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DEPARTMENT OF PHILOSOPHY

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FOREWORD

The tradition of philosophical research at Tartu University is not a new one. Since its foundation in 1632 under the name of *Academia Gustaviana*, the University has contained a Faculty of Philosophy which has been responsible for philosophical studies. Fate has willed that throughout the years philosophy in this Estonian university has been taught and studied mostly in languages other than Estonian, particularly in Latin, German, and Russian. Estonian became a language of study and research at Tartu University only in the independent Republic of Estonia (1919–1940). This development was interrupted for a longer time by the incorporation of Estonia into the Soviet Union. Although during the Soviet period philosophy was continually taught mostly in Estonian, the main language of philosophical publications was Russian. Thus from 1958–1991 *Papers on Philosophy*, published as a series of a *Tartu University Transactions*, were almost entirely in Russian. These 36 issues include studies of very different content and quality. Hundreds of articles reflected very precisely the possibilities and impossibilities of philosophical research at that time. The restoration of the Republic of Estonia has brought about great changes at the University and in the Department of Philosophy, which now has three professorships and trains its students to become our own professional philosophers in Estonia. Another reflection of recent changes is that since 1993 the Department of Philosophy has published its new series *Studia Philosophica* primarily in Estonian. However, it did not take long to learn that Estonian philosophers should not hide themselves from Europe and the rest of the world behind the barrier of the Estonian language. The present issue of *Studia Philosophica*, published in English, attempts to open the door for the foreign reader into the realm of philosophical research in this country. As the reader can see, it does not differ in principle from the philosophical studies of the English-speaking world. I would like to stress that the articles published here were not selected for special display. They are average everyday studies by Estonian philosophers and teachers of phi-

losophy, dealing with different areas of philosophy from philosophy of science to aesthetics, written at different centres of philosophical research from Tartu to Tallinn, representing different generations of philosophers from doctoral students to full professors, and reflecting both the history of philosophical thought and modern philosophy itself.

In the hope of finding tolerant readers,

Ülo Matjus,
Professor of History of Philosophy,
Head of the Department of Philosophy,
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SOME COMMENTS ON A NATURALISTIC APPROACH TO THE PHILOSOPHY OF SCIENCE

Rein Vihalemm

In this paper I am going to continue my considerations about the goals and status of the study of science I presented in Uppsala at the 9th Congress of Philosophy of Science (Vihalemm 1991: 89), taking into account the discussion at the Meeting on Social Epistemology and Social Theory of Knowledge, affiliated with the congress,¹ and at the Joint Conference of the Society for Social Studies of Science (4S) and the European Association for the Study of Science and Technology (EASST) in Gothenburg 1992.² My aim here is to call attention to some principal methodological difficulties arising when we want the study of science to be a science itself. The present paper is to be, substantially, a discussion of Ronald Giere's conception of naturalized philosophy of science (Giere 1985, 1988, 1989).

R. Giere claims that the study of science should be a science itself, i.e. a theory of science. He uses the term "theory" in the sense of a scientific theory, which in turn means simply a "nonphilosophical theory". Therefore, a naturalized philosophy of science

is not a part of philosophy at all. If one regards the philosophy of science as a part of epistemology, for example, the dominant enterprise has been to show how claims of scientific knowledge can be justified in some suitably noncircular manner. The philosophical goal, in short, has been to provide some extrascientific foundation for scientific claims. (Giere 1988: VII.)

R. Giere's view is that science is a cognitive activity and therefore, as any cognitive activity, should be studied by the cognitive sciences.

The social approach to the study of science is a kind of naturalized philosophy of science. I agree with the proponents of the social and/or

¹ Affiliated Meeting: Social Epistemology and Social Theory of Knowledge. Abstracts of the 9th International Congress of Logic, Methodology and Philosophy of Science. Uppsala, August 7–14, 1991. Vol. III, pp. 203–215.

² 4S/EASST — Joint Conference: Science, Technology and "Development". — Gothenburg, August 12–15, 1992. Abstracts for the joint 4S/EASST Conference 12th–15th August 1992. Göteborg.

naturalistic approach to the study of science that a general rational methodology of science is a philosophers' myth. For instance, when analyzing books by Steve Fuller (1988, 1989) at the above-mentioned Meeting on Social Epistemology and Social Theory of Knowledge, R. Giere said that S. Fuller's

approach to the study of science is **naturalistic**. Even though he has strong sympathies with humanistic (**geisteswissenschaftlich**) approaches, in the end he sides with a broadly natural science (**naturwissenschaftlich**) approach, one broad enough to include sociology as a natural science. He argues against all attempts to explain science as following principles of rationality that themselves require *a priori* or transcendental justification. (Giere 1988: 2.)

R. Giere continued:

I agree with Fuller that the operation of general methodology in science is a philosophers' myth. And I agree with one of his main arguments for this view, namely that the myth simply is not supported by the actual history of science. But Fuller goes too far in adopting the position of social constructivist sociologists of science who, at a minimum, insist that we, as students of the scientific enterprise, remain agnostic about the actual success of natural science in representing the world. (Giere 1988: 3.)

Instead of the position of social constructivists R. Giere defends a position called by him 'constructive realism'. Similar views have also been developed elsewhere (Chalmers 1982, Niiniluoto 1991: 143). In my opinion constructive realism is, in principle, equivalent to the position of those philosophers of science, who proceed from the Marxist notion of praxis as the basis of cognition. I agree in principle with R. Giere. However, there remain some unanswered questions concerning the status of science in the strict sense of the word. Thus, a naturalistic and/or social approach to the study of science does not free us from philosophical and epistemological questions concerning realism, truth, the status of science as a cognitive enterprise and so forth. Philosophical methodology of science, indeed, cannot help scientists. Naturally, scientists are influenced by the philosophical methodology of science too (Niiniluoto 1991: 137), but methodology is a paradigm-laden (I mean the paradigm-conception of Thomas Kuhn). A paradigm is not free from philosophical methodology, but scientists cannot learn their job from philosophers of science or from textbooks of scientific methodology. A methodology outside a paradigm is not a working methodology. What is primary, is a paradigm.

But this is not to say that philosophy of science as a critical analysis of philosophical and methodological discussions about the criteria of scientificity is meaningless. Not only for scientists, and maybe first of all not for them, but for philosophers, and for the cultural study, it is important to understand why science is held in high regard. Why physics as an

exact science has gained the status of a standard of an ideal science, scientificity in general? Is it right to consider science the highest form of cognition? Why is there an ideology of science in contemporary society?

Philosophy and methodology of science often rely on physics and its history without analyzing explicitly whether it is justified that methodology of physics represents methodology of science in general and whether methodology of science represents the universal rational and highest form of cognition and knowledge.

The presentation of the criteria of scientificity and the estimation of their universality and rationality depend on the aim of the analysis. Classical epistemologists proceed from the analysis of philosophical problems, traditionally considering science to be the highest form of cognition and rationality without any special argumentation, therefore finding it necessary to reconstruct rationally the development of science. They do not take seriously the study of science how it is. They do not observe actual scientists in historical contexts. Naturalists and/or social epistemologists are right that there are no *a priori* reasons for supposing that philosophical epistemologists' theories of rationality and cognition are necessarily presupposed or can be used at all for an explanation or rational reconstruction of scientists' practice, the real history of science. R. Giere's cognitive theory of science is an example of a nonphilosophical theory of science. He admits that an early attempt to develop such kind of theory of science was made by T. Kuhn (Giere 1988: 32).

Since the writings of T. Kuhn, at least, it has been no more possible to deal with the philosophy or methodology of science without taking into consideration the history of science and *vice versa*. The term 'history' in 'history of science' is not employed here in its usual narrow sense only, i.e. as knowledge about the past. The notion 'history of science' includes the entire development of the socio-historical or cultural phenomenon called 'science' — its past, present, and even some considerations about its possible future. But it should be emphasized, following, e.g., P. Feyerabend (1978 a, 1978 b) or some Russian philosophers like M. A. Rozov (1977) and N. I. Kuznetsova (1982), that one has to take essentially different positions when dealing with the history of science or with the methodology of science (sometimes the term 'philosophy of science' is also used nearly in the same sense, i.e. in the sense of 'methodology of science'). As T. Kuhn, for instance, has claimed "no one can practice them both at the same time" (Kuhn 1977: 5). The position of a methodologist of science is in principle the same as the position of a scientist himself, because it is the position of a participant in scientific research, who wants to know how to obtain scientific knowledge and to rationalize or improve scientific methods. The position of the historian of science, however, is that of an observer, who

wants to know and understand what science is, how science actually proceeds, has proceeded in the past and what can happen in the future.

It seems that such a broad meaning of the notion 'the history of science' should also be taken into consideration in the often-quoted Kant-paraphrase, stated by I. Lakatos as follows: "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." (Lakatos 1971: 91.)

I. Lakatos and perhaps the majority of the historically oriented philosophers of science have presumed that the aim of methodology of science is to be a theory of scientific rationality or rational reconstruction of the history of science (Lakatos 1971, Curtis 1986). At the same time they identify methodology of science with the theoretical part of history of science — theory of science — without which the history of science cannot be written (Lakatos 1971: 107). Certainly, purely empirical or descriptive history of science is impossible. But as T. Kuhn argued, the history of science has an autonomy. Its theoretical part — theory of science — is not identical with the philosophy or methodology of science, "a *a priori* philosophical position is not the only selective principle and ... it is not, as a selective principle, inviolate." (Kuhn 1971: 143.)

At least starting from the works by T. Kuhn and I. Lakatos we know the dilemma of 'aprioristic rationality' and 'historiographical positivism', i.e., if the principles of rational reconstruction of the history of science are given *a priori* by the normative methodological conception, then the real history of science cannot have any effect on the principles of rationality. If, however, it is claimed that the very understanding of rationality should be derived from the real history of science, then it is not clear how it will be possible to avoid the so-called 'historiographical positivism', i.e. a simple description or a, so to say, theoretical' justification of everything that takes place in the real history of science.³

R. Giere's conception of a 'naturalized philosophy of science' has been proposed just following Kuhn's approach and trying to avoid aprioristic rationalism. R. Giere claims that

³ Kuhn, T. 1971. Notes on Lakatos. (Op. cit.); Kuhn, T. 1980. The Halt and the Blind: Philosophy and History of Science. *The British Journal for the Philosophy of Science*. Vol. 31, pp. 181-192; Lakatos, I. 1971. Op. cit., p. 120., 132-133. See also: Vihalemm, R. 1982. The Dilemma of 'Aprioristic Rationality' and 'Historiographic Positivism' in the Western Philosophy of Science. *Voprosy Filosofii*, no. 2, pp. 55-65. (In Russian, Summary in English); Vihalemm, R. 1983. On the Philosophical Foundation of Different Programs Concerning the Rational Reconstruction of the History of Science. *Abstracts of the 7th International Congress of Logic, Methodology and Philosophy of Science*. Salzburg. Vol. 6, pp. 201-202.

the principles of rationality are only instrumental, or conditional. They connect research strategies with the goals of research. And establishing these connections itself requires scientific inquiry. (Giere 1989: 377.)

However, for surmounting the dilemma, the scientific study of science should not be only descriptive (empirical), but theoretical as well.

A question arises, what kind of knowledge is the relatively autonomous theoretical part of the history of science — theory of science, and is it possible at all? I think it is the essence of a more general question: what is science? What do we mean by the term 'science' if we want the study of science to be a science itself?

R. Giere (1988: 1) writes in his monograph "Explaining Science: A Cognitive Approach" simply:

Science is a cognitive activity ... concerned with the generation of *knowledge*. Indeed, science is now the major paradigm of a knowledge-producing enterprise.

He speaks of a theory of science, which

would thus serve to explain the phenomenon of science itself in roughly the way that scientific theories explain other natural phenomena. My view, therefore, is that the study of science as a cultural enterprise is itself a *science*. To be sure, it is a *human science*, and that raises question about the extent to which any human science can be like such sciences as physics or biology. I shall not be diverted by such general questions here. (Giere 1988: 1.)

In 'Notes' R. Giere adds: "This is not to say that I fail to appreciate the seriousness of the issue." (Giere 1988: 281.) In the monograph under discussion the task was only to propose the term 'scientific theory' in the sense of a 'nonphilosophical theory' of science, or naturalized philosophy of science ("The label is not significant", the author mentions) (Giere 1988: XVII).

However, I would like to deal just with this question whether a theory of science as a human science can be like such sciences as physics or biology, more principally, namely as physics. It is because physics as an exact science has gained the status of a standard of an ideal science, scientificity in general. Not only human scientists, but biologists and methodologists of biology, and even chemists and methodologists of chemistry (I myself (Vihalemm 1987) presented a paper on the subject to the 8th Congress of Philosophy of Science) are also discussing whether their fields of knowledge are similar to physics or whether it is in principle possible to become methodologically similar to physics, i.e. to obtain the status of a perfect science.

R. Giere does not accept the methodologists' view about the ideal of a scientific theory and the ideal of a science in general. He asks rhetorically, "Why does the picture of science developed by philosophers and sociologists differ so greatly from that found among the educated pub-

lic?" (Giere 1988: 11.) Among the educated public not only physics and its theories, but biology or geology and biological or geological theories, too, are regarded as sciences and scientific theories. R. Giere begins, nevertheless, with physics, with a physical theory, namely with classical mechanics, but instead of taking it in the classical methodological or epistemological context, he takes it in his own context of naturalized philosophy of science based on cognitive sciences and the evolutionary theory. He writes:

I shall begin with scientific representations themselves, keeping in mind that scientists, after all, are only human. The representations scientists construct cannot be too radically different in nature from those employed by humans in general. (Giere 1988: 62.)

If we wish to learn what a theory is from the standpoint of scientists who use that theory, one way to proceed is by examining the textbooks from which they learned most of what they know about that theory. (Giere 1988: 63.)

He emphasizes that although there is no doubt that historians of sciences, such as Kuhn, are right that textbooks distort the history of the subject,

it is of no consequence here. The task here is not to reconstruct the historical development of any science but simply to describe, in general terms, the character of a theory as it is understood by contemporary scientists. (Giere 1988: 63.)

It seems to me, however, that R. Giere ignores the fact that physics as an exact science has gained the status of an ideal science, and scientificity in general not only for the methodologists of science, but for the educated public as well. The task of a philosopher of science is to explain that fact. Naturally, then we should analyse the specificity of physics as a branch of cognitive activity instead of emphasizing the similarities between physics and biology or geology or human cognitive activity in general by examining the character of a physical theory from textbooks, not from the history of the subject.

