Circle of the Vienna University about the reception and influence of the Vienna Circle in Hungary. The results of the research have reinforced my previous impressions that this influence was rather poor. Hungarian intellectual life before the First World War was open to new and modern ideas and because of geographical and political reasons, new ideas from Vienna have found especially easily their way to Budapest. But in the inter-war period, Hungary became a bad-tempered, stuffy, conservative and nationalistic country – this was a ressentiment against the lost war, the huge territorial losses that Hungary suffered from and the continuous economical difficulties in comparison with the dynamic development for a half century before the War. The official, academic philosophy was dominated by conservative tendencies, and a little minority of the intellectual life had their orientation towards innovative ideas coming from the part of Europe lying west from Hungary – mostly towards very different ones from the views of the Vienna Circle. I have found in the journals of that period a few papers by younger philosophers who knew that views and tried to convey them – but nothing more.
During the Communist period, the situation became at first even worse. For the first fifteen years, there was any other mention of the names of scientifically oriented philosophers of the Western world than some condemnatory ideological phrases which displayed mostly the incompetence of their author. In the sixties, the activity of the circle of Georg Lukács changed the situation: they made a requirement for the Marxist criticism of "bourgeois" philosophy that it be based on the accurate knowledge and analysis of the ideas and on arguments, not on pure ideological patterns. But their central interests were not philosophy of science and related topics, either. Nevertheless, György Márkus's translation of Wittgenstein's *Tractatus* (1963) brought a turn on this area, too, and some of his students formed a seminar on the philosophy of science. Their leading personality, Ferenc Altrichter was strongly involved with the philosophy of the Vienna Circle. He translated together with the other leading philosopher of science of that generation, Márta Fehér a thick volume of translations from the writings of members of the Vienna Circle, whose extensive introductory essay (by Altrichter) is the best secondary literature in Hungarian concerning the Circle until today. We Hungarians often quote the words of the poet Endre Ady to characterize ourselvels: "people who always come too late." The ideas of the Vienna Circle could have their liberatory, enlightening function in Hungary of the late sixties at the very last moment in the history (and for rather few people). Their criticism of subtle philosophy operating with obscure, ill-defined concepts and their view of philosophy as analysis and criticism of the conceptual tools of human

knowledge brought some fresh air for their new readers and their critical attitude seemed to be a good example to follow against official Marxism-Leninism but against other, conservative philosophies that dominated the intellectual theatre in the previous period. However, in the seventies and eighties – when the next generation of philosophers could read politically not relevant philosophical literature from the West relatively free – we learned that most of the theses of the Vienna Circle philosophers got refuted by the immanent development of the sort of philosophy partly originated by them.
Another favorable development for the reception of Vienna Circle ideas in the sixties and seventies was that Imre Ruzsa, originally a mathematical logician, came to philosophy and founded modern Hungarian philosophical logic. (He has been my mentor.) His own ideas were not very close to the Vienna Circle, but their works included thorough, competent and objective discussions of Vienna Circle views relevant to his topics – especially the logical views of Carnap. I think in his work did many valuable ideas of the Vienna Circle gain the place they deserve – no more the programmatic foundations of contemporary philosophy, but important pieces of philosophical tradition that everybody who discusses their topics should take into attention.

A.Sch.: Which contributions of Hungarian logicians became the most interesting and promising up untill today?

A.M.: Just a brief enumeration of some names and achievements:
Gyula/Julius König (1849—1913), who played an important role in set theory of the first years of the 20th century. In his forgotten posthumous work *Neue Grundlagen der Logik, Mathematik und Mengenlehre* he devised an extremely interesting intuitionist-like "synthetic logic." Its analysis is my next goal of research.
János/John von Neumann (1903—1957), who didn't publish in logic (he resigned from publishing his proof of the Second Incompleteness Theorem, acknowledging Gödel's priority), but had done fundamental work on rather different areas now closely related with logic as set theory, quantum physics and computer science.
László Kalmár (1905—1976), who radically reformulated Gentzen's proof of the consistency of arithmetic; it was published in the 2nd edition of the *Grundlagen der Mathematik* by Hilbert and Bernays as "Kalmár's Proof." He made substantial contributions (together with his student János Surányi, 1918—2006) to the decision problem of the first-order logic (determining decidable and undecidable fragments of the first-order language). His writings concerning the philosophy of mathematics are important and interesting, too; he developed a fallibilist view on mathematics and elaborated critical arguments against the Church-Turing thesis.
Rózsa Péter (1905—1977), who had done fundamental work in the theory of recursive functions – she was one of the firsts who acknowledged the importance of this sort of functions as a model for the intuitive notion of final algorithm.
Imre Ruzsa (1921—2008), who elaborated systems of modal and intensional logic with truth-value gaps and formalized a large fragment of Hungarian language in a gappy quasi-Montaguean framework. He reformulated in an especially elegant and powerful manner the theory of canonical calculi as a metalogical theory and made by that a valuable contribution to the circularity problem of the foundations (set theory versus logic, syntactical versus semantical approach to logic).
Mihály Makkai (b. 1939), whose large and world-wide acknowledged life-work embraces category theory, categorial logic and (category-theoretical) structuralist philosophy of mathematics.
István Németi (b. 1942) and *Hajnal Andréka* (1947), who were disciples of Alfred Tarski and did important work with him on the area of algebraic logic. They and their circle are working in the last years on the formalization and logical analysis of relativity theories. They succeeded in formalizing special relativity theory and its several different fragments (so making it clear what does depend on the maximality of light speed in relativity theory and what not years before the disputes brought for by very probably false empirical results).

A.Sch.: What is logic recently? How has its definition changed since Aristotle, the father of logic? Is it science, art, ability? How much recent approaches to defining logic could you notify?

A.M.: For me, logic is primarily a branch of philosophy and a branch of mathematics at once, and – being a historian who not so much writes but rather reads logical works – an object of investigation. But for other people who use logic it is perhaps more an art or ability. I don't think definitions are too much important at such a pre-theoretic or metatheoretic level – the limitations of exactness are obvious. Aristotle doesn't give a general definition that embraces the whole area of the *Organon* although at the beginning of each essay he defines more or less exactly the topic of that special investigation. I think it is a characteristic – and, for me, very attractive – feature of logic that it interferes and overlaps with several other areas. It is due to the central position that logic occupies in the structure of human knowledge. The borders between logic and other areas are elastic, historically changing and not always clear (they needn't to be).

A.Sch.: What is classical logic? Has the difference between classical and non-classical logic any sense still? How much do logical systems exist? How can they be classified? Which logical system is closer to our real thought?

A.M.: The term 'classical logic' has a technical sense: strictly two-valued, extensional logic. There is an important and widely disputed philosophical thesis connected with classical logic in this sense: that it has a distinguished role within the plurality of logical systems. It has adherents and enemies – I'm a moderate, a bit sceptic adherent of the thesis. The distinguished role may consist in that classical logic should be the most general system of connections between truths that there are somehow in the world, independently of our discovering or thinking them. I.e., according to this thesis, logic is primarily about truth and not about thinking, and it is more connected with metaphysics than with epistemology. If we identify classical logic as *the* logic or exaggerate its distinguished role, we must accept radical realistic consequences that I don't want to accept. Nevertheless, classical logic works somehow as a zero hypothesis about the world (and not about our thinking) and in that sense it is unavoidable and hardly exchangeable for some other logic. We do and should study several different logical systems and apply them on different areas but in most cases (even if not always) our metalanguage argumentation about them is governed by classical logic.

A.Sch.: Modern logic is developing now as applied logic above all. Are fundamental logical researches still possible? Where?

A.M.: I agree that within contemporary research in logic, the continuation of the investigations by Gödel or Tarski loses step-by-step its importance and innovative force. Our great ancestors had done the great work and there is not too much to add to it. But logic was never substantially developed on such a cumulative way. The great schools or trends after the originator Aristotle – Stoic, Medieval and mathematical logics – have begun always with a radical change of method and theoretical framework. The medievals and the first mathematical logicians (I mean Leibniz and Boole) hardly did anything more at the beginnings than reproducing the old results within the new (supposition-theoretic resp. algebraic) framework. I think such a change of the framework (I try to avoid using the word 'paradigm') is quite possible. Of course I don't think that mathematical method in logic could be rejected but the Frege-Hilbert-Tarskian calculus-plus-(set-theoretical) semantics construction may be changed by some other mathematical framework. Just some guesses: combinatoric logic or category-theoretic logic – theories that exist for decades now – may gain by some development the role of the general framework of logic instead of being somewhat exotic branches of it.