It is essentially important to realize the premises and limits for knowledge that has a status of a perfect exact science like physics. Scientific cognition is paradoxical, i.e. theoretical knowledge presupposes empirical knowledge, but the latter, in turn, presupposes the former. This paradox⁴ does not cause essential difficulties only if we deal, as in

⁴ In order to prevent misunderstandings I must specify the paradox. I am very grateful to Prof. Hugh Mellor for kindly reading and commenting an earlier version of this paper when visiting Tartu University in April 1992. One of his comments was about this paradox. In his opinion it means that we assume some theory to make an observation, but then the observation may contradict the theory. This is not paradoxical: it just shows that observation can be 'theory-laden'

physics, with an experimental-theoretical research which, operating with experimentally substantiated idealizations, itself constructs its subject as the object of research in physics (physical reality, physical phenomena). I would like to emphasize the fact that the subject of modern physics (i.e. physics since Galileo) is not determined by any definite objects of nature or any fundamental level of nature itself. In this sense indeed social constructivists are right that the natural world has a small or nonexistent role in the construction of scientific knowledge (Collins 1981). Nature is the subject matter of physics only on the basis of those of its characteristics, aspects and phenomena which can be expressed mathematically, be measured, exposed and reproduced experimentally. In this sense physics itself constructs its object of investigation, considering nature only through idealized and mathematically projected situations. Therefore physics represents an experimental exact science in general, in its purest form, making it possible to study the methodological structure and functions of the exact science theoretically.

I think that the premises and limits of science as actually or in principle exact science, have been distinctly recognized by I. Kant already. I mean his famous 'Copernican revolution'. Exact science is possible on condition that the object of investigation is definable by cognition itself, by the very principles of exact science (as in physics). If, however, we have the opposite situation: the task of cognition demands that we must get knowledge about the object that is already anyway 'given' before and independently of its investigation, then the purely scientific knowledge, the knowledge following the pattern of exact sciences about that object is not possible. In the field of empirical knowledge there are two main types of theoretical cognition: (i) scientific (more precisely — exact-scientific) cognition, being of a constructive-hypothetico-deductive character and (ii) non-exact-scientific cognition, being of a classifying-historico-descriptive character (say from biology to the humanities).

The study of science, if its goal is, indeed, to obtain knowledge about real science, not to construct a presumably ideal rational science, then the study of science is, without a doubt, also such a field of cognition that belongs to the second type of cognition. The study of science cannot be developed by a purely scientific method, i.e. after the pattern of exact

and still capable of refuting a theory, i.e. it does not prevent theories from being empirically testable.

But my point is about the origin of a scientific theory (including the origin of that theoretical knowledge we assume to make observations in science). So, I would like to pay attention to the very possibility of constructing a non-speculative theory, i.e. a theory having empirical content, but being nevertheless more than that empirical content or merely an inductive generalization of the observation or suchlike.

sciences. Therefore, the scientific theoretical history of science, as an analogue of theoretical physics is not possible. It is possible, naturally, to develop a descriptive theory and theoretical models methodologically analogous to those in biology, for instance. But biological models, being nonspecific for social and historical phenomena, cannot help essentially. In this sense philosophy of science should not be as much 'naturalized' as 'socialized', meaning that we must also grasp the peculiarities of science as a socio-historical and cultural phenomenon. Socio-historical phenomena, certainly, include natural characteristics as well, and are themselves in a broader sense natural, too, i.e. they are not, for instance, mystical or divine, or anyway purely human and *a priori* rational.

The goal of exact-scientific cognition is not purely epistemological — to understand the natural world as it is, to obtain the truth about it, but the goal is socially determined — to understand the natural world in a way that makes modern technology possible (the latter has been mentioned by Giere (1988: 132), too). As long as technology is central in society, exact science assumes the position of an ideal science, of scientificity in general, and science in turn will be continuously considered the highest form of cognition and rationality. But science as actually or in principle an exact science — the ideal of which being physics is epistemologically justified — should not be epistemologically considered the universal and highest form of cognition, but only one of its rather specific forms. Science is a specific socio-historical and cultural phenomenon. Its analysis needs a philosophical approach at least in the sense of conceptual, historical and normative criticism in order to avoid one-sided and/or ideological interpretations. (By the ideological interpretation I mean the use of some one-sided concept of science in defence of one's socio-political position.)

It seems that R. Giere's naturalized philosophy of science is, due to its naturalism, in one sense too broad, in another sense too narrow. It is too broad because it does not take into consideration the term 'science' in its strict sense as (in principle, at least) an exact science. And therefore R. Giere's theory of science is a remarkable theory of knowledge in general rather than a theory of science in particular. And it is too narrow or abstract because the applied cognitive approach seems to be excessively biological, its theoretical models are mostly constructed on the basis of a biological analogy (although I admit that Giere's theory is open to nonbiological models as well).

Concluding my short analysis, I hope, it is not futile to draw attention to the different goals, respective means and structures of the two types of cognition, called above constructive-hypothetico-deductive or scientific in the strict sense, and classifying-historico-descriptive, i.e. in the strict sense nonscientific, methodologically open, non-subordinate to any aprioristic normative methodology. In addition, it should be noted that

the co-operation between these two types of cognition is possible and recommendable (in contemporary biology, for instance, or in I. Prigogine's theory of self-organization, it has been very successful), but namely co-operation, not reduction of one to another. Co-operation, I think, may be successful for the study of science, too. Namely the co-operation between the study of science from the scientist's or the methodologist's position and from the historian's position. This is the present-day situation in our field of research, in my opinion. For the future, co-operation is the keyword.

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IS SCIENTIFIC REALISM TOO OPTIMISTIC?

Endla Lõhkivi

In this article* I will consider a form of scientific realism which is called critical scientific realism, and has been developed by the Finnish philosopher Ilkka Niiniluoto for the last 10–15 years.

Nicholas Rescher (1987) has given a definition of realism as a philosophical doctrine which coincides with the most widespread use of this term: "Philosophical realism as a general doctrine maintains the thesis that there is a domain of mind-independent existence and that we can obtain some reliable knowledge of it."

Both in history and in contemporary philosophy, realism concerns itself with the real existence of entities that lie beyond the reach of human perception. In medieval times discussion focused upon universals. Contemporary realists are less concerned with unobservable abstract than with the unobservable "theoretical entities" of modern science, like electrons, genes or magnetic fields.

If general metaphysical realism¹ asserts that we can know something, *scientific realism* claims that we can know a lot about the world, and our knowledge is essentially of the state of affairs in the world. For scientific realism the best way of getting knowledge is science (it has a clear priority over types of knowledge). The sciences describe the real world where abstract entities like electrons exist the same way as grains of sand or black cats. The essential aim of the sciences is truth. Science seeks truth, approaching the goal step by step. Thus, there is progress in the sciences which can be estimated using the concept of verisimilitude.

This is really quite an optimistic doctrine. It seems especially promising, if we compare realistic philosophy with scepticism, relativism or other antirealist ideas. Antirealist philosophies like pragmatism, positivism, relativism, anarchism, etc. have attained large popularity, therefore I assume the reader has some basic knowledge of their assertions. An an-

* I am indebted to Professor Ilka Niiniluoto who kindly supplied me with his main works. I am also grateful to Professor Aant Elzinga of the University of Göteborg for the advice and support that I experienced when writing the first version of the paper in 1991.

¹ "Metaphysical realism" is a term allowing different interpretations, e.g. in an extreme case metaphysical realism might coincide with essentialism. About different uses of metaphysical realism, see Bhaskar 1978, Lepin 1984, Porus 1984.

tirealist philosopher cannot agree with the idea of convergence in the sciences and therefore with the idea of progress in knowledge either, because there have been breaks in the historical development of the sciences like scientific revolutions. According to Thomas Kuhn (Kuhn 1970) theories are incommensurate, it is thus impossible to speak about progress towards the truth. For pragmatist and anarchist thinkers the concept of truth does not make sense. What makes sense from their point of view, are the problem-solving ability of theories, their inner coherence, logical consistency and other epistemic, also social and political utilities.

Having taken all this and some dissonance inside the realist school into consideration, I feel forced to ask: IS SCIENTIFIC REALISM TOO OPTIMISTIC? Is it too optimistic to expect scientific theories to correspond to something real? Is it too optimistic to presume theories to have an ability to explain and predict the events of the world?²

In the following review I will try to find out the position of critical scientific realism among its alternatives, and to justify the distinction.

Realism in ontology

As it is written in the definition of realism by Rescher (1987), there are two different parts, the first declaring the existence of a domain of mind-independent reality which is ontological, and the other, stating the possibility of knowledge of the domain, which is the epistemological thesis in the definition.

According to Niiniluoto (1987 a) the central assumption of realism in ontology is the existence of a mind-independent reality (either in the medieval sense of universals or in the materialistic sense of the existence of the real world, or in the sense of independent existence of both, of mind and reality (dualism), or even in the sense of objective idealism, i.e. both matter and mind are taken to be generated from an ultimate spiritual reality. Niiniluoto insists that the independence of reality must not be understood as causal, since there is a causal interaction between mind and physical reality. Ontological realism does not deny that physical objects may have nonphysical, mind-involving properties which ontologically presuppose relations to consciousness or consciously created cultural and social institutions, e.g. beauty, economic value, etc. On the other hand, we cannot regard all properties as mind-involving (as inter-

² In principle, there are three fields of the use of the term "realism": firstly, in the sense of objective existence of abstract objects indicated by our words denoting them; secondly, in the sense of existence of material objects and our knowledge of the external world; and thirdly, concerning the field of debates over the cognitive status of theoretical terms in theories. See, e.g. Stockman 1983.

nal realism does³), otherwise we cannot explain what existed in the world before the evolution of man and the first conceptual system. Ontological realism does not presuppose essentialism, according to which the world has a ready-made structure and its own privileged language, neither is ontological realism a kind of actualism: real means actual, but it also means potential. Thus, ontological realism is not compatible with subjective idealism (solipsism, phenomenalism).

Realism in epistemology

Realism in epistemology claims that we are able to obtain some knowledge about mind-independent reality. This general claim may be developed in different directions, thus giving rise to a number of different positions and interpretations.

According to naive realism (and classical empiricism and rationalism of the seventeenth century) certain, reliable and cumulative knowledge about reality is obtainable by sense experience or by reason. This position was criticized by Descartes, in his formulation of the problem of knowledge he urged that the thinking subject can have "clear and distinct" knowledge only of its own states, thus knowledge about the external world becomes problematic. Kant made a distinction between the "noumenal" realm of things in themselves and the "phenomenal" world of things for us: things in themselves cause our sensation, although, as Niiniluoto has mentioned, this is a contradiction in Kant's system: the category of causality is applicable only to the world of phenomena, so we are not able to know anything about "things in themselves" apart from their existence only.

Phenomenalism eliminates the epistemological subject, leaving only the phenomenal world including the phenomenal ego as a bundle of sensations. Phenomenology (Husserl) puts the objective world into brackets and concentrates on the analysis of ways in which theoretical and practical interests constitute our "life-world". For pragmatists the world in itself is meaningless, only a reality related to the conceptual and cognitive practices of the scientific community make sense. This is why pragmatists do not accept the objective concept of truth (correspondence to the world in itself). We could mention here some other antirealist theories of knowledge such as logical positivism, which considered both ontological realism and phenomenalism senseless as unconfirmable metaphysical statements. Some new influential versions of antirealism appeared in the 1970-s in the form of Dummett's antirealism, Rescher's conceptual idealism, Putnam's internal realism and Goodman's theory of world-making (Niiniluoto 1987 a).

³ About internal realism see Margolis 1987, Niiniluoto 1993, Putnam 1981.

Critical realism

Critical realism as another line of epistemological thought can be represented in Kantian terms: Kant's phenomena could be interpreted as expressions of our partial knowledge of things as they are in themselves in the mind-independent reality.

Niiniluoto offers the following theses to distinguish critical scientific realism from its alternatives:

- R0 At least part of reality is ontologically independent of human minds.
- R1 Truth is semantic relation between language and reality. Its meaning is given by a modern (Tarskian) version of the correspondence theory, and its best indicator is given by systematic inquiry using the methods of science.
- R2 The concepts of truth and falsity are in principle applicable to all linguistic products of scientific inquiry, including observational reports, laws and theories. In particular, claims about the existence of theoretical entities have truth value.
- R3 Truth (together with some other epistemic utilities) is an essential aim of science
- R4 Truth is not easily accessible or recognizable, and even our best theories can fail to be true. Nevertheless, it is possible to approach truth, and to make rational assessments of such cognitive process.
- R5 The best explanation for the practical success of science is the assumption that scientific theories are, in fact, approximately true or sufficiently close to the truth in relevant respects. Hence, it is rational to believe that in the long run the use of the methods of science has been, and will be, progressive in the cognitive sense (Niiniluoto 1987 a).

Realism, then, is a theory that refuses to disjoin the so-called ontological and epistemological questions. In other words, the denial of realism entails disjoining the two to at least such an extent that either the world is rendered cognitively inaccessible or inquiry is cognitively stalemated or defeated (Margolis 1986). As a common-sense ontology, realism takes seriously the existence of the things, structures and mechanisms revealed by the sciences at different levels of reality. It is not necessary that the sciences should always be right in their assertions, substances like phlogiston may have to be purged from our ontology. As William Outhwaite says: "The realist assumption is merely that the existence of such entities is not to be construed as a heuristic, but as a truth-functional assertion like any other, even though we may not yet, and never finally know, whether it is true or false." (Outhwaite 1987.) Accordingly, a non-empiricist epistemology belongs to this non-atomistic ontology.

Truth as correspondence

Most theories of truth have been (1) correspondence theories, (2) coherence theories, and (3) versions of pragmatist theories (Niiniluoto 1987 b). According to the correspondence theory, truth is a relation between belief and reality, or between judgement and the world. Scientific realists' following the correspondence theory separates them from pragmatists who replace the realist concept of truth with some surrogate such as verified knowledge, proved knowledge or ideal consensus, and from epistemological anarchists who deny the existence of any reasonable concept of truth.

A fundamental difficulty for the correspondence theory is to specify what it means to say that the statement corresponds to the reality. Popper, in his analysis of the question what the truth is, replaces the question with another: is there a correspondence between the theory and facts? If there is such a correspondence, this must be also proper and applicable for the truth concept of pragmatists and adherents of coherence theory as well. According to Tarski, if we want to speak about a correspondence between the theory and facts, we should use a language which enables us to describe both of them, i.e. the language should possess the means needed to refer to statements and to describe facts, i.e. metalanguage. A statement *S* of the objectlanguage corresponds to the facts if and only if *f* takes place, where *S* is the name of the statement and *f* is the abbreviation of the expression. A truth sentence in language *L* depends on the structure of the world in relation to this language. The structure of language *L* is representative to some fragment or aspect of actual THE WORLD. Thus, epistemic mapping is provided (Popper 1972).

Knowing the meaning of a statement, we know the conditions of its truth, we know what the world is like, if the statement is true. Niiniluoto emphasizes that Tarski's definition of truth distinguishes truth as a concept from truth as a procedure. So, truth conditions do not entail truth-value (as any definition of function does not give their actual values in any variable argument).

What we have to take into account is that every language creates its own world. In other words, the world is not "ready-made". Our ontology is a reflection of our choice of language. As Margolis has said: "To theorize about the structure of the actual world is to theorize about our competence to grasp that structure and to claim any cognitive competence is to claim that its exercise entails some grasp of the actual world's structure." (Margolis 1986.)

Bunge (1983) maintains an approximately similar position: a factual theory, at least in the first approximation, describes some aspect of reality, or refers to some piece of reality.

Törnebohm (1976) talks about mapping territories in reality. A factual theory must be considered both referentially and evidentially. The first is a relation from theory to the world, and the other from the world back to the theory.

"Referentially (semantically) considered factual theory points, in an immediate way, to a conceptual image or theoretical model which in turn supposed to refer to real system. Evidentially (methodologically) considered, the same theory points in an indirect way to a set of observed facts (the available evidence) and in a mediate way to a more comprehensive potential class of observable facts." (Bunge 1967.)

Mediate or real referents of a theory need not be observable directly and very often they are not. The evidence for a theory is different from the descriptions of referents because mediate referents are supposed to exist independently of theory. Evidence is possible only in connection to theory: observation data might be or not be relevant to the theory. Theory determines how to interpret data. Thus, theory has double assumptions which determine both the correspondence between the symbols of theory and referents, and evidential correspondence, testability or truth.

Correspondence should not be rendered here as a correspondence rules model, but as an interpretation of semantic assumptions which are corrigible hypotheses.⁴

Ronald Giere, who also associates himself with the criticism of "correspondence rules" and "meaning postulates" and of logical empiricism as a doctrine in general, takes the position that there is a relation of truth between a theoretical model and an exemplar. This relation is not epistemological because the model is defined as relevant to formulas. This relevance means a semantic relation. But the theoretical model is something more than an exemplar, the model represents something. Thus, a hypothesis is seen as a linguistic entity purporting a relationship between the model and the real system represented (Giere 1988). Bas C. van Fraassen holds that the relation is one of isomorphism (van Fraassen 1980). In Giere's opinion the relation between the model and reality is one of similarity (which must be determined in what respect and to what degree). But there cannot be any correspondence between a set of statements (linguistic entity) and the world, the relation is through the intermediacy of the theoretical model. If there were a semantic relation between the world and statements, then we would not need any models, Giere asserts. The present author, however, holds that there is no contradiction between semantic realism and Giere's epistemological (cognitive) theory. They speak just about different aspects of truth. As

⁴ Realism entails holism in epistemology, speaking about models, hypotheses, etc. instead of atomic sentences that are characteristic of positivism.

we could see, Tarski's version of correspondence does not cover the epistemological aspect of truth, the way how truth could be attained. This is a question of procedure. So, semantic and epistemological truth do not exclude one another, on the contrary, they presuppose each other.

Niiniluoto's variant of approximate truth theory gives us an example of coexistence of semantic and epistemological aspects of truth. His solution to the problem of similarity (and truthlikeness) between real system, model and theory runs as written in his comment on Giere: "... a theory can be defined to be 'approximately true' if it is true in a model which is similar to the real system (i.e. to the fragment of the actual world that we are interested in our inquiry). A theory is 'truthlike' if it is similar to the most informative true statement (of our relevant conceptual system). If the theory contains counterfactual idealizational assumptions, then it has to be compared to factual statements through 'concretization' where idealizations are removed." (Niiniluoto 1991.)

Nevertheless, there are some realist thinkers who do not share the position of semantic realism, e.g. Alan Chalmers. Chalmers calls his variant unrepresentative realism. He agrees with the assumption that the world is the way it is independently of our knowledge of it, and he also agrees that theories are applicable to the world inside and outside experimental situations, theories are more than correlation between observation data. At the same time theories do not describe entities in the world, "because we do not have access to the world independently of our theories in a way that would enable us to access the adequacy of those descriptions." (Chalmers 1988.)

This seems to be close to the instrumentalist viewpoint because instrumentalists deny the truthvalue of theoretical statements. Theories serve as instruments to give us new data. According to instrumentalism, their own character is unknown for us. There is also a viewpoint, which Niiniluoto has called entity realism, claiming that concepts do refer to something real indeed, but theories and laws do not, and a position known as internal realism, which takes the world to be a construction of the scientific community and deals mainly with linguistic and epistemic questions, such as the creation of concepts and theories between critical realism and instrumentalism.

If Niiniluoto's thesis *R1* made a clear distinction between realism and pragmatism, then thesis *R2* separates realism from descriptivism and instrumentalism. The question is about the applicability of truth (falsity) to linguistic products of science such as theories, laws and even observational data.

For descriptivists theories are economical descriptions of the observationally given world. Both descriptivists and instrumentalists regard the main task of science as instrumental, to consider observational data.

Theories are invented to predict new data and to test them. Science is like a "black box" into which we put some information and get a prediction. The "black box" does not presume the existence of theories or other theoretical entities in any certain way. For this reason the question about truth does not arise at all.

Both descriptivist and instrumentalist philosophies belong to radical empirism. According to Rescher contemporary empiricism has at least four variants: (1) theories are considered to be classifications of data; (2) theories are expressible in actual or possible data, but only data; (3) theories are independent of data, thus, one cannot believe them; (4) believe your theories, but keep a watch on their adequacy to data! (Rescher 1987.)

The fourth is known as constructive empiricism of Bas C. van Fraassen, according to whom "Theories are formulated in order to be empirically adequate with respect to observational data or phenomena." (van Fraassen 1980.) Adequacy does not presume truth value. In this regard a theory is adequate, if it is possible to accommodate the statements to the data. Theories can be empirically adequate even if their central terms do not correspond to anything real. Only philosophers are interested in general and necessary laws, whereas scientists speak about translatability, invariance, empirical adequacy. We may believe theories to be true, but the only way to justify the belief is using probability.

Truth as an aim of science

For realists truth is an essential aim of science, science is a truth-seeking activity. Historically, development or progress is supposed to occur in the sciences. "Progress is a normative or goal-relative concept which should be distinguished from such neutral descriptive terms as "change", "development", ... To say that the step from stage *A* to stage *B* constitutes progress means that *B* is an improvement of *A* in some respect, i.e. *B* is better than *A* relative to some standards of criteria." (Niiniluoto 1986 a.) In the same sense of scientism all the science is progressive because science is taken to offer the best solutions to the problems, etc. (Bunge 1983.)

A goal, relative to which progress is defined may be accessible in the sense that it can be reached in a finite number of steps in a finite time. In traditional philosophy of science one has the theory of convergence to which science should be analogous to the work of solving an equation by numerical analysis, where the concept of truth is conceived as the ideal limit of inquire in the Peircean sense (Niiniluoto 1984). After every step we reach closer and closer to the final solution (truth), and successive levels encompass all previous steps. This is analogous to the process of iteration. Convergence in the sciences is really problematic. According

to Rescher: (1) there is progress in scientific research, and also growth; (2) the research process entails systematization of hypotheses using data, and this is balanced process: we cannot know more about nature than we can ask through our questions; (3) there will always be scientific revolutions (Rescher 1987). Thus, convergence is impossible. Science describing reality perfectly is pure idealization, and scientific realism is over-optimistic when declaring progress towards truth via the sciences.

Niiniluoto agrees with the claim that truth in the strong sense is not accessible. However, truth is not utopian either. The crucial problem for a theory of scientific progress is rather how it is possible to make rational appraisals that affirm that we have attained progress towards truth (Niiniluoto 1984).

Laudan, instead, speaks about the success of theories, and according to him, there is no connection between the success and the truth claims of a scientific theory. Theoretical terms' having reference itself does not guarantee the success of the theory, on the other hand, does not warrant reference either (Laudan 1984). Laudan's argument against approximate truth is that we cannot know how far from the truth we are, therefore, an approximate truth is not the truth. Any theory may be successful without constituting truth. The only criterion of success in science is problem-solving ability, thus, successful theories need not explain the failed former theories as Laudan finds it possible to deduce from Niiniluoto's theory.

In Niiniluoto's opinion, success in the sciences is being evaluated only in retrospect (Niiniluoto 1986 a). Progress might be represented in backward-looking and in forward-looking terms. Niiniluoto himself prefers the latter: "It is natural to define real progress in forward-looking terms: the cognitive aim of science is to know something that is still unknown, and our real progress depends on our distance from this destination, but, as this goal is unknown to us, our estimates or perceptions of progress have to be based on backward-looking evidential considerations." (Niiniluoto 1986 a.) This means that the history of science can play an important role, a point that Giere takes up: "By looking back at evolutionary history, scientists themselves can better understand their own cognitive situation and investigate the development of their own cognitive capacities. Using our enveloped capacities we extend our knowledge of the world, including our knowledge of our own cognitive abilities. This latter knowledge helps us to extend our knowledge of the world still further, and so on." (Giere 1988.)

Such reflections easily lead to an evolutionary epistemology. Indeed, obtaining knowledge is an evolutionary process. Originally science began as a continuation to the adaptation of man to nature. But, we must not overestimate evolution in contemporary science, otherwise the result

will be instrumentalist again: science would be nothing but a tool for human action, and the question about truth would have no meaning (Niiniluoto 1984).

Biological models are often used as processing analogues in epistemology. For instance, it is common to find the model of scientific growth as exponential in analogy with the growth of biological populations, or, the model of the "knowledge-tree" — a tree-like structure which grows up from a common stem into more and more branches. Popper as an advocate of evolutionary epistemology has written: "We choose the theory which best holds its own competition with other theories, the one which, by natural selection proves itself the fittest to survive ... a theory is a tool which we test by applying it, and we judge as to its fitness by results of its applications." (Campell 1974.) Sometimes Popper is referred to be a Darwinian philosopher of science because of his notion for the survival of the fittest (hypotheses).

The method of trial and error is, however, not simply identical with the scientific approach. Theories must be testable, there must be a system of choice which guarantees objectivity. In the history of science the theory of natural selection is confirmed by the fact that many scientists have reached the same results in the same domain of science in independent ways.

According to Toulmin, another philosopher of science who uses the biological metaphor, a collection of concepts, methods and fundamental aims corresponds to a population of genotypes which constitutes a gene pool. The task of a model is to give an evolutionary account of historical development or various intellectual disciplines through conceptual change. New variants of these are generated within the discipline (conceptual variation) and they may get their place in the science in an evolution process with two intellectual factors, in intellectual selection and ecology (Toulmin 1972).

Kuhn has also used biological analogies to describe scientific development as an "unidirectional and irreversible process" (Kuhn 1970). This means that in an evolutionary true representation of scientific specialities one can distinguish the earlier theories from the later ones by some objective criteria. The term "unidirectional" involves possible teleological understanding of scientific growth. Against that Niiniluoto asserts that there are unidirectional processes which are not teleological or goal-relative. On the other hand, there is an aspect of biological evolution (growth of knowledge) which is goal-directed indeed: species adopt to their environment (Niiniluoto 1984, 1986).

Supposing that there is progress in the science does not mean that we assume this in a teleological way. "Science is a truth-seeking activity but it does not guarantee any factual connections between the practice of

science and its success in terms of truth finding. That such a (probabilistic rather than necessary) factual connection nevertheless exists, is due to the skilful use of the best methods of research. (Niiniluoto 1986.)

Approximate truth model

Truth is not easily accessible or recognizable from the realist point of view. Niiniluoto's theses *R4* and *R5* distinguish critical realism from naive realism which thinks that there is simple accumulation in development of knowledge. It is, in principle, possible to get nearer and nearer to the truth, and to make rational assessments of such a cognitive progress. This is what separates scientific realism from scepticism which denies the possibility of all true knowledge and progress towards the truth as well.

Of course, the terms "closeness to the truth" and "approximate truth" should be understood as metaphors. Proper tried to explicate the concept of "closeness to the truth" using another term: truthlikeness or verisimilitude — likely to the truth or similar to the truth. The term has the same etymology in many European languages as Niiniluoto has found: German "Wahrscheinlichkeit", Swedish "sannolikhet", Finnish "todennäköisyys" (Niiniluoto 1987 b). What concerns the Russian term, the present author would prefer to use the term "pravdopodobie" instead of "veroyatnost" which Niiniluoto offers and which could be translated as probability. And we can add accordingly the Estonian term "tõepärasus" that means truthlikeness.

For Popper, the concept of truthlikeness represents the idea of approaching comprehensive truth or whole truth. Suppose *T* to be the class of all sentences which are true in the Tarskian sense of correspondence theory. In order to have a high degree of truthlikeness a theory *A* should have a large truth content $A \cap T$ and a small falsity content $A \cap F$.⁵ Then, theory *A* and the whole truth *T* closely overlap each other. This definition of verisimilitude by Popper is the culmination of a long historical tradition (Niiniluoto 1986 b; Tichy 1974; Goldstick, O'Neill 1988). The only problem in connection with Popper's theory is that it does not work. If we have theories *A* and *B*, both of which are false, then one of them cannot be more truthlike than the other, since two kinds of error — falsity and incompleteness are not independent of each other, they increase and decrease together.

There is an alternative way of defining verisimilitude, initiated in 1974 by Tichy and Hilppinen, that relies essentially on the concept of similarity. Niiniluoto has developed this strategy using the concept of the

⁵ About the concepts of "error" and "falsity" see Niiniluoto 1978 b.

distance of a proposition or a theory from some fragment of the world. "Such a fragment does not include "the whole truth" about the reality, but only these aspects which are expressible in the language which we employ in stating our research problem." (Niiniluoto 1984.)

The cognitive problem can be seen as a set of hypotheses

$$B = \{h/i \in I\}$$

Let h^* be the unique element of B that is true in the Tarskian sense in W_L (fragment of reality). Then h^* is the unknown target of our problem and closeness to the truth is explicated "locally" by means of the distance to h^* . Potential answers to our problem are given by g :

$$g = \bigvee_{i \in I_g} h_i, \quad I_g \in I.$$

The degree of truthlikeness:

$$M(g, h^*) = 1 - d(g, h^*),$$

where d is the distance between elements in B . Of course, in a large majority of cases the size of h^* is unknown. Therefore we need an estimated degree of truthlikeness:

$$ver(g/e) = \sum_{i \in I} P(h_i/e) M(g, h_i),$$

where $P(h_i/e)$ is epistemic probability which gives measures of a rational degree of belief in the truth of h_i at a given evidence e .

The theory of truthlikeness gives us a systematic tool for defending a realist theory of scientific progress. Let h and h^* be rival theories and L be the ideal language for this domain. Thus, h^* is closer to the truth if and only if h^* has a higher degree of L -truthlikeness than h . And if it is so, the step from h to h^* is a progressive one.

Niiniluoto has successfully reconciled realism and relativism in his model of truthlikeness, whereas it is usual to contrast realism with relativism. Relativism is relative, it may concern ontological, semantic, epistemological or axiological categories. On the other hand, relativity may apply to persons, groups, cultures, environments, languages, theories, conceptual frameworks, points of view, gender, social practices, values, interests, etc.