On the other side, I don't find appropriate the label 'applied logic' for a considerable part of contemporary research. E.g. the above mentioned works in physics-plus-logic or several investigations on the borderline of logic and linguistics are not just applications of logic but characteristically foundational researches. I don’t find anything wrong or derogatory in calling something applied science, but I think applied and foundational research are just different in many respect and I would call these and similar studies interdisciplinary foundational research.

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INTERVIEW
PHILOSOPHY OF SCIENCE IN HUNGARY

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Andrew Schumann: Due to some names such as Imre Lakatos, the Hungarian tradition of philosophy of science became well known. Which contributions of Hungarian philosophers to this subject could you notify as the most important?

Péter Szegedi: The first Hungarian who joined to philosophy of science was *Béla Juhos* (Vienna, 1901–Vienna, 1971), a Hungarian who lived mainly in Vienna. He was a truly logical empiricist member of the inner Vienna Circle all the time. He was interested in the epistemological (erkenntnislogische) foundations of sciences, especially physics. Juhos kept the torch of empiricism alive even after World War II, when – together with Viktor Kraft – he was the central figure of the so-called *Third Vienna Circle*. The discussions here exerted an influence on Paul K. Feyerabend and Wolfgang Stegmüller, as well. One of the main topics of the Third Circle was the problem of realism, which was later transferred to the English-speaking world and turned into a very important field of philosophy of science.

While Juhos was not really well known outside Vienna, there were also such Hungarian philosophers who launched new and important schools in philosophy of science. In chronological order the first was *Karl* (in Hungarian: *Károly*) *Mannheim* (Budapest, 1893–London, 1947), the father of sociology of knowledge. He studied philosophy at the universities of Budapest, Freiburg, and Heidelberg. The subject of his PhD thesis (Budapest, 1918) was already the structural analysis of epistemology. In 1919 – escaping from white terror – he moved to Germany. In 1925, he became privatdocent at the University of Heidelberg, and from 1929, he worked as the professor of sociology at the Goethe University in Frankfurt am Main. In 1933, he moved again, this time to England, where he got a job as the professor of sociology at the London School of Economics and Political Science (LSE). The founding of sociology of knowledge fell on the German period, this was the time when he discussed the basic goals and methods of the new discipline, and when he applied it to some of the questions of the intellectual life of societies.

The most important book of this era is the *Ideology and Utopia*. It begins with the sentence: "This book is concerned with the problem how men actually think". Later Mannheim gives the basic methodical principles for this type of investigation. Not mentioning such relevant notions from the book as "totality," "ideology," "utopia," "relativism," "relationalism," etc., I should only emphasize that the author was also able to apply his methods to relatively actual problems, like in the section "The Political and Social Determinants of Knowledge," where he analyzed the opinions of different political movements on the relation of theory and practice. The sociological turn in philosophy of science occurred only in the 70s, but with explicit reference to Mannheim.

Mannheim's life history in some aspects is similar to that of *Michael* (*Mihály*) *Polányi* (Budapest, 1891–Northampton, 1976). Polányi studied medicine in Budapest, thus served as a physician in the army during World War I. His PhD thesis (Budapest, 1917) was written in physical chemistry. When Mannheim moved to Germany, Polányi did the same. Polányi worked as a chemist at Kaiser Wilhelm Institute in Berlin. According to his son, the Nobel-prize winner John C. Polanyi (born in Berlin, educated in Manchester, living in Toronto), his father was an excellent physical chemist in their common research area (reaction kinetics). Michael Polányi had two other Nobel-prize winner disciples, Eugene (Jenő) P. Wigner and Melvin Calvin. In 1933 – parallel with Mannheim – Polányi moved to England and became the professor of physical chemistry at the University of Manchester. In England, they were in correspondence, they collaborated in publishing Polányi's works (Mannheim was general editor at Routledge), and they discussed different problems in the Moot circle. Some of their views on society and economics were in sharp contradiction, but in philosophy of science both of them fought against positivistic approaches.

Perhaps these discussions lead to Polányi's turn to philosophy of science. Therefore, in 1948 University of Manchester created him a chair of social science. The radically non-mainstream ideas – developed during his stay in England – attained full growth for the beginning of 50s and were published mainly in his *Personal Knowledge*. According to Polányi, beyond our explicit, articulate knowledge, we also have a tacit, inarticulate, unformalizable knowledge, based on experience and practice. By the words of Polányi: "we can know more than we can tell". His expressive examples are riding a bike and swimming, where we cannot say, how we manage it, but we do. We can transfer tacit knowledge to other people only by interactions. Connecting perception to the subject, he claimed that explicit knowledge is founded on the tacit one. Tacit knowledge gives a personal context to epistemology. However, the requirement of objectivity and the personal character of knowledge are not in contradiction with each other. On the contrary, science is only able to approach reality because it is personal. Tacit knowledge and personal commitments lead us to discoveries and theories (or in general, to creative acts), too. Tacit knowledge – as Polányi extrapolates from the Zande tribal beliefs to science – seeks for stability with the help of three kinds of mechanisms: circularity (vicious circles of belief systems), epicyclical (self-expanding) theories, and the principle of suppressed nucleation (prevents the germination of any alternative concepts). Kuhn's paradigm has rather similar features, as Kuhn himself admits (Kuhn sometimes attended Polányi's lectures). Polányi's influence had some role in Feyerabend's turn to the practical side of science as well. Sociologists of science used the concept of tacit knowledge in the 70s.

Undoubtedly, the most widely recognized Hungarian philosopher of science is *Imre Lakatos* (Debrecen, 1922–London, 1974). He began to study mathematics, physics, and philosophy at the University of Debrecen, where his youngest professor was *Árpád Szabó*, who later became a famous historian of mathematics. After the liberation of Debrecen (and later Budapest) by the Red Army, Lakatos went into politics, and participated in the organization of the already legal Hungarian Communist Party. He worked in the cultural, scientific, and educational area of the administration, helped to lay the foundations of the new system (and destroy the old one). Meanwhile, he did not loose his interest in sciences and philosophy, he took part in *Georg* (*György*) *Lukács*' seminar in Budapest. In 1947, he defended his doctoral dissertation in Debrecen with the title *On the Sociology of Concept Formation in Natural Sciences*. In 1949 – because of obscure reasons – he fell into disgrace: one after the other, he lost his scholarship, his job in the Ministry of Education, his flat, his party membership, and finally, he was transferred to the labor camp of Recsk (gulag in North-Hungary) for more than three years.

After his release, he began to work on mathematical problems. He also translated a few books on mathematics to Hungarian, e.g. *György Pólya*'s book on heuristical methods. Recognizing his talent, Alfréd Rényi, founding director of the Institute of Applied Mathematics of the Hungarian Academy of Sciences (HAS) employed him, first as a librarian, then as a researcher. This was the time, when Lakatos – also reading philosophical literature – was acquainted with Popper's philosophy of science. After the 1956 revolution, his situation was hopeless from several points of view, so he left Hungary and rather quickly got a Rockefeller fellowship in philosophy at the University of Cambridge. The title of his PhD thesis was *Essays in the Logic of Mathematical Discovery*, and the content bore marks of Pólya's and Popper's thinking. *Proofs and Refutations*

was developed from this paper. In 1960, Lakatos went to Popper's department at LSE. He became a radical anti-Marxist and Popperian. After a while, he broke with Popper's philosophy of science and formed his own one, the so-called "methodology of scientific programmes." Because of his early death, his "big book(s)" have not been written, but we have some long papers from which it is possible to reconstruct his views. Based on these reconstructions, some colleagues consider Lakatos the most talented philosopher of science in the 20th century.
The roots of Lakatos's own philosophy of science can be found partly in Pólya's heuristics, but he was also influenced – straining through the Lukácsian filter – by the Hegelian and Marxist ideas, and the communist political practice in Hungary. The motivation was perhaps to take back – at least in some measure, after all the hard core is a variation of the paradigm – the Kuhnian turn with its (apparently) irrational and non-explicable revolution, which interrupts the development. In favor of this, he introduced the competing research programs as historical series of theories, linked by a common hard core and heuristics. This core is irrefutable as the result of the functioning of the negative part of heuristics. The scientific research program has positive heuristics, too, which give ideas for the further developing of theories. For the development of the research program, it is necessary to eliminate the old theories via "minor crucial experiments", while "major crucial experiments" decide between two competing research programs. According to Lakatos, these latter kinds of experiments are seen – only with hindsight, occasionally several decades later – to have been crucial, actually they do not exist in falsificationist sense.
Lakatos used the criteria of progressivity and degenerativity for solving the problem of elimination of research programs. Of course, the evaluation of a program can change with time, and Lakatos wanted to evaluate the programs only in the long run. Lakatos also applies progression as a metacriterion for rationality and for the models of philosophy of science itself (of course, based on this criterion, his methodology gives a wider rational reconstruction for history of science than the rival models made by Popper, Polányi, Kuhn, Feyerabend, or Toulmin). The importance of Lakatos's work for the philosophy of science community is demonstrated by the fact that the most prestigious prize of the profession bears his name (Lakatos Award).