Niiniluoto finds relativism acceptable when it implies that knowledge claims to be in some way relative to the position from which a person argues or makes assertions, but he does not accept the stronger claim that truth and reality are also relative to social interests. The semantic concept of truth, as explicated in Tarski's model, gives us an objective relation between a sentence (language) and a structure (features of the actual world expressible in language). This relation is non-epistemic in the sense that it either obtains or not, independently of our knowledge or

beliefs — but not in the sense that it would be impossible to obtain fallible evidence about it.

Many philosophers accept realism within a conceptual framework and relativism between these frameworks. In this view, the choice of a framework is not a cognitive but a practical matter, relative to our variable interests and purposes. In Niiniluoto's opinion the incommensurability of frameworks (languages) is frequently exaggerated, there always exist ways of comparing the cognitive success of theories involving conflicting meaning postulates. And upon that, it is important to say that the concept of truthlikeness allows more relativity than truth in the strong sense. The degree of truthlikeness $M(g, h^*)$ is not a purely semantic concept like truth is, but it depends on our cognitive interests in a given situation of research. Truthlikeness as an epistemic utility characteristic of science is also able to give an account of both consensus and dissensus within the scientific community (Niiniluoto 1991).

Conclusions

We have tried to answer the question whether scientific realism was too optimistic when declaring the possibility of progress towards the truth in the sciences. In the paper we have analysed critical scientific realism as an example of realism. Critical scientific realism, in the shape of the theory of the Finnish philosopher Ilkka Niiniluoto, is a doctrine that distinguishes itself from logical empiricism (positivism) and pragmatism by asserting the modern version of the correspondence theory of truth; from descriptivism and instrumentalism by asserting the thesis of applicability, in principle, of truthvalue to all linguistic products of scientific research.

According to scientific realism, truth is an essential aim of the sciences, although not accessible in the strong sense of "whole truth", while antirealist theories speak about epistemic surrogates of truth, such as empirical adequacy, problem-solving ability, coherence within theories, etc.

Truth is not easily accessible. This thesis separates critical scientific realism from naive realism and scepticism.

The main problem for critical scientific realism is that of giving appraisal to the cognitive progress occurring in the sciences. Niiniluoto offers a model of estimated truthlikeness which enables us to compare different hypotheses as solutions to some cognitive problem through epistemic probability in the case of the given evidences. Thus, we can make a decision if one stage is an improvement in comparison to an another stage in scientific research, improvement in some respect, of course, relative to some criteria.

Critical scientific realism is an optimistic but not overoptimistic theory. Let me remind expressions emphasizing that "truth is not easily accessible", we "approach truth via our errors, step by step", etc. It is indeed analogous to the biological species' adaptation to their environment: the only way for human beings to survive has been to obtain knowledge about their environment and to behave using that knowledge. (This served as an argument against scepticism — the position according to which truth as correspondence might exist, but there is no evidence available about that.) Human beings have made an endless number of mistakes in their history, including the history of scientific research, however, there is progress in the sciences that we can estimate when comparing different stages of diachronic development.

The advantage of the realist theory is that it enables us to deal with a remarkably larger domain of problems, than its alternatives do, offering answers to both ontological and epistemological questions, covering pragmatic aspects of cognitive theories. Hereby it is suitable to refer to the theories of Laudan, van Fraassen and of social constructivists. Laudan's theory claiming the purpose and criteria of success of science to be problem-solving ability could be considered as an extreme case of realism: if a theory is of high truthlikeness, it should be the best solution of the cognitive problem. Van Fraassen in his turn insists on the adequacy of theories to phenomena. True theories must be adequate to the phenomena, but adequacy itself does not warrant truth(likeness), it does not involve problem-solving ability either. If social constructivists claim the content of science is a purely social construction, realists will ask: constructed by whom and constructed about what? Construction refers to something real, unconstructed.

Critical realism is an optimistic theory, asserting that our knowledge is more or less about the world, about reality. Thanks to this correspondence between theories and real things of the real world it is possible to explain events happening under certain conditions, it is possible to predict events which will happen in the future. Realism, in principle, can serve as a basis enabling us to build up sophisticated models of cognition, develop ideas either about epistemic utilities or social basis of interest in knowing anything.

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MAKING SENSE OF THE APPLICATIONS OF THE CATASTROPHE THEORY

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The purpose of this paper is to express some new viewpoints about some aspects of the applications of the catastrophe theory. There were quite intensive debates throughout 1970s about whether the catastrophe theory really had any important applications. During the last decade the debates have relatively calmed down. This should enable us to take a more neutral look at the problem than in the writings published before. On the other hand, the previous critiques have been based mainly on the mathematical aspects of the catastrophe theory. We attempt to attack the problem from the philosophical point of view. Therefore we concentrate our attention chiefly on the applications of the catastrophe theory (CT) to the social sciences. To say in the other words, we shall be dealing with the "applied" catastrophe theory (ACT).

Why do we think the CT may be applicable to social phenomena? The task of this paper is not to give a full answer. Let us only grant that the CT has features, which refer to its probable dialectical nature. The historical development of society can be described with the help of the laws of dialectics (struggle of the opposites, transformation of quantity into quality), therefore the CT should be suitable to formalize at least some social phenomena. At the same time, by the opinion of the founder of the CT, the French mathematician René Thom, "if some disciplines, like social sciences and biology, resisted mathematical treatment for so long, even if they have succumbed, this is not so much because of the complexity of their raw material, as is often thought (all nature is complicated), but because qualitative and empirical deduction gives them sufficient framework for experiment and prediction." (Thom 1975: 2.)

Let us take a short look at some concepts on which the CT is based. Such account may help us to give a sound analysis of the applications of the CT. The central idea of the CT is discontinuity. The idea is as old as human thinking itself. The problem of continuity-discontinuity was already present in Zeno's paradoxes. Although the word "continuity" (or "discontinuity") cannot be taken there in its strict mathematical sense. Zeno argues about the continuity of motion. It can be doubted whether it is possible to prove in an empirical manner, if the motion of a body is continuous. For example, " 'cinematographic' motion, in which the

jumps are so minute that they defy observation, would for us be indistinguishable from continuous motion." (Wedberg 1982: 54.) The ACT models meet very similar difficulties. It even seems that we can use the same type of language dealing with Zeno's paradoxes and with the methodology of the application of the CT.

To fulfil the task connecting the paradoxes of Zeno with the CT models, we have to take a more concrete standpoint, from which we intend to approach the ACT. We shall concentrate our attention on some of the elementary catastrophes, proposed by the founder of the CT René Thom. The most suitable ones for building models are the cusp and the butterfly. We need not give their detailed descriptions here. This has been done, for example, by E. C. Zeeman (Zeeman 1976: 65-82). Let us only recall that the cusp has five basic characteristic properties: catastrophe, divergence, bimodality, hysteresis and the existence of an inaccessible region. The central moment of these is the catastrophe. It occurs, if the continuity of a process is interrupted. All the other properties characterize the possible features of the catastrophe.

Most of the models based on the cusp refer to any kind of discontinuity. This is the principal reason why many scholars have managed to apply the CT to such a great diversity of subjects. If we consider motion to be cinematographic, objective reality is full of discontinuities, which can be taken as catastrophes. So there can be no limits at all for the ACT. This is not surprising. The reader is certainly aware of the view in the methodology of modelling that anything can be a model of everything, except itself. In principle, the matchbox can be the model of society. One can point out several features, which are common to the mentioned objects. For instance, they both occupy a limited space. Naturally we cannot speak about analogy here. Still we can say that false models do not exist, but one model can be preferable to another.

The paradoxes of Zeno represent a sequence of discontinuities. A catastrophe takes place at any moment. But all paradoxes formulated during antiquity are not connected with Zeno. One of these is the paradox of baldness. From the viewpoint of the CT we can give two interpretations to it. Let us formulate the paradox according to A. Wedberg: "If a man with n strands of hair is bald, then one with $n + 1$ strands is also bald." (Wedberg 1982: 134.) Beginning the argument with $n = 1$ and continuing it, for any $n > 1$, one proves that any man is bald. We can interpret the problem the same way as Zeno's paradoxes. Then we can say that the adding of every strand is a catastrophe.

Our previous interpretations of Zeno's paradoxes and the paradox of baldness are purely quantitative. Numerous mathematical tools have been elaborated to treat that kind of problems. The CT does not have any advantage in quantitative analysis. The real power of the method can be

tested if we face a problem of a qualitative nature. As a matter of fact, the paradox of baldness offers a possibility of qualitative interpretation. To give it, we have to find the kernel of the paradox. The principle stated above is obviously applicable only to sufficiently small numbers n . The kernel of the problem is to indicate where the boundary between baldness and non-baldness lies. The existence of such a boundary means that the problem is no more purely quantitative but also qualitative. The transformation of quantity into quality cannot be abrupt in our case. There exists a certain area of values of n when classification as bald or non-bald is impossible. Nevertheless, there exists a class of bald individuals and one of non-bald ones.

The situation corresponds to R. Thom's law of compensation: "when the catastrophes are frequent and close together, each of them, taken individually, will not have a serious effect, and frequently each is so small that even their totality may be unobservable. When this situation persists in time, the observer is justified in neglecting these very small catastrophes and averaging out only the factors accessible to observation." (Thom 1975: 43.)

Processes, like the transformation of a non-bald person into a bald one have resisted mathematical formalization for a long time. We take the paradox of baldness as the basic case to analyze the methodology of building models with the help of the cusp catastrophe. In this case we have two stable regions: baldness and non-baldness. Each of these fit to the lower or upper sheet of the behaviour surface of the cusp where the behaviour of the system is stable. Individuals who can be named either bald or non-bald form the bifurcation set. It is obvious that the qualitative change (catastrophe) can occur only in the bifurcation set. Let us emphasize the feature which is crucial in making the paradox of baldness analogous to the model of the cusp catastrophe. It is the fact that both can be interpreted qualitatively as well as quantitatively. It is possible to give exact numerical answers to certain problems using the CT, but dealing with the ACT, the majority of writers have been interested in qualitative interpretations. A catastrophist is not interested how many strands a bald person can have. His goal is to formalize the process of qualitative change.

In terms of Hegelian dialectics, we have the problem of measure in the ACT. The theory seems to be well fitted to formalize processes, which contain the breaking of measure. The basic critiques of the ACT claim that the CT is not the best mathematical method for treating the problems of above-mentioned character. We shall not follow such kind of discussion. Our task is to provide an evaluation to the ACT. To do so, we need to take a look into the basic problems of the methodology of modelling.

At first let us recollect, what the purpose of modelling is. By building a model a researcher hopes that some feature of the process under study will be described more clearly by the model than in objective reality. This refers here to the problem of the reversibility of processes. According to synergetic all processes in objective reality are unique. If so, modelling can have no sense at all. From the viewpoint of natural science such an opinion can not be acceptable. A philosopher cannot stop thinking either, knowing that all his thoughts are unique. Besides, we know that the thought patterns can be taken as imaginary models. So let us not abandon the problems of modelling completely.

There should be some kind of isomorphism between two objects so that we could look at one of them as a model of the other. At the same time the object under study has to become a tool of cognition. There can be no model without a subject. Modelling goes together with the abstract dialectical view on cognition given by Hegel. The division of cognition given by him was the following: 1) nature; 2) human cognition; 3) the form of reflection of nature in human cognition.

Dealing with the problems of modelling, we have to make clear our attitude to the question of truth. We have to take into account two different approaches: truth as the correspondence of knowledge to reality and truth of logical judgements. Looking at the model from the heuristic point of view, we can discuss the adequacy of the model just in connection with a definite object and a definite problem. It is necessary to take into account that a subject has only a limited ability to pose a problem and to choose the model. The decisive power strays in the objective reality. An analogous situation is formed while drawing conclusions from a model's analysis. Here the subject's possibilities face certain limits as well.

Let us conclude the last argument as follows: "We consider model *M* of object *O* adequate to problem *P* if and to such extent as it produces true judgements in the process of cognition, which are confirmed by practical experience. As it produces false judgements, a model is non-adequate. Such treatment does not contradict to the notion of truth in a broad sense. Truth is always a binary relation while adequacy is a ternary one: object-problem-model." (Valt 1975: 23.)

Some researchers have expressed the opinion that it makes no sense at all to speak about the adequacy of models. It has been suggested that the real world is so complicated that any model cannot reflect it even approximately. Such an opinion goes well together with the basic ideas of synergetics that everything happening in objective reality is unique. At the same time models are quite widely used in synergetics. We cannot avoid the difference between the world of scientific theories and the real world. No model can be absolutely adequate. But still many of them re-

flect some features of a process under study. Our task is to find out if any of the models based on the CT belong to the latter category.

We are going to use the following method. Let us take the totally negative opinion on the ACT by H. J. Sussmann and R. S. Zahler as the basis of the argumentation. We shall argue if such an opinion can hold. It sounds as follows: "While it may be possible to draw sensible inferences from some CT models, they lead to at least as many nonsensical conclusions. No CT model that we have seen is quantitatively correct, and the qualitative conclusions drawn are frequently wrong or vague or tautologous. Finally, the models do not really make testable predictions. It is thus hard to see what contribution CT has made to the biological and social sciences." (Sussmann, Zahler 1978: 212.) The dialectics says that the source of development is contradiction. The strongest contradiction to our last quotation is formed by the views of E. C. Zeeman, who has been called a Twentieth-century Zeno. E. C. Zeeman has been the most prolific builder of models based on the CT. But in spite of that, his opinion about experiments that might test the CT is the following: "There are none! Like Differential Equations Theory, Catastrophe Theory is a mathematical theory, not a scientific one, that nevertheless gains interest from its applications. Of course there are many applications, both good and bad, and some of those can be tested — but the best people to ask about experiments are the practitioners in the specific fields, who have proposed models and are working with them, rather than to ask mathematicians." (Zeeman 1976: 213–214.)

As we see, we face problems, the solutions of which are obviously negative. Does it make sense to deal with them? At least we try to give a philosophical abstraction of the crucial moments of the ACT. At the same time we are going to pay more attention to the transformations of the language of science that have occurred or are likely to occur in connection with the introducing of the CT. We have to take into account two aspects of the problem: the transformations of the language of science themselves and the modelling of such transformations by the ACT.

Let us begin with the first aspect. There can be two kinds of transformations. The meaning and amount of some notions can change and new terms can be introduced. Needless to say, we try to connect our argumentation with the applications of the CT as closely as possible. The central problem in this case for the CT is the use of the following terms: "quantitative", "qualitative", "continuity", "discontinuity", "divergence", "jump", and even the term "catastrophe" itself.

The CT is said to be qualitative rather than quantitative by the Catastrophe Theorists themselves. At the same time the ACT repeatedly perpetrates "spurious quantification". To build the models, the ACT turns the terms like "range", "threat", "fear" into real-valued variables

(Zeeman 1976: 199). But without such kind of transformations, it would hardly be possible to create any mathematical models. What about so-called "qualitative reasoning"? It certainly does not mean that qualitative theories are immanently good and quantitative ones are immanently bad. Sussmann and Zahler claim that most of the Catastrophe Theorists share the opposite opinion. Let us take the dog aggression model by Zeeman as the basis of our further argumentation (Zeeman 1976: 65–68). Here we face the controversy continuity–discontinuity. The problem is quite similar to Zeno's paradoxes. Sussmann and Zahler claim aggression to be a clearly discrete phenomenon, which has been turned into a continuous one in the ACT model. At the same time they assure continuity to be a local property. Again a paradox arises. Continuity, being a local property can be globally discontinuous itself. One can even ask: how local is locality? As we see, the most convinced critics of the CT face problems similar to Zeno's paradoxes as well. It is also quite hard to agree with the opinion of critics about aggression. The model of aggression contains a jump. This should mean that aggression has not been turned into a purely continuous phenomenon. And is it really absolutely clear that aggression is discrete?

The principal cause of misunderstanding between the critics of the CT and the Catastrophe Theorists seems to be just the different consideration of some crucial semiotic units. Analysing the use of the terms "quantitative" and "qualitative" in the discussions about the ACT, it becomes clear that the scholars expressing their opinion are quite ignorant of the categorical structure of dialectics. According to dialectics, we should consider the terms "quantitative" and "qualitative" as opposites, which can act in cooperation. The critics tend to use the words "quantitative" and "qualitative" as semiotic units having clearly different meanings. If we remain on the logical plane, such understanding is correct. But as we know already from Zeno's paradoxes, adequate perception of objective reality by purely logical thought is impossible. On the other hand, changing the meanings of words within one argument can obviously not be permitted. Otherwise we get equated to "anything goes". The latter principle can be useful to provoke interest to certain thought patterns, but the present writer does not believe in the permanent effectivity of "anything goes".

Let us now make an attempt to connect our argumentation with everyday experience. Once again we turn to Zeeman's dog aggression model. It has been said to "predict that a non-frightened dog, if enraged, will not attack." (Sussmann, Zahler 1978: 198.) Sussmann and Zahler claim such prediction to be wrong, qualitatively as well as quantitatively. But everyone, who has some experience with angry dogs, knows that a frightened dog is much more dangerous than a non-frightened one. At least in this case the CT model fits quite well to everyday experience.

Another question is, do we find any testable prediction here, or is it postdiction. It still seems that we have proved the following. An ACT model can be reasonable. The critics, who are convinced of the opposite, manipulate with the changing meaning of some basic terms.

Dealing with mathematical modelling, the problem of locality-globality is immanently present as continuity or differentiability are local properties. The differential equations are local, but one can extract global information from them. One can make any decision about a model only by this information. Most of the mathematical models contain the controversy of locality and globality. We may know that a system has at least one stable equilibrium. This says little unless we have some idea of the size of the domain of attraction of the equilibrium. And still the historical experience of applying mathematics allows to think that most of the local mathematical properties have some useful output into global objective reality.

To go on we have to fix at least approximately the meaning of globality. It is possible to speak about globality in the mathematical and everyday sense. But what is globality in the sense of modern natural science? Here we cannot do without considering the problem of time. We can speak about globality at a fixed moment. But at the next moment such globality may not hold. What about globality for a longer time interval? We can talk about prediction only if the answer to the last question is positive. In the light of the latest achievements of so-called post-nonclassical science it cannot be. The last decades of scientific research have shown the falseness of the initial goal of Newtonian mechanics connected with predictability. It has been proved that the ideas of the determinism of systems based on Newtonian dynamics do not hold (Lighthill 1986: 35-50). It has been made clear that to speak about global predictability in the context of Newtonian science does not make sense at all (Stone 1989: 123-131). But does it make sense in the context of the new postnonclassical science? Obviously not. One of the basic ideas of synergetics just says that long-time prediction of whatever nature is impossible. So it makes no sense to care about the global properties of mathematical terms.

The last remark also seems to affect our understanding of qualitative mathematics, if mathematics can be qualitative at all. It can be more correct to speak about qualitative methods in mathematics. The latter characterization suits for the CT also. The term "qualitative" has gone through remarkable transformations during the existence of the post-nonclassical science. For Rutherford qualitative was nothing but poor quantitative. Still the transformations have little, if anything at all, to do with new mathematical methods. They have occurred due to contemporary natural science.

Next we undertake a survey of the use of the terms "continuity" and "discontinuity". The use of these terms in the CT should be compared to their biological and psychological meaning. Some critics link the treatment of continuity-discontinuity with the problem of globality, claiming to test an ACT model. We need some global information, or at least some local information that can be integrated to yield global conclusions. In the light of the latest scientific achievements we can see that the search for global information is hopeless. How does this recognition affect the decisions about discontinuous behaviour? We cannot decide about continuity discontinuity without considering the use of the term "jump". We face the contradiction between everyday experience and scientific cognition. There should be nothing mysterious in the fact that every break of continuity in any process, which can be depicted by scientific methods, cannot be perceived as a jump by the human senses. But this is just the crucial point for the critics of the ACT. It must have been chosen quite narrow-mindedly. It is a well-known truth that rational cognition and sensual perception cannot be identical.

The situation changes a bit if we deal with a model. We build one directly for the use of human cognition and we have to base on it. But still our dependence on the ability of human cognition is not total, like it was in the times of Hegel. Nevertheless, the difference is not so deep that we cannot apply the division of cognition by him. We only have to take into account the possible transformations of the form of reflection of nature in human cognition. It depends on our point of view if there have been any transformations. In the abstract philosophical sense obviously not. But the human cognition has obtained powerful tools of help. For instance, computers and electronic microscopes. Another question is if we can consider the information which has been received with the help of such technology adequate. Some methodologists share the opinion that nothing should be taken for granted which has not been thought over by human mind. In such case, we cannot use computer results to improve human cognition, unless they are computed over "on paper". As a matter of fact, there exists a large number of cases where it is impossible. For instance, one human being cannot calculate all the possible combinations of the four colour problem during his lifetime. The only way is to trust the computer. Contemporary physicists face same kinds of problems. There are whole fields of physics where all the basic results have been produced with the help of modern electronic apparatus. If we refuse to trust these results, natural science would probably come to its end soon.

What can we conclude from the last remarks for our continuity-discontinuity problem? It seems we should consider a process discontinuous if the discontinuity can be discovered even only with the help of some additional equipment. When we have discontinuity, some kind of jump is always present. Of course it can be the so-called technical jump,

which may be not observable in everyday experience. Sussmann and Zahler claim that models, exhibiting jumps nonobservable by human senses, do not make sense. This can be true, when we deal with social sciences. But we must not forget that the first reasonable applications of the CT belong to thermodynamics. R. Gilmore states that it is possible to formulate thermodynamics in the terms of the CT. He considers such "translation" useful in two aspects. One of them can help to connect equilibrium and nonequilibrium thermodynamics. For R. Gilmore this is the crucial point of contemporary thermodynamics (Gilmore 1984: 238). Thus, R. Gilmore provides a proof for the statement that the CT is in fact a language which can be useful for scientific theories.

It would make sense to analyze more properly the connections of the CT with nonequilibrium thermodynamics. Here we take the term "nonequilibrium thermodynamics" as a synonym of the theory of self-organization or synergetics. It may seem strange why we suddenly turn to a theory which is apparently physico-chemical by its nature. But let us remember how one of the founders of synergetic, H. Haken, has characterized the theory: "Synergetics deals with systems, which consist of subsystems of a very diverse nature, such as electrons, atoms, molecules, cells, neurones, mechanical elements, photons, organs, animals or even human beings." (Haken 1985: 19.) So, dealing with synergetics, we do not need to quit the area of our main interest. To give an explanation of the links between synergetic and the CT, we need to introduce the term "chaos". It is necessary to remember that basically there exist two types of chaos: nonequilibrium turbulent chaos and thermal chaos. The latter exhibits itself in the equilibrium conditions. The time-space scales are microscopic here. In the case of turbulent chaos the large number of the macroscopic scales of timespace create the impression of the chaotic behaviour of the system. It is quite difficult to draw a demarcation line between "chaos" and "order" here. Turbulent chaos is in fact not chaos at all. It can be taken also as part of the process of self-organization. One can observe certain regular patterns that lead to turbulence. For example, the pattern of Feigenbaum, the principle of which is the duplication of the period of change of the nature of the system under study when some control parameters pass a critical value. A certain kind of hierarchy of discontinuities (catastrophes) appears. Regularity can be observed not only on the way to turbulence but also in the final condition. We can say that chaos cannot be taken as a synonym of lack of order, as well as physical vacuum cannot be equated to nothing any more (Arshinov, Klimontovich, Sachkov 1986: 407).

The role of chaos differs significantly in open and closed systems. In closed systems only physical chaos is possible as maximal disorder is an equilibrium situation. The approach to chaos in open systems is completely different. Here dissipate structures emerge from physical chaos as

a result of self-organization. It is interesting to remember that the idea of chaos as the initial state of the world has maintained its importance from the very beginning of philosophical and scientific thought up to the modern times.

Turbulence can be characterized by a large number of degrees of freedom. Still, this fact is not sufficient to name a motion chaotic. There exists no quantitative measure for chaos. Therefore traditional mathematical methods cannot be used to formalize the patterns of chaos. There is no great help from qualitative methods either. There exists a strong need for a theory which could estimate the rate of order of the structures which have emerged in open systems. Here we reach the boundary of feasibility of contemporary exact science. Methodologists claim that quantitative analysis of systems with a large number of degrees of freedom could be impossible. Nevertheless, the very last results from the school of I. Prigogine have provided some hope. If the latter appears to be unjustified, it would probably make sense to give up the traditional scientific methods in this field. The new possible approach can be based upon the epistemological anarchism of P. K. Feyerabend, although it is also unacceptable to make the nonscientific method an absolute one.

We have been dealing with chaos in general. Let us now consider what impact the last arguments can have in social sciences. Contrary to prebiological ones, biological systems are unavoidably open. This openness means that the synergetic explanation of the development of biological systems is quite simple. If chaos is present in such a system, the start of the process of self-organization is guaranteed. In society things are not so simple, of course. Instabilities appear in society as they do in other kinds of open systems. In this sense, chaotic situations play a kind of progressive part in society as well. It is also obvious that too much chaos and anarchy in society will lead to tragic conclusions, which is too high a price for ensuring the development of society.

We find strong similarity in the behaviour of closed and open systems in general and in the social ones. Like anywhere else, closed social systems are not capable of development. Chaos in such kind of systems leads irreversibly to the devastation of the system. We can connect synergetic with the CT in this context. According to the CT, a catastrophic jump is necessary to change the nature of a system. There is no help from the CT in avoiding any social catastrophes. But the theory can be successful in finding common features in various kinds of qualitative jumps. It is important to know that the CT exhibits not only the disruption of continuity but is capable to model different kinds of qualitative changes. The most common elementary catastrophe for the ACT, the cusp, can be used to model various kinds of single jumps. But the more complicated types of catastrophes can also describe changes that consist of a sequence of jumps. Still the easiest way to undertake such modelling

is to connect two or more cusps. As a matter of fact we possess no information about applying more complicated types of catastrophes. Another elementary catastrophe, which has been used in applications, is the butterfly. A quite interesting interpretation, based on the features of the butterfly, has been given by M. Zwick to the dialectical struggle of the opposites (Zwick 1978: 129-154). His model shows that the struggle of the opposites may not give dominance to one of the opposites but can also end with reconciliation. The geometrical interpretation of the latter is the centre (pocket) of butterfly's bifurcation set. Nevertheless this does not mean that there will be no jump. The reconciliation of the conflicting factors can occur as the result of a qualitative jump. This model exhibits the features of modern society much better than the cusp models. Besides direct application to social changes, it can improve the dialectical understanding of the process of development. To go further, this interpretation may help to make dialectics (or at least its applications) more contemporary. The level of understanding the qualitative change in society achieved by Marx is certainly not sufficient to reflect similar aspects of the modern world. The movements in post-industrial society cannot be modelled by the struggle of antagonist classes. The moment of convergence is more important. Its achievement, however, marks the creation of a qualitatively new situation. So the basic meaning of the term "qualitative jump" does not change significantly.

How much do we know about the catastrophe and what impact has this knowledge on our ability of cognition of objective reality? Before the catastrophe occurs, some kind of instabilities should emerge. If the state of a system is not stable any more, it can rapidly proceed towards the catastrophe. The instabilities can be discovered, but it is very difficult to predict when the catastrophe is going to occur. It can be known within certain limits in some fields, but in society it is almost impossible to predict the moment and nature of the decisive change. For I. Prigogine it is absolutely impossible to predict when a system reaches the bifurcation point and what will happen after it has passed the point. It is only possible to influence the future behaviour of a system qualitatively when it is situated in the bifurcation point. Still the situation does not seem to be hopeless. Certainly there can be no exact prediction. But the area of possible new values of some decisive parameters can perhaps be estimated. This suggestion can be made taking into account the results of the chaos theory.

The CT is not so powerful in describing qualitative changes as synergetics or the chaos theory. Once again we have to remember that it is a mathematical method, not a scientific theory. It has been suggested that the bifurcation theory suits better to formalize synergetics than the CT. Such a decision must have been made as a result of quite superficial studies because the bifurcation theory can be taken as a special case of

the CT. If we attempt to formalise only the jump itself, the bifurcation theory is really quite sufficient. And it is not the only method to describe a discontinuity mathematically. The mathematics of the quantum theory or shock waves will do the job as well. The CT is a kind of mathematical method, but it also represents a certain style of thinking, which makes it rather a philosophy than mathematics. The same can be said about synergetic. It is a scientific as well as a philosophical theory. The chaos theory is connected with philosophy already through the philosophical essence of the term "chaos". But this certainly is not the most important link. The crucial moment of the problem is the fact that the chaos theory has shown that there can be different types of chaos. Besides, some phenomena which have been considered chaotic before, appear not to be chaotic at all.

The connections between the CT and the chaos theory may not seem so evident as those between the CT and synergetics. But there can be no doubt about the presence of a qualitative jump in the chaos theory. It occurs at the point where J. Lighthill's horizon of prediction is crossed. After that the values of the variables under study seem to express chaotic behaviour, but remain inside a certain area. The last remark goes together well with the last suggestions about the conception of I. Prigogine. Although we are not able to predict the next bifurcation, the development of the system under study after the bifurcation can still be estimated to a certain limit. Does a qualitative jump disappear? By no means. The appearance of the "regular" (deterministic) chaos marks a qualitative change. But it can no longer be taken as a jump into destruction. So it may seem that there is nothing to do with catastrophes. Here we have to remember that the term "catastrophe" in the sense of the CT means just a discontinuity. It is not necessary that a catastrophe of the ACT should model any kind of destruction. Instead it can mark a starting-point of a new development.