A.Sch.: Which Hungarian schools of philosophy of science are still heavyweight? What are their results?

P.Sz.:*Márta Fehér*, who became more or less the master of the whole recent generation working on these problems, founded the first group on philosophy of science in Hungary. She exerted the upmost influence on Hungarian philosophy of science. She began her researches in history of science (especially on the 17th century), but always with erudition in philosophy of science. She translated Newton's *The System of the World* and the Bentley-letters. Turning to philosophy of science, she has never truly committed to any school. Some Popperian influence can be detected on the first Hungarian philosophy of science textbook written by Fehér and *László Hársing* in 1976. Then her favorite is Kuhn, she wrote for example the afterword to the Hungarian edition of *The Structure of Scientific Revolutions*, and published a book on incommensurability (in Hungarian). The analytic approach was never alien to her, but she likes the sociological approaches (especially the "strong program"), as well. In the last decade, she has also supported the hermeneutic approach. Besides research, she has always given lectures and not only in the regular way. In the 80s, she organized a philosophy of science circle – with *Imre Hronszky* and *Tibor Szécsényi* – for the interested colleagues, and she was a leading figure in the one-week courses of the Center of Continuing Education for Philosophy Teachers. The young Hungarian philosophers were acquainted with theories of scientific development, i.e. with Feyerabend, Lakatos, Laudan, Toulmin, etc. through her lectures and papers. Márta Fehér also contributed to the propagation of philosophical culture by her translations of Neurath, Schlick, Carnap, Peirce, and Berkeley.
As Fehér worked at the Department of Philosophy (now Department of Philosophy and History of Science) of Budapest University of Technology (now Budapest University of Technology and Economics), one of the philosophy of science centers was formed there. Fehér's co-worker in this

area was Hronszky, who had come from the Department of Philosophy (now Department of History and Philosophy of Science) at the Faculty of Natural Sciences of Loránd Eötvös University of Science (ELTE). He was interested in philosophy, sociology, and history of science and technology. Later on, he also took up innovation research and management, and founded the Department of Innovation Studies and History of Technology at his university. Some of Fehér's engineer students converted to philosophy of science, e.g. *Tihamér Margitay*, who is the head of the department recently. In 1998, they successfully started a PhD school in history and philosophy of science, the only one existing in Hungary. The school is a well-functioning one; it has 5 – 10 new students every year, so young blood is guaranteed, there are talented young people at the department, as *Benedek Láng*, *János Tanács*, and *Gábor Zemplén*.

Another center formed from the above-mentioned HPS department at the Faculty of Natural Sciences of ELTE. From the middle of the 70s, the average Marxist philosophy department was gradually filled in with young people coming from natural sciences, mainly from physics. Of course, the latter small group was interested in philosophy of physics at first, but later on, one or other also reached general problems in philosophy of science, history of physics, sociology, and hermeneutics of science, etc. *Miklós Rédei* (at LSE since 2007) and I began with researches in quantum mechanics, Rédei rather from a logical and mathematical, while me from a philosophical and historical point of view, but we had common areas of interests, too. Rédei specialized in the problem of non-existence of hidden parameters in quantum mechanics first, then in Reichenbach's common cause theory. On the latter subject, he worked together with *László E. Szabó* (who had a job at the department at the end of the 70s, but later on, he took a job at the Theoretical Physics Research Group of HAS, and recently, he is a professor at the Department of Logic at the Faculty of Humanities of ELTE), and with Rédei's student, *Gábor Hofer-Szabó*. In the last decade, Rédei was engaged – among others – with the heritage of John von Neumann.

László Ropolyi was concerned with the philosophical problems of thermodynamics first, but his range of interest has broadened, and now he deals, among others, with the problems of computers and Internet, and with the philosophical, sociological and hermeneutical approaches to science. After the regime change, the department was transformed – in a long process – into the Department of History and Philosophy of Science, and *György Kampis* was invited to lead it. He came from biology and broke into philosophy of science with his book on self-modifying systems, which had a part on epistemology. Since, he has been interested in complexity, the mind-body question, different aspects of the evolution theory (he newly translated Darwin's *The Origin of Species* into Hungarian), new approaches of classical problems like causality, and modeling biological systems. *Gábor Kutrovátz* is the youngest member of the group; he began his studies with the philosophical and historical aspects of the "heat death" concept, then he dealt with Lakatos and Árpád Szabó in the context of the modern history of philosophy of science and mathematics, and recently he wrote on the epistemology of Science War and conflicts between sciences and "pseudosciences."

Besides the two universities, the third center for philosophy of science is the Institute of Philosophy of HAS. Earlier, there was a largish group for epistemology, but it has been ceased and now only a few members of the institute constitute a group for philosophy of science. *Vera Békés*, coming from linguistics, expanded the Kuhnian theory of scientific development, called the "missing paradigm" model. According to Békés, there are not two, but three paradigms, in the process of scientific revolution, one of them, as an "island", or "inclusion" coming from the earlier stage of development. In her opinion, this model is able to keep the incommensurability thesis and discontinuity, but can solve their paradoxes and fits more to the real history of science than the original Kuhnian theory. Her example for the "missing paradigm" is the Humboldtian program for science at the University of Göttingen (Georg-August-Universität) from the end of the 18th century until the first decades of the 19th century. Unfortunately, Békés' book was published only in Hungarian, and her short English paper appeared only in the periodical of the Institute of Philosophy of HAS, hence the international community of philosophers has not recognized it. *János Laki*, among others, has been also dealing with Kuhn, and published a book on his researches (in Hungarian). In 2010, during the – politically motivated – debates in the Institute he was fired out

based on faked reasons, recently he became a member of Forrai's department, which will the last, I will mention. Another member of the group, *András Benedek* was interested in the philosophical and historical problems of mathematics. *László Székely* works on the philosophical-epistemological background of relativity theory and modern cosmology, the hermeneutic approach of sciences, and the relationship between science and religion (anthropic principle and intelligent design). The Institute was recently merged to a huge Research Institute of Humanities, further dismissals are expected, so the little group is in danger.
Gábor Forrai at the Department of Philosophy at University of Miskolc founded a relatively newly formed philosophy of science group. Forrai received his PhD degree at University of Notre Dame, Indiana, where he studied scientific realism. First, he worked at the Institute of Philosophy of HAS, later he went to University of Miskolc to teach, and now he is the director of the Institute of Philosophy there. He applies the analytic tradition in a wide sense and he is mainly interested in realism, Locke, and the theory of mind. He has translated many papers into Hungarian including Lakatos' most important essays. In his Institute, work two other philosophers of science, *Gergely Ambrus* and *Tamás Demeter*, both mainly engaged in philosophy of mind, as well. Demeter graduated already at Miskolc, and so did *László Nemes*, who is now working at the Institute for Behavioral Sciences, University of Debrecen in the fields of philosophy of biology, philosophy of ethology, evolutionary psychology, and bioethics.

A.Sch.: The Maecenas George Soros is the best known Hungarian the world over. How can you estimate his role in the transition from communism to capitalism in post-Socialist countries and in the development of Hungarian contemporary science and education?