We have found out that the terms "qualitative jump" and "mathematical catastrophe" have become very closely related in the light of postnonclassical science. Now it would make sense to analyze if there is any difference at all. If we talk just about a jump, there is obviously no difference from the catastrophe of the CT. But we are interested in the qualitative jump. As the result of a qualitative jump a new quality should emerge. There is no need for a qualitative change after a mathematical catastrophe. The models of the ACT are simply built up to describe situations where a qualitative change does happen. The theory itself cannot contain any qualitative changes. It makes no sense to speak about qualitative mathematics. One can talk about qualitative mathematical methods. A better term is "discrete mathematics". But this can also rise confusion. For instance, is infinitesimal calculus discrete or not? We learned that the philosophical category "qualitative jump" is still basi-

cally different from the mathematical term "catastrophe". The everyday meaning of the same word is even nearer to the abovementioned category, being one type of qualitative change.

Next we shall discuss the relevance and changing meaning of another central notion for the ACT models, namely "divergence". Divergence in itself can take place into whatever direction. The CT enables to make the term more concrete. Divergence can occur only if the values of the control parameters belong to a certain, quite limited area. Speaking about the cusp models, we can say that divergence works for the values of parameters, which are close enough to the cuspidal point. For building models of real-life phenomena, the researcher has to locate such points in space and time. The adequacy of a model is directly connected with the preciseness of such locating. The future development of the system under study is determined in this area. The cuspidal point of the cusp catastrophe model is analogous to the bifurcation point in the scheme of I. Prigogine. This is the moment where fluctuations perform a decisive role in the development of the system. I. Prigogine seems to be mistaken, saying that the CT is unable to take into account any fluctuations. In fact, the cusp model is very sensitive to fluctuations. There seems to be a question of interpretation. Can a theory of natural science be compared to a mathematical method at all? It may really be impossible to formalize a situation that strongly depends on fluctuations, although an intuitive geometric interpretation of such a method can be given.

Divergence, as it appears in the ACT models, is still a quite local phenomenon. It determines the behaviour mode of a system before the catastrophe takes place. So it is not absolute divergence, but divergence for a short period. Besides, it is divergence where only two basic directions are possible. Divergence in the ACT models can be observed only in connection with the catastrophic change. The models become useless if we try to apply them to systems, where no catastrophe can be foreseen.

Mathematical divergence, described by the ACT models, has therefore a very narrow amount as a notion, compared to divergence in common sense or in social sciences. Still the models of ACT which express the phenomenon of divergence help to bring mathematical cognition closer to the real world.

Now we are going to give a summary of the impact of introducing the CT into contemporary scientific terminology. The CT has been built up using already existing terms. Some new aspects of the basic terms are inevitable if there are to be new theories. We are interested in finding an answer to the question whether the introduction of the CT has brought about any qualitative changes in the use of some terms. As to the pairs of terms having the opposite meaning, "quantitative-qualitative" and "continuity-discontinuity", the answer would obviously be negative.

Remarkable changes may be observed in connection with the terms "jump" and especially "catastrophe".

Nevertheless, the term "jump" has not obtained any new aspects of meaning. The ACT models have given rise to an interesting discussion about the extension and meaning of the term. It has become clear that the term "jump" is often used in extremely diverse situations. Generally, it only expresses the presence of any kind of discontinuity, perceived by either human senses or special electronic equipment. So there can be a confusion about using the terms "discontinuity" and "jump". Is there any difference between them? The answer to this question seems to be a matter of convention. Perhaps it would make sense to talk about discontinuities in science and about jumps in everyday experience. In dialectics "qualitative jump" could act as a synonym for "qualitative change", or it can be a special type of qualitative change.

The word "catastrophe" has obtained the status of a mathematical term besides its everyday meaning. This choice of the word for the corresponding term is not very successful because of the potential confusion with the common meaning of catastrophe. Any kind of catastrophe is a discontinuity and a jump. The mathematical catastrophe is not necessarily catastrophic. But this is characteristic of several mathematical terms that they do not correspond in their meaning to the analogous words in ordinary speech. This is, in fact, typical of mathematics itself. Therefore introducing terms into mathematics which make it closer to normal human communicative languages should be a thankworthy activity. The terms themselves are certainly not able to solve the whole problem. Still they create the formal framework which can be filled with the necessary contents.

As we know, the term "creation" is not the only aspect which connects the CT with the language of science. By the way, it has been suggested that the CT has been created only by introducing new terms. Even if this is true, the creation of the CT can be useful. Term creation has been a method of conscious scientific innovation at least from the times of H. Poincare. At the same time, term creation should never be made an aim in itself.

Now we shall take our chance to test if term creation (or transformation of terms) in the framework of the CT has made any sense. We are interested whether the CT can be applied to model the changes in the structure of language. "The structures found in a particular language can elaborate the archetypal basis in several ways: if on pragmatic grounds a subset of archetypes is not realized, it exists as a latent possibility which is actualized as soon as language use calls for it." (Wildgen 1982: 19.) For W. Wildgen semantic archetypes are holistic entities: linguistic *gestalts*. The conception of W. Wildgen is based on the triadic treatise of

Charles Sanders Peirce. Peirce speaks about icon, index and symbol. Wildgen insists on adding two aspects to Peirce's conception of similarity: stability and selectivity. The latter has to be divided into catastrophic selectivity and social selectivity (Wildgen 1982: 20). The catastrophic selectivity of Wildgen is a direct reference to the CT. "In a certain sense CT and logical semantics seem to be complementary rather than competitive." (Wildgen 1982: 24.) CT semantics is real world-orientated, while logical semantics is language-orientated. Logical structures are tools to capture real world phenomena. But they neglect the problem of structural stability and thermodynamic diffusion. They choose an a priori stable level of consideration. Therefore the logical structures represent the classical style of scientific thought in semantics, which is orientated towards stability. "The CT model is a synthesis which reintroduces real time in the structuralist framework, thereby creating a dynamic model of language." (Wildgen 1982: 30.) We see that the CT models reflect the postnonclassical methodology much more adequately than the logical structures. Logic is dependent on specific languages and specific levels. The principles of the CT, on the contrary, can serve as a unifying device bringing together the rather atomistic results given by the structural paradigm.

By the last remarks the CT models seem to be extremely successful in treating semantics. But their probable advantage before the logical structures does not help to avoid the usual limitations of the ACT. Still W. Wildgen suggests that the CT models furnish a platform on which an integrative model of language system, language use and language change can be built (Wildgen 1982: 112). By now the CT is apparently the most suitable mathematical method for handling the problems of language. It may be the only one which has remarkable perspectives in the field.

We shall try to progress further on analysing the ability of the CT to handle the problems of languages (especially the language of science). R. Thom starts his analysis by citing Condillac and Heraclitus. The first of them says that "all science is a well-made language." (Thom 1975: 117.) From this aspect Condillac can be taken as a predecessor of Wittgenstein. On the other hand, "it is no less true that all natural phenomena constitute a badly understood language." (Thom 1975: 117-118.) The last phrase corresponds to the Heraclitean idea of nature hiding itself. But Thom prefers to consider another fragment of Heraclitus: "The lord whose oracle is at Delphi neither speaks nor conceals, but gives signs." (Thom 1975: 118.)

To apply the approach of the CT, there should exist a qualitative transformation from one language to another. If we concentrate on the language of science, the first item of discussion would apparently be the existence of such transformations. Qualitative changes in the language of science can occur only if science itself can go through such kind of

changes. In the following argumentation we shall use the conception of Th. Kuhn, therefore taking the validity of scientific revolutions for granted.

The first remarks about the problem will be dedicated to the conception of Th. Kuhn itself. According to Martin Bronfenbrenner, Kuhn's paradigm hypothesis is "catastrophic" by its nature (Bronfenbrenner 1971: 136–151). Bronfenbrenner's catastrophe is not the mathematical catastrophe of the CT. Bronfenbrenner speaks about the complete disappearance of a paradigm or a mode or framework of thought and language in some branch of science, following a revolutionary upheaval. If Bronfenbrenner's suggestion holds, Kuhn's hypothesis is really catastrophic and can be described by an ACT model. To make any conclusion about the problem, we have to take a look at Kuhn's theory.

We can really speak about the complete disappearance of a framework of thought as the result of the scientific revolution. But it does not occur immediately after the change. Kuhn emphasizes just the opposite, saying the old paradigm will disappear completely only after the death of all of its followers. It certainly makes no sense to think that the real changes come with the death of the followers of the old paradigm. Before this happens, a third or fourth paradigm may appear. We find Kuhn's paradigm hypothesis not to be a concrete one. Maybe this is the point where the CT can help philosophy of science. There can be no question about describing a scientific revolution in Kuhn's sense with the help of an ACT model. Their approximate similarity is obvious. The main problem is well acquainted to us. Does modelling by means of the CT help to improve Kuhn's conception? Our answer to this question sounds as follows. If the conception of Th. Kuhn is valid, it can be concretized by the ACT models. If we can speak about revolutions in science, they should exhibit some kind of qualitative change. The latter can be modelled by the CT.

One feature of the cusp model may help to improve Kuhn's consideration significantly. The phenomenon of divergence enables the cusp model to reflect the birth of a new paradigm as the result of small-scale alterations in the style of thinking. The latter is quite usual for the beginning of a new kind of scientific theory. The starting-point of a new paradigm is the doubt, which can arise from a quite unremarkable defect in a scientific theory. It can be either esoteric (in the logical structure of the theory) or esoteric (in its applications). The development of human thought that led to the creation of the theory of the heliocentric world system or the theory of relativity should have been like this.

To avoid misunderstanding, we have to point out two possibilities of fixing the emergence of a new paradigm. At first it happens in the mind of a researcher. This could be called a potential paradigm (like the pri-

mary substance of Aristotle). The real paradigm comes into existence when it becomes accepted by the scientific community. The cusp catastrophe model is applicable to both considerations of paradigm change, although the first exhibits only a transformation in the mind.

Next we need to analyze the second part of the thesis of Bronfenbrenner. Does a paradigm change cause the complete disappearance of mode or framework of language in some branch of science? In the case of an affirmative answer to this question, the CT is obviously better suited to model the changes in the language of science than any type of logic. But even if the answer would be negative, according to W. Wildgen, the CT should help to interpret such kind of changes better than the logical structures.

The thesis of Bronfenbrenner does not seem to hold. In spite of that, it would make sense to speak about qualitative transformations in the mode of the language of science due to paradigm change. Let us take the Copernican revolution as an example to prove the last suggestion. There is a significant member of important terms the use of which does not alter after the Copernican revolution. The basic structure of the language also remains the same. But a strange thing happens. Opposite meanings are constructed, using the same words and the same syntactic structures. So there is in fact no change in the language. The choice of a word corresponding to a given meaning is the result of a long historical process. R. Thom calls it a quasi-permanent generalised catastrophe (Thom 1975: 118). The new paradigm can be expressed without changing the language itself. In such sense the thesis of Bronfenbrenner does not hold at all. The catastrophe takes place in another sense. As our main interest lies in the field of the language of science, we have to draw some conclusions about its transformations after the paradigm change.

"The appearance of language in main is a response to a double need:

1. For a personal evaluative constraint, aiming to realize the permanence of the ego in a state of wakefulness.
2. For a social constraint, expressing the main regulating mechanisms of the social group." (Thom 1975: 309.)

Dealing with the language of science, we should concentrate on the second constraint. It expresses the need for the social body to disseminate the information necessary for its survival. A single individual does not have the need to create a special type of language (the language of science). "The social constraint will create structures in the most unstable zone of the individual by an effect of interaction between hierarchical levels of organization." (Thom 1975: 310.) The structures can be on the shock waves separating sleep and wakefulness or genetic and spatial forms. The existence of instabilities offers a chance to describe these phenomena with the methods of the ACT.

There can be no talk of a catastrophe in the structure of the language of science in the common sense of the word. New scientific terms are usually introduced gradually. A qualitative supplement to a national language is made by creating a term apparatus in the language for expressing some branch of science rather than as a result of an overall paradigm change.

In fact, the scientific scene is more complicated than just changing from one paradigm to another. Usually several paradigms are in existence simultaneously. From the viewpoint of the catastrophe theory, it would be most convenient if there were two competing paradigms. In such a case the situation would fit well with the cusp catastrophe model. The paradigms can be represented by the lower and the upper surface of the cusp. The crucial ideas that determine the development of scientific thought lie in the area, where divergence can be exhibited.

Following the logical sequence, we reach the problem whether the overcoming of one paradigm by another is necessary, or maybe there can be a kind of conciliation between the competing paradigms. It seems that in fact there can be two possible types of the development of science, described in Kuhn's sense. One paradigm can gain total domination over its rival. Or none of the original competitors win and the result of the conflict can be some kind of interpenetration of the basic ideas of the competing paradigms. Of course, after such kind of interpenetration a third paradigm may emerge, that gains the dominating position. The second path of development is inaccessible for the cusp. The most appropriate elementary catastrophe for modelling such a case is the butterfly with its conciliation pocket. This property of the butterfly makes it more suitable for modelling situations of the objective reality than the cusp.

Surprisingly enough, the analysis of the Kuhnian paradigm thesis offers us not only a chance to test the applicability of the CT, but also enables us to search for the hypothetical dialectical nature of the CT from an original point of view. Richard X. Chase has developed the thesis that the essential elements of a Hegelian triadic process are imminent in the Kuhnian schema (Chase 1983: 812). The paper by Chase explores various facets of the relationship between Kuhn's thesis and the dialectical method. The thesis in the consideration of Chase is the paradigm as a "Disciplinary Matrix", antithesis — the competing paradigm. After an inter-paradigmatic debate a revolutionary synthesis is formed.

The correspondence of the cusp catastrophe model to the Kuhnian paradigm seems obvious from our former analysis. This supposition makes one of our main tasks quite easy. We just have to transfer the argumentation about the Kuhnian conception to a triadal dialectical model. And if, according to Kuhn's terminology, the scientific community finds

the model to be an adequate one, it has to be accepted. Still, our task may be more complicated.

Once again we face the problem of language. We agreed to treat the CT as a branch of the language of science. If so, the problem is reduced to the following. Can dialectics be expressed in another language rather than in which it has been created? Maybe the only "dialectical" languages are ancient Greek and German, the languages of Aristotle and Hegel. According to Heidegger it is really so. Therefore it should be impossible to explain dialectics to individuals who are not acquainted with at least one of these languages. There seems to be a certain amount of truth in the last statement. It is really quite difficult to translate some terms of Hegel. On the other hand, dialectics has been presented in very different languages, using very different characters in comparison with the common Latin alphabet (for instance the Chinese hieroglyphs). Are the Chinese ignorant of dialectics? Even if most of them appear to be, it can hardly be the matter of their language. More obviously it happens due to the different type of the whole culture. By the words of R. Thom, "it is well known that all speech can be decomposed into elementary phrases, each phrase being characterized by the fact that it contains precisely one verb, ignoring here the difficulties (about which specialists are still debating) of the definition of the traditional grammatical categories; noun (substantive), adjective, verb, preposition, and so forth. The fact that any text can be translated from one language to another confirms the belief that these categories are almost universal." (Thom 1975: 311.)

Now we shall switch from the language of human communication back to the language of science. Is there any principal difference between them? An answer to this question is the crucial moment of the problem of the formalization of dialectics. It would be nonsense to state that there can be a difference between perceiving a communication language unit and a mathematical symbol. Still, the mathematical symbol obtains a meaning only with the help of the language of communication. In fact, dialectics can be expressed by symbols of whatever kind. If we accept this approach, every phenomenon of the objective reality can be formalized. But what do we gain from such kind of formalization?

H. Sussmann and R. Zahler call the abovementioned activity spurious functionalization. They give the following example: "... one could translate the sentence "older people are wiser" into mathematics, by writing $W=f(a)$, $df/da>0$ where a is age and W is wisdom." (Sussmann, Zahler 1978: 196-197) It is really quite hard to imagine what can be gained by this translation.

The situation seems to be similar when we try to formalize the laws of dialectics. It is certainly possible to translate them into mathematics and perhaps at present the CT offers the most convenient apparatus to do

it. But it is hard to see what kind of help can dialectics have from this translation. Do we really need to examine or test the laws of dialectics, using mathematical properties?

In order to strengthen our argumentation, we turn to M. Heidegger. Heidegger states that mathematical research into nature is not exact because it calculates with precision. But all the "sciences concerned with life must necessarily be inexact just in order to remain rigorous." (Heidegger 1950: 120.) What kind of science is dialectics? From the natural philosophical point of view, dialectics should reflect the development of non-biological nature as well as that of living beings. In this case it would make no sense to attack dialectics with a mathematical tool. But in fact, dialectics gives only the idealizations of some crucial moments of the process of development. Dialectics in the common philosophical sense for Heidegger is the style of thinking of a scholar rather than a theory concerning objective reality. Real dialectics cannot actually be expressed in any language, even in a common communicative one. A philosopher can only think in the dialectical style. It can be realized but not explained in words, just like logos of Heraclitus.

We have not been able to find out the connection of dialectics and mathematics. The solution of the problem depends on what do we mean by mathematics. For modern mathematics as the language of science, there is no obvious link with dialectics at all. But we have to remember that for the Greeks *ta mathēmata* means "that which man knows in advance in his observation of whatever is and in his intercourse with things: the corporeality of bodies, the vegetable character of plants, the animality of animals, the humanness of man." (Heidegger 1950: 118.) The aim of R. Thom (either conscious or unconscious) seems to have been to approximate mathematics to the Ancient Greek significance of the field of knowledge, using the CT as a tool for this. Still the CT is more close by its nature to a new branch of the typical language of science than to mathematics of the Ancient Greeks as a cultural component. The most influential shortage of the CT in the light of its probable dialectical nature is apparently its inability to offer a deep insight into dynamic changes. The last argument has been supported by the words of I. Prigogine about the CT being a rather static and conservative dynamic theory. "CT is one of many attempts that have been made to deduce the world by thought alone... an appealing dream of mathematicians, but a dream that cannot come true." (Woodcock, Davis 1978: 5.) Still the CT is said to be more a philosophy than mathematics. Even as a philosophy it does not explain the real world. As mathematics, it brings together two of the most basic ideas in modern mathematics: the study of dynamic systems and the study of the singularities of maps. The CT brings them together in an arbitrary and constrained way (Woodcock, Davis 1978). R. Thom's original goal seems to be extending the theory towards

modelling the structural stability of life. This task refers to the original Greek *ta mathēmata* but has remained up to now unrealized.

Returning to dialectics, we can only repeat that mathematics as the language of science has nothing to do with it. Modern qualitative mathematical methods (like the CT) can in the best case serve as a complementary field of knowledge to dialectics. R. Thom's saying about the applications of the CT in psychology also agrees with the formalizability of dialectics. By his words, our goal is to find the best means to formalize the unformalizable (Thom cited in Woodcock, Davis 1978). The strongest link between ACT models and dialectics is perhaps formed by the high rate of idealization of both. The elementary catastrophe graphs have no scale. They show the canonical shape of each catastrophe surface. The models are highly idealized by the assumption that only a single potential is involved. Besides, the CT is a nonteleological method. It is unable to reflect the direction of the process of development like the law of the negation of negation in dialectics. Therefore, the interpretation of that law with the cusp catastrophe or a combination of several cusps by M. Zwick (1978) is invalid.

Despite the great hopes there is only one large-scale conclusion which we can deduce from the last argumentation. It is still too early to attack the basic epistemological problems with the existing methods of the contemporary *ta mathēmata*. Something qualitatively new is needed to gain success. Attempts have already been made. For instance, the fractal theory can be the next step from the CT to the real qualitative mathematics.

Now the final conclusions of the paper. Could we make any sense of the applications of the CT? We just learned that, as for the connections with dialectics, the ACT models can serve as complementary structures of scientific knowledge to the laws of dialectics. We may list the most promising proposed applications of the CT before we can make a conclusive suggestion. In doing so, we base upon the above-cited book by Alexander Woodcock and Monte Davis. In their opinion CT could be applied to the following fields: in physics (caustics and catastrophes, engineering catastrophe, buckling, phase transitions), chemistry and biology, in animal behaviour (territoriality and pendulum fighting, aggression and motivation, group formation), in sociology (social psychology: crowds and armies, status and marriage), economics (competition and prices, inflation and expectation), in politics and public opinion (political involvement and control, conflicting lobbies), in psychology (depression, reactive and process schizophrenia, anorexia nervosa, psychoacoustics), in the history of science (Kuhn's paradigm thesis).

Naturally we could not analyze all the possible applications within one paper. And we did not set up such tasks. The purpose of this paper is

rather to draw some overall conclusions of the problem of applying the CT to nonmathematized fields of knowledge. Direct analysis is also not necessary in all fields because it has been done already (for instance, by R. Thom himself in biology). Besides dialectics we concentrated our attention on the paradigm thesis in the history of science. It can certainly be disputed if we could add anything new to the existing analyses of Kuhn's conception.

What can be said about the somewhat prejudicial question of experimental control? R. Thom himself has expressed the opinion that the ACT models cannot be subjected to experimental control at all. The lack of experimentally verifiable predictions "is an inherent defect of all qualitative models, as compared with classical quantitative models." (Thom 1975: 321.) There are two reasons that might commend qualitative models to the scientist. "The first reason is that every quantitative model first requires a qualitative isolation from reality in setting up an experimentally reproducible stable situation. [...] The second reason is our ignorance of the limits of quantitative models." (Thom 1975:322.)

We need to decide whether the CT can help to find any new viewpoint about a problem under study. We can be quite sure of at least one advantage of the catastrophe theoretic approach. It makes the researcher to focus on the very crucial moments of the process under study. This should help to hit the kernel of the problem. Thus, the expression "catastrophe theoretic approach" should mark the style of research concentrating on the most unstable moments of a process under study. It is clear that this approach is insufficient to understand all kinds of processes. But it can be combined with some other methods. The catastrophe theoretic approach can give any useful result only if it is included into the synergetic context. The CT can act as a tool for making some aspects of the synergetic concept of change in the objective reality more rigorous and concrete. Perhaps it is the best result one can hope to get from a mathematical method that is not purely qualitative (if such is possible at all). This doubt rests on the following thought expressed by Heidegger: "... as soon as the gigantic is planning and calculating and adjusting and making secure shifts over out of the quantitative and becomes a special quality, then what is gigantic, and what can seemingly always be calculated completely, becomes, precisely through this, incalculable. This becoming incalculable remains the invisible shadow that is cast around all things everywhere when man has been transformed into subiectum and the world into picture." (Heidegger 1950: 135.) Man can create however powerful quantitative methods. But the invisible shadow will extend itself and always points to something which is denied to us to know today.

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COULD A PEASANT OF LOWER AUSTRIA UNDERSTAND LUDWIG WITTGENSTEIN'S 'TRACTATUS'?

Urmas Sutrop

Not all philosophers know that while Ludwig Wittgenstein was a schoolteacher in Lower Austria he published another book after his *Tractatus logico-philosophicus* (Wittgenstein 1922), a dictionary for Austrian elementary schools (Wittgenstein 1977).¹ This *Wörterbuch für Volksschulen* is regarded by commentators as a linguistic phenomenon (Burkhardt 1984 a, Burkhardt 1984 b, Rest 1962) or is treated in the framework of Ludwig Wittgenstein's later philosophy (Brose 1985, 1987, Burkhardt 1984 a, Burkhardt 1984 b), i.e., in the context of *Philosophical Investigations* (Wittgenstein 1958).

Some authors think that if there is any philosophy in that book, then such series of words must be some kind of prelude to Ludwig Wittgenstein's later doctrine about language games, or poetically — children's language and language games (Brose 1985: 9, Brose 1987: 77).

One of the commentators thinks that because of the definite articles referring to genders before substantives in this dictionary, this is a real philosophy (Hammel-Haider 1989: 269). Such an opinion assumes that the philosophy of language is mainly the philosophy of the use of the articles — definite or indefinite or maybe enclitic? Artlessly, such a view is to some point correct, for analytic philosophy is the philosophy of the occidental Indo-European languages.

If I think about ordinary subject matters, i.e., in the sense of common sense, then there is hardly any trace of word articles in my mind as my native language is not an Indo-European language. Actually, I think in Estonian, which is an Uralic language. Under the influence of Low and High German, my language has taken over some principles in word order and some words from German during the last seven hundred years. This makes my understanding of occidental philosophy easier. If I think about more complicated matters than my common sense requires, e. g., about analytic philosophy, I must think how I should think this in Ger-

¹ See a facsimile edition of this dictionary: Wittgenstein 1977. This new edition contains also the author's preface which was not printed in the original edition in 1926.

man or in English and all the time translate my thoughts from one language into another and back. In simpler cases I can use only some foreign words or phrases to denote words, concepts, etc. which are absent in my native language. Nevertheless I can express the results of my thoughts completely in Estonian — in oral or in written form.

According to Ludwig Wittgenstein this must be simple, for “translating from one language into another is a mathematical task” (Wittgenstein 1980: 139e #778). Though I can make myself a rule or a grammar for such kind of translations, this is not widely accepted convention in my language community, for “conventions presuppose the application of language” (Wittgenstein’s 1980: 13)².

I agree readily with the opinion that the boundary between earlier and later Ludwig Wittgenstein is not so sharp as it is usually thought, but a fiction (Bartley 1970: 349–366, 391–396; Bartley 1974: 307–337). In treating Ludwig Wittgenstein’s *Wörterbuch für Volksschulen* it is important for me to proceed from the following scheme: *Philosophical Investigations* are not a sequel to the dictionary but the dictionary is a sequel to *Tractatus logico-philosophicus*. Thus, I put the dictionary between *Tractatus* and the first series of Ludwig Wittgenstein’s lectures read in Cambridge in 1930–1932 (Wittgenstein’s 1980).

Is Ludwig Wittgenstein’s dictionary a direct continuation to *Tractatus logico-philosophicus*? *Tractatus* is a closed, finished text. After completing his manuscript of *Tractatus*, Ludwig Wittgenstein wrote to Bertrand Russell, “I think I have solved our problems finally”, and some days later, “I believe I’ve solved our problems finally,” and to John Maynard Keynes that he believed that he had solved the essential question (Wittgenstein 1974)³. He thought he had said almost everything in philosophy and had solved all meaningful questions. Hence there was nothing to do in philosophy any more.

In that sense the dictionary is similar to *Tractatus*, since the dictionary is also a closed, finished text.

“No word is too common to be entered” into the dictionary, wrote Ludwig Wittgenstein in his cryptic preface to his dictionary (Wittgenstein 1977)⁴. In addition, the dictionary is at the same time as normative and short as *Tractatus*.

² Lent Term 1930, Lecture A VII, #1.

³ P. 67, #R. 34 Wittgenstein’s postcard from Cassino (10. 3. 19.) to Bertrand Russell; p. 68, #R. 35 Wittgenstein’s letter from [Cassino] (13. 3. 19) to Bertrand Russell; and p. 112, #K. 9 Wittgenstein’s letter from Cassino (12. 6. 19.) to John Maynard Keynes.

⁴ See footnote no. 1, author’s preface p. XXXIII.

Is the dictionary a game? Ludwig Wittgenstein has said in one of his lectures:

When we talk about propositions following from each other we are talking of a game. Propositions do not follow from one another as such; they simply are what they are (Wittgenstein's 1980).⁵

This makes a difference. According to the above notion *Tractatus* is a game, for the propositions follow from each other, and we read *Tractatus* as a bound text. But in the dictionary we can see only alphabetically ordered words, their forms, and some expressions. This means that we cannot find any game in the dictionary, for only bound propositions form a game, but words are not propositions at all.

There is no smaller unit in language than the proposition; it is the first unit that has sense and you cannot build it up from other units that already have sense

uttered Ludwig Wittgenstein (Wittgenstein's 1980)⁶. In that sense language is a collection of bound sensical propositions and contains as an extra-propositional part the phenomenon of symbolism, which involves all the conditions which modify the signs (words) from which the propositions are composed, and all utterances which are not propositions, e. g., questions, interrogative sentences, and affecting utterances as Oh! & Ah!, i.e., all the utterances which are not deniable.

So the words are only the building stones for propositions — the signs. And

we give the scratch or the noise — the word — meaning with which it is used in the propositions which has sense (Wittgenstein's 1980)⁷

This may be the reason why the words in the dictionary are naked, without explanations and commentaries. The words obtain their meanings when they leave the dictionary and enter into propositions.

Using the dictionary, we may reconstruct the language of a peasant of Lower Austria. Suppose, for simplicity, that the dictionary's vocabulary is exhaustive, for "No word is too common to be entered," (Wittgenstein 1977) and the only way to create new words is to form compounds and to change the wordclass, e.g., use a verb as a noun and *vice versa*. These are our limitations and rules. Suppose, in addition, that the grammar — how to bind the words into propositions — is known to everybody.

Can we now read *Tractatus logico-philosophicus* using *Wörterbuch für Volksschulen*, i.e., translate *Tractatus* into a peasant's language? We may be supported by Ludwig Wittgenstein's argument that everything is translatable — "Translating from one language into another is a mathe-

⁵ P. 57. Lent Term 1931, Lecture B XIV, #2.

⁶ P. 57. Lent Term 1931, Lecture B XIV, #2.

⁷ P. 26. Michaelmas Term 1930, Lecture B II, #4.

mathematical task" (Wittgenstein 1980: 139, #778). But now we fall into difficulties. If everything is replaceable: a word by word, a joke by another, etc., there must be a one-to-one correspondence between any pair of languages.

In real languages we may form the one-to-one correspondence only if their vocabularies have equal powers. This is realized only when the number of terms is equal or infinite in the comparable vocabularies. For simplicity, let us assume that there are no one-many and many-one relations between any pair of vocabularies.