P.Sz.:George (*György*) *Soros* came (back) to Hungary in 1984. One of its first actions was raining free copy machines on Hungarian libraries and universities. In a country, where to all copy machines belonged a responsible person, whose name was given down to the Ministry of Internal Affairs. This was a very smart tactics for loosen up the system.
The aim of his educational programs was also – at least partly – to build an open society. For instance, several hundreds of English teachers could take part in 5 weeks courses in the USA. They strengthened their English, learned new teaching methods and were acquainted with a country, which was earlier impossible for most of them. Coming back, they started to spread their experiences.
The Soros Foundation played a very important role in the development of Hungarian science, partly with the support of libraries, and especially through improvement of the international connections. It provided some scholarships for young Hungarians – for instance, the present prime minister of Hungary could spend a few months in Oxford – and offered numerous grants for scientific tourism, too, mainly for participating on conferences.
Concerning philosophy of science, Soros founded "The Popper Project" affiliated with the Central European University, founded also by Soros in Budapest (later on the Project moved to Vienna). The aim of the Project was not only the Popper research, but also to publish Popper's manuscripts (see e.g. "The Myth of the Framework", "Knowledge and the Body-Mind Problem"), and to translate his books into many languages. The Project organized dozens of workshops and summer schools for translators (among them many philosophers) of East Europe and FSU. The Soros Foundation, the Central European University, and Soros' Open Society Foundation assumed – at least partly – the financial support for these translations.

A.Sch.: Why are so huge and expansive projects like Large Hadron Collider implemented? Can we expect that this collider will give us absolutely new data causing a scientific revolution? What do you think if the God particle, Higgs boson, will be detected some day or other in fact?

P.Sz.: Why physicists should like to build larger and larger equipments? – This is not a question. The more interesting question is: Why do politicians give so huge amounts of money for these

projects? I know two possible answers from the 70's. The first came from some counter-movements of science. Those left-wing sociologists told that the politicians wanted to withdraw the best minds from the society. "Let the children play!" They do not make revolution meanwhile. The second follows from an idea by Fred Hoyle, the astrophysicist and science fiction writer. In an article he wrote, that the complexity of particle accelerators had reached the complexity of society. Who are experts in operating an accelerator? Of course the particle physicists. Therefore, we have to give the power or the administration to the particle physicists. If Hoyle's argumentation was valid, than the LHC is nothing other than a training school for managers of the world, and it is worth the price. I think a third answer is nearer the truth. The keyword is the nationalism, even in its internationalist form. In the middle of 80's I have seen a documentary, titled "The Geneva Event". The subject was the constructing the forerunner of LHC, the proton-antiproton collider at CERN. This equipment gave the possibility to Carlo Rubbia to find the W and Z bosons and to win the Nobel Prize. The film was like a victorious production report from a newsreel of any socialist countries (only much longer). The film and the project itself was a kind of promoting the European Communities (including the European Economic Community and the European Atomic Energy Community) which was after the first and second and before the third enlargement. The common scientific enterprise symbolized the economic, political, and cultural unity and superiority of the participating nations (this is the international nationalism). Therefore, the goals of the project were almost the same, as in the case of the Apollo Program, for instance. The particle physics is not so impressive than the space programs, but proportionally cheaper, too. Nevertheless, it is undeniable that some politicians recognized that the support of basic research could strengthen development of industry; that the big projects with obscure objects, like Higgs-bosons, may have useful side shoots (Rubbia's new concept on the safe nuclear power reactor, using a particle accelerator-driven system; medical diagnostic and therapy techniques); that the international cooperation improves the climate for the foreign policy; etc.
Now one can say this moment the results of the LHC experiments. Its effect on physics depends on the mass of the discovered particle(s) – Higgs-boson(s), other particles, or nothing new. Probably it will simply confirm the standard model of particle physics. Even if it will be the case, the certainty can encourage physicists to elaborate the further details of the model. In other cases, they have to modify the theory. However, in my opinion one experiment (maybe only with a few events, like in the Rubbia-story) will not cause a scientific revolution. Speaking with Lakatos (see the first question), the negative heuristics will defend the core of the standard model (remember the quark confinement idea, which was at that time a typical *ad hoc* hypothesis in philosophers view), and we shall be able to evaluate the status of this experiment only later.

A.Sch.: Physics is usually considered as the most exact science. It is maintained that it is built up on mathematics and hence it does not depend on common sense and other unscientific knowledge. Is physical knowledge so pure? How does physics depend on social competencies and social behaviors as a whole?

P.Sz.: It is a difficult and debated problem, but I think the sociology of science (or the sociology of scientific knowledge) have some convincing – and some not so convincing – demonstration of the embedding of physics to the structure of society. First, the scientific knowledge does not come from ivory tower. For instance, the Marxist Boris Hessen already in 1931 tried to describe the social and economic roots of Newton's *Principia*, and in my opinion, Robert Merton partly justified it. Paul Forman's attempt at attributing the acausality of quantum mechanics to the hostile intellectual environment of the Weimar Republic was clearly not so successful. Nevertheless, the intellectual environment plays a role in the accepting the different scientific theories, as well. I think Gideon Freudenthal's analysis on the acceptance of Newtonian ideas and the rejection of Leibniz is a good example for the influence of society to the theory choice – of course through the scientists, as the agents of the society. The Strong Program in sociology of science, the Empirical Program of Relativism, and the social constructivist approaches proved that micro sociological tools could examine the science, as well. Their case studies showed that the new empirical data could be interpreted in different ways; that behind the different interpretations there are different personal histories, different local traditions. In the end, the debates will be closed, but the course of a debate may be determined by the previous agreements of representatives of the special discipline; by the authority structure of science; by individual and group interests. Further fact, which casts a doubt on pure rationality of physics, is the presence of tacit knowledge (see the Polányi part of the first question).

However, these results do not query, that the physics is the most exact science, build with the help of highly developed equipments and of mathematics, because all the other sciences (astronomy, chemistry, life sciences) are influenced by extra scientific factors (economy, politics, religion, philosophy, etc.), too. Unfortunately, the mentioned sociological case studies did not get together to a general theory of science, so the debate will go on.

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INTERVIEW
P-ADICS: MATHEMATICS FOR SIGMUND FREUD?

Andrei Khrennikov studied at Moscow State University, Department of Mechanics and Mathematics, in the period 1975–1980. In 1983 he received a PhD in mathematical physics (quantum field theory) from the same department. He started his teaching and research career at Moscow University for Radio-Electronics and Automatics and in 1990 he became full professor at Moscow University for Electronic Engineering. He started his career abroad at Bochum University and since 1997 he is professor of applied mathematics at Linnaueus University, South-East Sweden, since 2002, the director of the multidisciplinary research center at this university, International Center for Mathematical Modeling in Physics, Engineering, Economics, and Cognitive Science. His research interests are multi-disciplnarity, e.g., foundations of quantum physics and quantum information, foundations of probability (in particular, studies on negative probabilities), cognitive modeling, ultrametric (non-Archimedean) mathematics, dynamical systems, infinite-dimensional analysis, quantum-like models in psychology, economics and finances. He is the author of about 400 papers and 16 monographs in journals in mathematics, physics, biology, cognitive science, economics, and finances.

Andrew Schumann: According to Galileo Galilei’s famous claim, the book of nature is written in the language of mathematics. Hence, mathematics has been regarded as cornerstone tool in physics since Galilei. His claim is self-evident for physicists till now, but not for philosophers. What do you think how far math can be applied in cognitive sciences? If there are any limits?

Andrei Khrennikov: One of the sources of the extremely successful mathematical formalization of physics was the creation of the adequate mathematical model of physical space, namely, the Cartesian product of real lines. This provides the possibility for “embedding” physical objects into a mathematical space. Coordinates of physical systems are given by points of this space. Rigid physical bodies are represented by geometric figures (cubes, balls, etc.). By describing dynamics of coordinates, e.g., with the aid of differential equations, we can describe dynamics of bodies (from falling stones to Sputniks). For 15 years I have advocated a similar approach to description of mental processes in cognitive sciences and psychology (and even information dynamics in genetics).
Similar to physics, the first step should be an elaboration of a mathematical model of *mental space.* This is a problem of huge complexity and it might take a few hundred years to create an adequate mathematical model of mental space. I recall that it took three hundred years to create a mathematically rigorous model of real physical space. In works many critical arguments have been presented against the real model of space as a possible candidate for a mental space. One of the main arguments was that the real continuum is a continuous infinitely divisible space. Such a picture of space is adequate to physical space (at least in classical physics), but *mental space is not continuous:* mind is not infinitely divisible! Another problem with the real continuum is that it is homogeneous: “all points of this space have equal rights.” In opposition to such a homogeneity, mental states have clearly expressed *hierarchical structure.* Therefore a model of mental space that we are looking for should be (at least) *discontinuous* and *hierarchical.* Such models of space were recently invented in theoretical physics. These are non-Archimedean (ultrametric, *p*-adic) physical space.

A.Sch.: What is *p*-adic mathematics and *p*-adic physics? What reasons were to start using non-Archimedean space in physics?

A.Kh.: Special ultrametric number systems, *p*-adic numbers, have been successfully used in quantum physics and string theory (V. S. Vladimirov, I.V. Volovich [10], E. Witten, P. Frampton,

G. Parisi, I. Aref'eva. A. Khrennikov [4], and so on). In this approach the main role was played by non-Archimedean features of these systems, violation of Archimedean axiom (which was interpreted as an axiom of measurement theory). The possibility to encode hierarchy by ultrametric was explored in physics of disordered systems (R. Rammal, G. Toulouse, M. A. Virasoro and then G. Parisi, S. Kozyrev, A. Khrennikov [7]), in particular, in physics of proteins (V. Avetisov, S. Kozyrev, etc.), image analysis (J. Benois-Pineau [2], A. Khrennikov, and others), modeling of information processes in complex cognitive and social systems and multivariate data analysis, clustering, data mining (F. Murtagh [8], A. Khrennikov [3]), computer science and cryptography (V. Anashin [1]), bioinformatics.

A.Sch.: What is better in simulating real processes the conventional mathematics or the *p*-adic one? Is reality *p*-adic or conventional?

A.Kh.: In 1994 (in a series of papers in *Theoretical and Mathematical Physics*) two researchers from Steklov Mathematical Institute of the Russian Academy of Sciences started to speculate about a possibility to use *p*-adic numbers in theoretical physics. They speculated that at the fantastically small Planck time and space scales the conventional model of space-time based on real numbers is not applicable. Instead of the real continuum, we have to use *p*-adic space combining discreteness with a new type of continuity – with respect to the corresponding ultrametric. One of important features of this space is violation of the Archimedean axiom which was interpreted by V. Vladimirov and V. Volovich as an axiom on existing of non-commeasurable physical quantities. In 1989 I joined the research group of Vladimirov and Volovich. My interests were in quantum models with wave functions taking values in the fields of *p*-adic numbers and their algebraic extensions, quadratic as well as extensions of higher orders. One of distinguishing features of quantum mechanics with *p*-adic valued wave functions is boundedness of the basic quantum operators, e.g., position and momentum or creation and annihilation operators. As a consequence, Hamiltonians are also bounded. This is valid even for systems with infinite number of degrees of freedom, quantum field theory with *p*-adic valued quantum fields. Another unexpected feature of *p*-adic quantum mechanics is existence of nonequivalent representations of canonical commutation relations in the case of finite number of degrees of freedom.
However, the most intriguing consequence of the usage of *p*-adic numbers for quantization is the appearance of *p*-adic valued probabilities.

A.Sch.: What is non-Kolmogorovian probability theory you are dealing with and how does it connect to *p*-adic mathematics and physics?

A.Kh.: To solve the interpretational problems of *p*-adic quantum mechanics (in the model with *p*-adic valued wave functions), there are defined rigorously *p*-adic probabilities by extending von Mises approach to the *p*-adic case. The starting point is the evident fact that experimental data, including probabilistic data, are always rational. As a consequence of finite precision of any measurement and finite time which can be used to collect data, only rational numbers can be produced by experimenters. In particular, relative frequencies of realizations of events are always rational numbers. On the field of rational numbers von Mises considered the topology which is induced from the field of real numbers. Probabilities were defined as the limits (if they exist) of sequences of relative frequencies. This simple definition (the principle of statistical stabilization of relative frequencies) was combined with rather contradictory definition of a random sequence, collective.
Formalization of the notion of randomness attracted a lot of interests in communities of probability theory and logic. In particular, Kolmogorov proposed theory of algorithmic complexity. I proposed to consider one of *p*-adic topologies on the field of rational numbers containing all relative frequencies of realization of events. Probabilities were defined as the limits (if they exist) of sequences of relative frequencies. It can happen that, for one prime *p*, the limit and hence

probability exists, but for another not; it can happen that it does not exist in the ordinary meaning, i.e., with respect to the real topology, but exists for one of *p*-adic topologies. Hence, the absence of probabilistic regularities in the ordinary sense does not imply that there are no probabilistic regularities, *p*-adic probability may exist. Notice that *p*-adic probability theory is used now in *p*-adic quantum physics and biological modeling (see [5, 6]).

One of interesting applications of *p*-adic probability is a possibility to justify usage of "negative probabilities." Such "probabilities" appear regular in quantum physics. Dirac actively used such "probabilities" to quantize (relativistic) the electromagnetic field. Feynman also applied negative probabilities to quantum foundations. Some authors noticed that Bell's inequality (the fundamental test of compatibility of local realism and quantum formalism) can be violated in classical physical models, but under the assumption that hidden variables can have negative probability distributions. It is impossible to justify usage of negative probabilities in the classical (Kolmogorovian) probabilistic model. To provide a frequency interpretation (which is only useful for practice) is especially difficult.

In the framework of *p*-adic probability theory negative (rational) probabilities were defined in the mathematically rigorous way. Negative rational numbers (as well as all rational numbers) can be embedded in any field of *p*-adic numbers. If a sequence of relative frequencies for trials for some random event converges in the *p*-adic topology (for some prime number *p*) to a negative rational number, this number is by definition the negative probability of this random event. The same negative rational number can be embedded in various fields of *p*-adic numbers. Its probabilistic meaning depends on the prime number *p* and topology of statistical stabilization.

A.Sch.: Have you ever met Andrei Nikolaevich Kolmogorov, the founder of axiomatic probability theory?

A.Kh.: Yes. First of all, I met him regular as a student during the course on mathematical logic which he gave to us, students of the Dept. of Mechanics and Mathematics of Moscow State University. At one occasion (the submission of a note to "*Doklady of Academy of Science of USSR*", DAN), see [9], I, the first year graduate student, was introduced (by my supervisor, Prof. Smolyanov) to Andrei Nikolaevich Kolmogorov who at that time was physically very disable, but mentally bright. The paper (joint with Prof. Smolyanov) was devoted to a generalization of probability theory to complex valued probabilities. It attracted interest of Kolmogorov. In the discussion on the main ideas of this paper he stressed the role of the frequency definition of probability by von Mises and related logical problems. This conversation played a crucial role in forming of my interest to foundations of probability theory, especially the frequency definition.

Andrei Nikolaevich asked for a copy of the note for his personal use. At that time the only possibility to make a copy was to tape the paper once again – the use of copy-machines was under the strong control. It is a pity, but the graduate student Andrei Khrennikov was so busy with his own "very important tasks" that Andrei Nikolaevich had never got a possibility to study this paper in details. The Editors of DAN sent the note recommended by Kolmogorv to a reviewer who wrote a negative report with motivation that the axiomatics of probability theory had been established in 1933 and there are no reasons to try to generalize this axiomatics. Of course, the editors did not accept such a report written on the paper recommended to publication by the creator of the axiomatic of probability theory. So, finally the paper was published only three years later. At that time Kolmogorov was already very disable physically and only his former students had possibilities to visit him. Albert Nikolaevich Shyryaev told that Kolmogorov was still mentally active; Shyryaev read him whole days mathematical books.

A.Sch.: How ultrametric can be used in cognitive science?

A.Kh.: It should be stressed here that actually the *new approach based on ultrametricity enables one effectively use "continuous" methods to study "discrete" problems*, both of theoretical and applied origin. This is the core of leading scientific idea of my research.

The simplest class of ultrametric spaces is given by homogeneous *p*-adic trees (here *p* is a prime number giving the number of branches of a tree at each vertex). It is interesting that such trees are nicely equipped: there is a well-defined algebraic structure which gives the possibility to add, subtract, multiply, even divide branches of such a tree. There is a natural topology on such trees encoding the hierarchic tree structure. This topology is based on a metric, so called *ultrametric.* Thus *p*-adic trees are not worse equipped than the real line. However, the equipment – algebra and topology – is very different from the real one. I proposed to choose *p*-adic trees as possible models of *mental space*: points of this space – *mental points* – are branches of a tree. It is possible to encode tree's branches by sequences of numbers. These are *mental coordinates* representing *mental points.* By using mental coordinates we are able to embed into the space mental analogs of physical rigid bodies – *basic categories (special associative classes) and ideas.* They are represented, respectively, by *balls and collections of balls* in the ultrametric mental space. Association-relation (which is an equivalence relation for mental points) is based on ultrametric. The use of ultrametric is crucial! In what following we call basic categories simply by categories. But we emphasize that these are specific categories coupled to mental ultrametric.

Mental points (represented by branches) are the *elementary mental entities.* A category is represented as a subset of the mental space. The crucial point is that the associative coupling of mental points is fundamentally hierarchical. Therefore a category is not an arbitrary set of mental points, but a hierarchically coupled collection.

Since in our model the mental hierarchy is encoded by the topology of the mental space, it represents the associative coupling of mental points into balls. A larger ball couples together more mental points. Thus it is a more general category. Decreasing of a ball's radius induces decreasing of generality of a category which is represented by this ball. It becomes sharper. In the limit, we obtain the ball of zero radius. That is nothing else than a single mental point (the center of such a degenerated ball). This is the limiting case of a category. We remark that the real brain produces *finite mental trees.* For such a finite tree, each point (its branch) is simply a ball of finite radius (it is determined by the size of the tree). It is a trivial associative-class: the mental point associated with itself. However, consideration of idealized mental spaces based on infinite trees is an extremely useful mathematical abstraction – as well as consideration of continuous real line or plane. In principle, even in physics one can work on e.g. discrete plane. However, the real analysis is developed for its continuous idealization. Therefore it is convenient to work on the real "continuous plane." In the same way a powerful ultrametric analysis was developed for a (special) class of infinite trees and it is convenient to use such spaces for mathematical modeling, e.g., in psychology or cognitive science.

I applied such an approach to mathematical modeling of Freud's theory of interaction between unconscious and conscious domains. One of basic features of my model is splitting the process of thinking into two separate (but at the same time closely connected) domains: *conscious* and *unconscious*. I use the following point of view on the simultaneous work of the consciousness and unconsciousness. The consciousness contains a *control center CC* that has functions of control over results of functioning of subconsciousness. *CC* formulates problems, and sends them to the unconscious domain. The process of finding a solution is hidden in the unconscious domain. In the unconscious domain there work complex dynamical systems – *thinking processors.* Each processor is determined by a function *f* from mental space into itself (describing the corresponding feedback process – psychological function). It produces iterations of points of mental space. These intermediate mental points are not used by the consciousness. The consciousness (namely *CC*) controls only some exceptional moments in the work of the dynamical system in the unconscious domain – attractors and cycles. Dynamics of mental points induce dynamics of mental figures, in particular, ball-categories and, hence, ideas (collections of balls). The crucial point is that behaviors of the dynamical in the mental space and its lifting to spaces of categories and ideas can be very

different. Extremely cycling (chaotic) behavior on the level of mental points (and even categories) can imply nice stabilization to attractors on the level of ideas. Therefore it is profitable for the brain (modeled in this framework) to use as solutions of problems attractors on the level of ideas and not mental points (and categories).
The computational ("thinking") machine described in my works represents an *unconventional computation in ultrametric mental space.* I notice that the statement that Turing machines completely express the intuitive notion of computing is a common misinterpretation of the Church-Turing thesis. For instance, Turing asserted that Turing machines could not provide a complete model for all forms of computation, but only for algorithms. Therefore he defined choice machines as an alternative model of computation, which added interactive choice as a form of computation, and later, he also defined unorganized machines as another alternative that modeled the brain.

A.Sch.: Are you the first who proposed to use math in explicating conscious and unconscious ideas?

A.Kh.: One of the first scientists who proposed a mathematical simulation of processing mental information was the famous 19th-century philosopher Johann Friedrich Herbart (1776 – 1841). His model was based on real analysis and classical mechanics (e.g., see his *Psychologie als Wissenschaft* written in 1824 – 1825) and it assumed the difference between conscientious and unconscious ideas too.

A.Sch.: Where can your *p*-adic cognitive science be applied?

A.Kh.: In series of models I considered cognitive systems with increasing complexity of psychological behavior determined by structure of flows of categories and ideas. Using this basic conceptual repertoire an increasingly refined cognitive model is developed starting from an animal like individual whose sexual behavior is based on instincts alone.
At the first step a classification of ideas to interesting and less interesting ones is introduced, and less interesting ideas are deleted. At the next level a censorship of dangerous ideas is introduced and the conflict between interesting and dangerous leads to neurotic behaviors, idees fixes, and hysteria. These aspects of the model reflect more the general structure of conscious/unconscious processing rather than properties of *m*-adic numbers. I stress again that the basic mathematical structure for this model is mental ultrametric space. In particular, ultrametric is used to classify ideas – to assign to each idea its measures of interest and interdiction.
One of my interests is creation of *psycho-robots*, exhibiting important elements of human psyche, including the presence of two blocks: unconscious and conscious. Creation of such psycho-robots may be useful improvement of domestic robots. At the moment domestic robots are merely simple working devices (e.g. vacuum cleaners or lawn mowers). However, in future one can expect demand in systems which be able not only perform simple work tasks, but would have elements of human self-developing psyche. Such *AI*-psyche could play an important role both in relations between psycho-robots and their owners as well as between psycho-robots themselves. Since the presence of a huge numbers of psycho-complexes is an essential characteristic of human psychology, it would be interesting to model them in the *AI*-framework.

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INTERVIEW

IS EVERYTHING A COMPUTATION?

Andrew Adamatzky is Professor in Unconventional Computing in the Department of Computer Science, amir of the Unconventional Computing Centre, and a member of Bristol Robotics Lab. He does research in reaction-diffusion computing, cellular automata, physarum computing, massive parallel computation, applied mathematics, collective intelligence and robotics. He is one of the founders of unconventional computing thinking in natural sciences.

Andrew Schumann: What is unconventional computing? How does it differ from other approaches to computation?

Andy Adamatzky: Usually, in answering this question I could not resist quoting Tomasso Toffoli: "... a computing scheme that today is viewed as unconventional may well be so because its time hasn't come yet — or is already gone." This means that everything flows and nothing stays the same, e.g. at the time of analogue computers digital ones were considered unconventional, but nowadays they top the charts of modern unconventional computing devices.

The simplest explanation what the conventional computer is may be as follows. A conventional processor converts all the inputs of keyboard into binary numbers (0s and 1s in the form of electrical pulses) and from then on its function involves the movement and transformation of these pulses in simple electrical circuits. Its advantages lie in the fact that it has millions of such circuits operating at high speed and can thus 'compute' outputs very quickly.

However, the conventional processor has rigid limitations. Obviously, if the basic technology behind this conventional computation had endless development potential then any limitations might be easier to ignore. Nevertheless, there is a consensus that current methods will indeed reach a threshold and this has led to an explosion in research into unconventional methods of computation.

The main limitations are in that conventional processors compute in a serial manner whereas biological and natural information processing seems to be predominantly via parallel mechanisms. Conventional processors are hard wired while unconventional ones are soft-, chemical- and molecular-based devices. Conventional computers are fragile, in a sense that damaging one component will usually halt the work of the whole machine, and unconventional ones are 'self-healing', re-constructible, due to the behavior of the physical matter they are built of.

Recently, unconventional computing is a huge area of joint researches of computer scientists, biologists, physicists, etc. The subjects commonly addressed in unconventional computing research are as follows now:

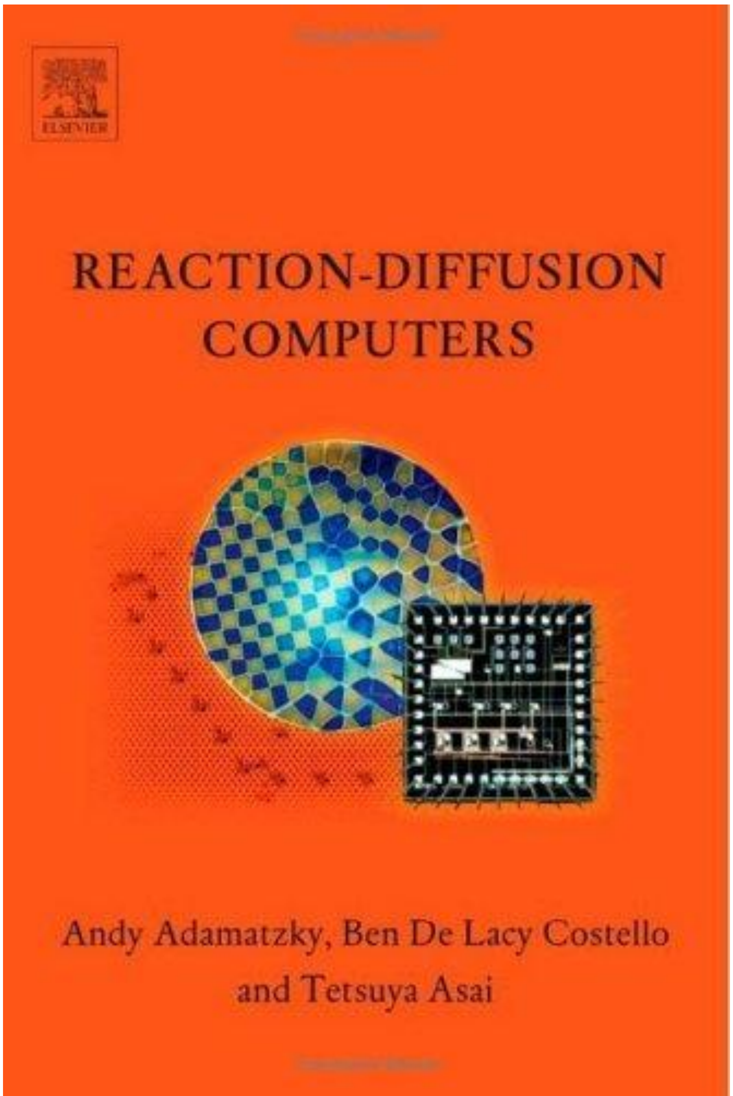
• *cellular automata* (the way of designing models, topological conformity to natural spatially extended systems and huge potential to exhibit all types of complex behavior with simple local transition rules);

• *biological and molecular computing* (conformation-based computing, DNA computing, information processing in micro-tubules, molecular memory, biochemical computing, artificial chemistry, etc.);

• *chemistry-based computing* (amorphous computing, implementation of logical functions, image processing and pattern recognition in reaction-diffusion chemical systems and networks of chemical reactors);

• *hybrid and non-silicon computation* (plastic computers, organic semiconducting devices, neuronal tissue-silicon hybrid processors);
• *logics of unconventional computing* (logical systems derived from space-time behavior of natural systems, non-classical logics, logical reasoning in physical, chemical and biological systems);
• *physics-based computation* (analogue computation, quantum computing, collision-based computing with solitons);
• *stigmergic and population-based computing* (optimization in cellular cultures, computing in societies of social insects, ecological computing);
• *smart actuators* (molecular motors and machines with computational abilities, intelligent arrays of actuators, molecular actuators, coupling unconventional computing devices with arrays of molecular or smart-polymer actuators).

Thus, the research in unconventional, or nature-inspired, computing aims to uncover novel principles of efficient information processing and computation in physical, chemical and biological systems, to develop novel non-standard algorithms and computing architectures, and also to implement conventional algorithms in non-silicon, or wet, substrates.
To sum up, in a word it is difficult to draw a clear borderline between 'conventional' and 'unconventional' computing. As I said, analog computation was conventional half-a-century ago, then digital computing came to power, and now the analog computation is 'unconventional.' Memristors is another good example here. Memristors (resistors with memory proposed by Leon Chua in 1973) were purely theoretical curiosities years ago. When fabricated in HP Labs in 2008, they became 'unconventional' for a year or two. Now hundreds of companies are producing memristors, and thousands of papers are published on memristors. Are memristors 'unconventional' or 'conventional' recently? For some guys 'unconventional' means quantum computing, for others reaction-diffusion chemical computing, for others ant-based algorithms and so on.



A.Sch.: What is provided by combining physics, chemistry and biology within the theory of computation?

A.A.: Novel principle of information processing, e.g. wave-based computation and collision-based computing, and novel substrates for computation. But, most importantly, years of fun and entrainment by playing with unknowable and exploring the ways the Mother Nature thinks.

A.Sch.: What is reaction-diffusion computing that was proposed by you? Which features distinguishing it from other unconventional computing approaches does it have?

A.A.: A reaction-diffusion processor is a real chemical medium, usually composed of a thin layer of solution or gel containing chemical reagents, which in its space-time dynamics transforms data to results in a sensible and programmable way. Data, to be processed, can be represented by the concentration of certain reagents and spatial structures, e.g. diffusive or excitation waves, spread from these initial data points. The spreading patterns interact to produce either stationary structures, e.g. a precipitate concentration profile, or dissipative structures, e.g. oscillating patterns. The final state, or even just a particular spatial state of the whole medium, represents a result of the reaction-diffusion computation. The spreading of waves is analogous to information transfer. And, the interaction of diffusive or phase waves realizes the computation.

An important attribute of this mode of computation is that there is an absence of a rigid hardware-like structure. Essentially, the 'liquid' processor has an 'amorphous' structure which may be considered as a layer of micro-volume reaction-diffusion chemical processors capable of massive parallelism. Characteristic advantages of reaction-diffusion processors include parallel input of data (usually, via the spatial distribution of the reactant concentrations), massively parallel information processing (typically, via spreading and interaction of either phase or diffusive waves) and parallel output of results of the computation (commonly, the results are represented by patterns of reactants or a colored precipitate that enables the use of optical reading devices).

These features together with the relative ease of laboratory experiments (most reactions occur at room temperature and do not require any specialist equipment), constructional simplicity of formal design (all reaction-diffusion systems are well simulated in two-dimensional cellular automata) and the pleasure of parallelism per se make reaction-diffusion chemical processors an invaluable tool for developing advanced unconventional parallel computing architectures.

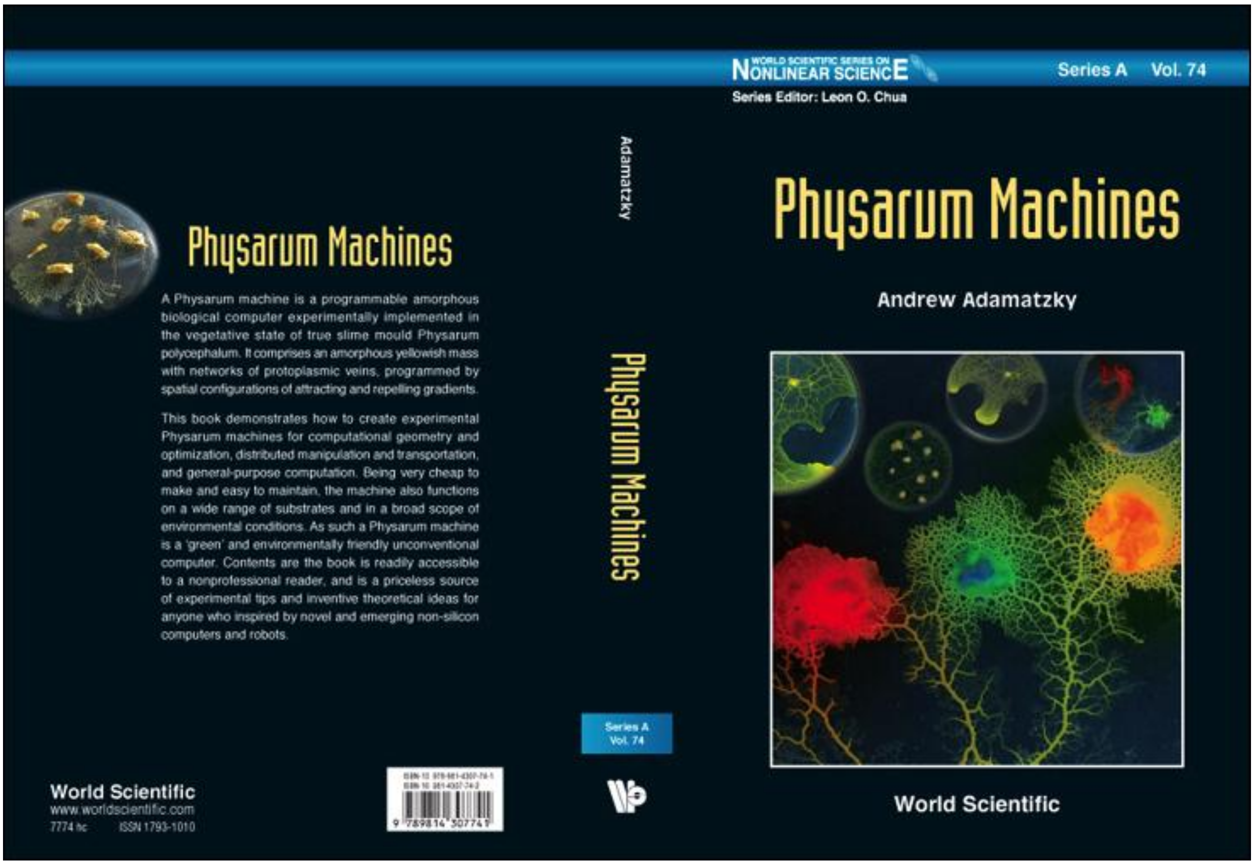
For more details you can see our book Adamatzky A., De Lacy Costello B, Asat T. *Reaction-Diffusion Computers* (Elseviers, 2005).

A.Sch.: One of your latter investigations concerns *Physarum polycephalum* computing. Why are you going to implement the computations on the medium of *Physarum*? Could we say that *Physarum polycephalum* is intelligent?

A.A.: Physarum polycephalum is a very user-friendly creature. It does not require any specialist laboratory equipment. Anyone can do experiments with *Physarum* at the office, kitchen and even bedroom. (I guess we discuss experiments in bedroom in some other interview.) So, yes, when in 2006 Soichiro Tsuda sent me samples of *Physarum*, I thought at first that it is total crap, but then I started to play and realized that it is a wonderful creature indeed, very responsive, adaptable and capable to solve numerous problems of computational geometry, mathematical logic and even navigate robots. Since then I have developed a concept and experimental laboratory implementations of *Physarum* machines.

What is a *Physarum* machine in a word? A *Physarum* machine is a programmable amorphous biological computer experimentally implemented in plasmodium of *Physarum polycephalum*. The point is that a *Physarum* machine on a nutrient-rich substrate behaves as an auto-wave in an excitable medium. On a non-nutrient substrate it propagates similarly to a wave fragment in a sub-excitable medium. A *Physarum* machine can be programmed by configurations of repelling (salt) and attracting (food) gradients, and localized reflectors (illuminated obstacles).

What *Physarum* machines can do first of all? It solves mazes. A *Physarum* machine represents a path from start to finish sites in a maze by its protoplasmic tube. It approximates a planar Voronoi diagram. Data planar points are mapped by pieces of plasmodia. Bisectors of the Voronoi diagram are composed of the substrate's loci not colonized by plasmodium. As a result, a *Physarum* machine computes a nearest-neighborhood graph, a spanning tree, a relative neighborhood graph and a Delaunay triangulation at various stages of its development. Nodes of a graph are represented by sources of nutrients, edges by protoplasmic tubes connecting the sources.



Also, notice that a *Physarum* machine is a universal computer. The machine implements Boolean logic conjunction, disjunction and negation on a geometrically constrained substrate. The machine can also realize binary adders. Hence, we could claim that a *Physarum* machine is a programmable manipulator. The machine can push and pull objects floating on a water surface by expanding and contracting its protoplasmic tubes.

Thus, *Physarum* is a pretty university amorphous biological substrate. Moreover, a *Physarum* machine is a simple biological implementation of a Kolmogorov-Uspensky machine, i.e. a biological substrate for all general purpose computing devices. For details see my other book Adamatzky A. *Physarum machines* (World Scientific, 2010).

As concerns your last part of the question, *Physarum* is intelligent in fact as a drop water running down the window glass. *Physarum* just follows gradients of attractants and repellents. Nothing more.

A.Sch.: What else from physical or biological systems may be presented as process of computation? Everything? Why?

A.A.: Absolutely everything! The matter is that a computation is just our interpretation, our view. Computing potential of any biological, chemical or physical systems is determined only by phantasies of researchers who have built unconventional computers from these systems.

A.Sch.: Could we claim that unconventional computing is a novel paradigm in natural sciences that will absorb physics, chemistry, biology and other sciences in process of time?

A.A.: Unconventional computing is always in the flux. Some concepts become conventional new concepts and prototypes emerge. Unconventional computing is the art of interpretation, and we will always have plenty of phenomena to interpret. For example, a plasmodium, or vegetative state, of *Physarum polycephalum* behaves like a giant amoeba. It is possible to show that topology of the plasmodium's protoplasmic network optimizes the plasmodium's harvesting on distributed sources of nutrients and makes more efficient flow and transport of intra-cellular components. As a result, this dynamics could be interpreted as approximation of shortest path in a maze, computation of proximity graphs, Delaunay triangulation, construction of logical gates, robot control, implementation of storage-modification machines, approximation of Voronoi diagram, and a network of biochemical oscillators and so on. Hence, all depends on our interpretation and imagination. Just our fantasy holds.

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WHO WILL MAKE PHILOSOPHY IN RUSSIA?

Review of the book

A. Nilogov (ed.). Who Makes Philosophy in Russia Today? [Кто сегодня делает философию в России?] Vol. II. Moscow: Agraf, 2011, 528 pp.

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The philosophical project "Who Makes Philosophy in Russia Today?" whose second volume was published by the end of 2010 at the publishing house Agraf (Moscow, Russia), may be compared with Diogenes Laertius famous book *Lives and Opinions of Eminent Philosophers* by its invention. I believe that if the majority of philosophical works were deleted from the memory of mankind by a pandemia of computer viruses or by an attack of the Chinese hackers, these two volumes would be saved for mankind as well as the book of Diogenes Laertius was saved. The two-volume book covers all spectrums of trends of Russian philosophy in the early 21st century.

The project "Who Makes Philosophy in Russia Today?" represents the new, long-awaited, step towards developing *philosophy of* philosophy in Russia: obviously, it is not an historical-philosophical book or chrestomathy of philosophical works. The main feature of the two-volume book "Who Makes Philosophy in Russia Today?" is a prospectiveness of format: the majority of philosophical interviews concerns recent problems and have a "primary" footing in the journalistic meaning of word.

In my opinion, the project is unique not only for Russian philosophy. Whether it is possible to imagine the book in which interviews of all leading American philosophers, including Hilary Whitehall Putnam, Daniel Clement Dennett and Alvin Carl Plantinga, are collected? If we answer this question, it will approach us to the answer of the second question: whether it is possible to make in Russia in philosophy something, what is impossible to make in the USA?

In order to explain a difference between a usual chrestomathy of philosophical texts and the given project, we should compare both formats with methods of empirical science: the first is a passive retrospective survey, and the second is a prospective study. In science there is a consensus that a retrospective survey can never reach contingency and cogency of results received by experimental prospective methods. After all only in the latter case the experimenter is capable to act as the high-grade "demiurge," completely excluding any methodological bias. Reading the two-volume book, we als ob get to a situation of the intellectual experiment carried out by the author.

In this case the role of shock electrodes have been played by (brilliant) Nilogovian excellent tricky questions from which it is impossible to hide in high ranks and former merits. Using irony, Nilogov's interviews would be compared with the interrogations in KGB torture chambers.

Actually, Niligovian inquirer is focused on two questions: "What is philosophy in general?" and "What is Russian philosophy in particular?" (As it turned out) Different authors answer this question absolutely differently. Philosophy, as Vladimir Krasikov, one of the authors of the second volume claims, is *"the most extremist kind of spiritual creativity*." Among all sciences philosophy plays the role of absolute sovereign. After all someone should give an initial intellectual marking and conduct borders between (the) sciences. However it is impossible to consider this question definitively solved, because it is waiting for its Carl Schmitt. The opposite version of answering could be reduced to Vasily Vanchugov's remark: philosophy should become a part of "*Bologna Process of educational service*."

Anyway, philosophy always acts as the "tool of finishing" of human thinking. Depending on the degree of radicalism of the project it can be both small repair, and total transformation. Nevertheless, the reason movement forward is impossible without exemption from surpluses of memoirs: the philosopher as to the cosmetic surgeon always must "cut on live," deleting excesses of intellectual fabrics (and sometimes cleansing their pollutions). It is certainly not a way of "intellectual terrorism", (but rather a "humanitarian intervention"). Implementing a tolerance in society is another, not less important purpose of Russian philosophy. Therefore it was pleasant to see that in the Nilogov project there has been a place for both Orthodox, and atheists, and even for the so-called neo-pagans. People of absolutely different points of view are capable to get on peacefully on collection pages, communicating among themselves in a universal language of philosophy. To sum up, the philosophical language became original glue, fastening the new identity of Russia. (Then,) The identity catastrophe there would be a transition of the majority of philosophers of Russia towards working English, but it will never occur for the trivial (,but salutary,) reason: the knowledge of English in Russia is traditionally lowest.