But what must we do if we have only a vocabulary with limited terms whose power is drastically smaller than that of the others'? To translate *Tractatus* into a peasant's language, we may start cultivating some kind of gematria. We may enumerate all terms and give them certain values in the dictionary and after that present our translation in a number language. But such number-lore is not our business ... To simplify our task we shall not try to read all the text, but only the main propositions of *Tractatus* as they have appeared in the plan for the work on the third page of the manuscript of *Prototractatus* (Wittgenstein 1971).⁸ We add to these propositions the last one which says that everybody should keep silent. We use *Prototractatus* as a framework and follow the text of *Tractatus*, actually following only the numeration from *Prototractatus*. The terms used for generation of new words absent in the dictionary are shown in parentheses. Propositions whose translations remain nonsensical are not translatable and so are not presented.

A peasant can read the plan of *Tractatus* considering these remarks and the above rules for word generations as follows:

- 1 *Die Welt ist alles was der Fall ist.*
- 1.1 *Die Welt ist die Gesamtheit der Tatsachen, nicht der Dinge.*
- 2 —
- 2.1 *Wir machen uns Bilder der Tatsachen.*
- 2.2 —
- 3 *Das Bild der Tatsachen ist der Gedanke.*
- 3.1 —
- 3.2 —
- 4 *Der Gedanke ist der sinnvolle (Sinn + voll) Satz.*
- 4.1 —
- 4.2 —
- 4.3 —
- 4.4 *Der Satz ist der Ausdruck (aus + Druck) der Übereinstimmung (über + eine + Stimmung) and Nichtübereinstimmung (nicht + über + eine + Stimmung) mit den Wahrheitsmöglichkeiten (Wahrheit + Möglichkeit) der Elementarsätze (Element + Satz).*

⁸ See facsimile of the author's manuscript p. 3.

- 5 *Der Satz ist eine Wahrheitsfunktion (Wahrheit + funktionieren) der Elementarsätze (Element + Satz).*
- 6 *Die allgemeine Form der Wahrheitsfunktion (Wahrheit + funktionieren) ist: /abracadabra/.*
- 7 *Wovon man nicht sprechen kann, darüber muß man schweigen.*

A peasant cannot read the second proposition, since he does not find the equivalent to *Bestehen*. Hence he cannot read the propositions 4.1, 4.2, and 4.3 either, for he cannot understand that he could take for *Bestehen* (become into existence) *Existenz* (existence).

How should he read the original German title — *Logisch-philosophische Abhandlung*? He cannot find such terms as 'logic', 'logical', 'philosophy', and 'philosophical' in this dictionary. If our peasant studies the term *Abhandlung* (treatise), then he finds only the terms *Handlung* and *abhanden kommen* (get lost, be mislaid). He could think that something has got lost or somebody had lost his way. Anyway, *Handlung* is for sure a deed or act or doing, and maybe it has a connection with some business. Maybe our peasant has seen or heard that there are *Buchhandlungen* (book-stores) in towns. Hence all that is a bookish business.

As we see, this translation into a peasant's language is simpler than the original and contains fewer propositions. The propositions whose translations contain words deduced from other terms using our former rules are not so easily understandable as the propositions whose translations contain words as only their prototypes, for word generation causes disturbances.

The picture he gets is simpler and not logical (cf. the translation of the third proposition). The first and the last proposition seem *prima facie* very simple. But how could our peasant know that there are very many possible words coming into existence and that he must first of all know what is the case.

The last proposition where everybody should be silent is in its simplicity very misleading. To understand this proposition, he must know the meaning of the terms *Bestehen* and *Nichtbestehen*. We can speak only about these cases which come into existence, and we cannot speak about these cases which do not come into existence.

The proposition is the unit of what can be said. A proposition is a description of fact, of what is the case, and is either true or false (Wittgenstein's 1980)⁹.

When Ludwig Wittgenstein read Ludwig Uhland's poem 'Count Eberhard's Hawthorn' he wrote,

⁹ P. 45. Lent Term 1931, lecture B IX, #1.

The poem by Uhland is really magnificent. And this is how it is: if only you do not try to utter what is unutterable then nothing gets lost. But the unutterable will be — unutterably — contained in what has been uttered! (Engelmann 1967.)¹⁰

We can conclude that *Tractatus logico-philosophicus* and the translation using Ludwig Wittgenstein's dictionary are quite different texts. Evidently a peasant could understand his own translation, but in his own way, for

understanding is really translation, whether into other symbols or into action. (Wittgenstein's 1980.)¹¹

Does the translation denote the same work of philosophy as the original? Are they same? To understand both the translation and the original he needs another language or metalanguage which would explain his own language and fill the gaps in his language and the lacunae in his text.

From here a very important question arises. Suppose that one day all Ludwig Wittgenstein's manuscripts, typescripts, and printed texts disappear and the only fragment which survives is our peasant's translation in this paper. Is this then the text of *Tractatus* and could we assert that we understand what Ludwig Wittgenstein wanted to say?

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THE CONCEPT OF MEANING IN WOLFGANG ISER'S THEORY OF RESPONSE

Margit Sutrop

I

Meaning is a pivotal term in both the analytic and the continental traditions of philosophy. This however does not mean that there is a consensus about the "the problem of meaning". The lack of sympathy between the philosophers of the analytic and the continental tradition has meant that there has been little communication between the two schools of thought. However, after Dagfinn Føllesdal's paper "Husserl's Notion of Noema" (Føllesdal 1969) there have been constant attempt to establish the historical link between Husserl and Frege and to discover similarities in their thought.

Recently some authors have suggested that Frege and Husserl have much in common for they started from the same standpoint:

- both were engaged in the fight against psychologism (although there is an endless discussion between Frege scholars and Husserl scholars about the question whether Frege's criticism of Husserl's early work "Philosophy of Arithmetic" helped Husserl to escape psychologism);
- both contrasted the objectivity of sense (*Sinn* for Frege and *Bedeutung* or sometimes also *Sinn* for Husserl in *The Logical Investigations*) to the subjectivity of the *Vorstellungen*.

Both Frege's and Husserl's premiss was that sense cannot be in the consciousness of this or that particular speaker if communication is to succeed (and they were both convinced that it will succeed). Therefore sense must remain the same, in contrast to *Vorstellungen* which may vary, as they always belong to somebody and are part of the content of individual consciousness.

If there are so many similarities in the theories of Frege and Husserl, why have there been so few contacts between the philosophers of the analytic and the continental traditions? It has been sometimes supposed that communication between the two schools of thought has been hindered by terminological confusion. It is true that, for example, for making the same discrimination between sense and reference Frege and Husserl used different terms. Frege used *Sinn* for sense and *Bedeutung* for reference. Although Husserl knew the terminological innovations

made by Frege in his 1892 article "On Sense and Reference", Husserl did not adopt them. J. N. Mohanty, however, assumes that Husserl must have arrived at this discrimination independently of Frege, for as early as in 1891, in his review of Schröder's "The Lectures on the Logic of Algebra" (Husserl 1891), Husserl distinguished between *Bedeutung* or *Sinn*, *Gegenstand* and *Vorstellung* (Mohanty 1982: 2-3). Husserl uses both *Bedeutung* and *Sinn* for sense, in "The Logical Investigations" mostly preferring *Bedeutung*, and instead of Frege's *Bedeutung* he uses the more common word *Gegenstand*.

According to another opinion, a further source of misunderstandings has been the fact that many crucial philosophical terms used by Frege have been translated into English in very misleading ways. Claire Ortiz Hill assumes that because of the different ways that Frege's terms have been introduced into the predominantly English-speaking analytic tradition, starting with the works of Bertrand Russell, the "communication, when attempted, between the two schools of thought, has been muddled" (Hill 1991: XI).

Frege's *Sinn* has been translated as both meaning and sense, and *Be-deutung* as meaning, reference, denotation, indication, significance, and used by his successors, who have written mainly in English, in a very misleading way. The English term "meaning" hides the distinction between the German terms *Sinn* and *Bedeutung*.

Curiously, there are even some *Encyclopaedias* which, influenced by the use of the English term "meaning", speak about *Sinn* and *Bedeutung* as equivalents. For example, in the "Lexicon of Philosophy" (*Philosophielexikon* 1991: 78–80, 529–530), edited by Anto Hügli and Poul Lübcke, we find two separate articles, entitled *Sinn* (Engl. meaning) and *Bedeutung* (English meaning) which coincide in every word, except that in the article entitled *Sinn* there is always instead of *Bedeutung* the word *Sinn*, and *vice versa*.

Still, the possibility remains that Husserl's and Frege's difference in distinguishing sense from reference is not merely terminological, but lies deeper: it may derive from the fact that Frege did not have Husserl's concept of intentionality. J. N. Mohanty assumes that the crucial point that distinguishes Frege's theory of meaning from Husserl's, and as a result, the analytic tradition from the phenomenological one, is that "meanings for Frege are meanings of signs (words, sentences); for Husserl they are meanings of expressive acts or speech acts. Meanings are "ideal contents", "intentional correlates" of acts, rather than selfsubsistent entities." (Mohanty 1976: XIV–XVIII.)

I am inclined to agree with J. N. Mohanty that the main difference between the approaches to the sense-reference discrimination does not lie in the different terminology used by the two scholars of philosophy. It

seems to me that things may even be the other way round — the fact that the same English word “meaning” is used by both schools of philosophy has created the false impression that analytic and phenomenological traditions of thought have much in common. Although both traditions develop theories of meaning, it does not mean that analytic philosophy and continental philosophy share the same subject matter. In my opinion more attention should be paid to what those theories of meaning are theories of, that is, what actually is considered to be meaning in those theories.

As the English word “meaning” can serve as an equivalent for both *Sinn* and *Bedeutung* (in Frege’s terms), it has not been noticed that most of the philosophers working in the analytic tradition are dealing mainly with *Bedeutung* and their theories of meaning are, actually, theories of reference and truth value, whereas the philosophers working in the phenomenological tradition are interested in meanings as *Sinn* (in Frege’s terms), although they are not concerned with meanings of words and sentences but meanings of mental acts.

II

The same confusion in the use of the concept of meaning can also be noticed in contemporary literary theories. During the last twenty years many different literary theories have presented theories of meaning. However they are not theories of the same thing. In the words of Stein Haugom Olsen, “It is a philosophical commonplace these days that the fact that different theories make use of the same term is not sufficient reason for assuming that they deal with the same phenomenon. The fact that the term meaning is in use in different literary theories is not sufficient reason to assume that they have common focus: the problem of meaning.” (Olsen 1982: 187.)

There has been a constant discussion in literary theory as to whether meaning should be identified with the author’s intention or with the product of reading; whether meaning is stable or unstable; objective or subjective; hidden behind text or assembled in the reading process; whether meaning is an object to be defined or an effect to be experienced. But very seldom have literary theorists asked what is the meaning of the “meaning” they are talking about. William Ray assumes in *Literary Meaning: From Phenomenology to Deconstruction* that “the very real differences between, say, phenomenological criticism and structuralism, or reader-response criticism and deconstruction, result less from a fundamental disagreement as to the nature of meaning and how it occurs in reading than from divergent strategies of representing this phenomenon within a critical discipline.” (Ray 1984: 13.)

There are reasons to doubt this claim. It seems to me that although many contemporary theories talk about meaning, there is a fundamental disagreement precisely in the question as to what meaning actually is. Even the reader-oriented literary theories that talk of meanings as generated in the reading process do not concur with each other about the nature of meaning.

The fact that different literary theories do not present views about the same thing, although they are all speaking about "meaning", has remained hidden as most of these theorists have not taken pains to define the concepts they are using. Donald Davidson has disclosed the character of verbal disagreement: identification of a common subject matter for divergent theories requires numerous shared beliefs about the nature of that subject matter. Too much divergence ceases to be divergence altogether, it merely changes the subject (Davidson 1975: 20-22).

It seems to me that just this changing of the subject has taken place in the discussions about the problem of meaning in contemporary reader-oriented literary theories. In those theories the concept of meaning has been given different contents. So it is that one theorist, denying the other's views on meaning and representing his/her own theory, is already speaking about another phenomenon.

In the following I am going to give an example of how this change of the subject in discussions on the meaning of the literary text may take place. I have chosen the theory of aesthetic response of Wolfgang Iser, one of the leaders of the Constance School, as he has presented his views on the concept of meaning in a well-developed theoretical framework. It is interesting to see how the fundamental disagreement about the nature of meaning makes the break between Iser's theory of response and traditional hermeneutics, and distinguishes his theory from the phenomenological aesthetics of Roman Ingarden, which is one source of Iser's inspiration. I am going to argue that the new content Iser gives to the concept of meaning allows him to attach central importance of the "imaginary" in the reading process and to go from the theory of response, which has some of its roots in phenomenology, to literary anthropology.

It seems to me that until recently not enough attention has been paid to the new content Iser has given to the concept of meaning. And therefore it has also not been noticed that the new views about the meaning of the literary text that Iser has presented do not really refute the traditional understanding of meaning. For Iser's theory is not a contradictory theory, in which the same thing has been shown in a new light, but it produces views about a new subject. My task will be to find out what kind of phenomenon it is that Iser has called "meaning".

III

What has interested Iser from the outset, is the question how meanings are generated in the act of reading. His position has remained unchanged since his polemical essay "Indeterminacy and the Reader's Response in Prose Fiction" (Iser 1971). He claims that reading is an event in which a meaning that has never existed before is assembled. Meanings are constituted by readers and not revealed by critics. Iser thus resists the traditional form of interpretation which is concerned with discovering the ultimate meaning that is supposed to be concealed within the text itself. Here the target of Iser is "the author of a certain well-known essay on 'The Art of Interpretation' " (Iser 1971: 4)¹ who according to Iser says that the meaning is concealed within the text itself.

It seems to me that here and henceforth Iser reduces all theories of interpretation to this claim. In this essay "Indeterminacy ..." as well as in his later writings Iser constantly refutes the view that meanings are qualities hidden in the texts. He repeatedly stresses that meanings are products of an interaction between the text and the reader. As the meaning of the text is generated by the individual reader, it appears always "with a slightly individualistic touch".

Let us see what arguments Iser uses in order to refute the claim of the "traditional kind of interpretation" — that the meaning is concealed within the text itself. At first Iser assumes that saying that meaning is concealed within the text itself is equivalent to reducing a literary text to one particular meaning. He argues in the following way, "If a literary text could really be reduced to one particular meaning, it would be the expression of something else — namely, of that meaning the status of which is determined by the fact that it exists independently of the text. Put in extreme terms this means that the literary text would then be the illustration of this meaning existing outside itself." (Iser 1971: 4–5.) Iser goes on to criticize those critics who have understood literary texts as an expression of the *Zeitgeist*, the author's neurosis or as reflections of social relations.

There is nothing new in this kind of criticism of the literary critics' attempts to present their own context-guided interpretation as the only possible one. But I think Iser has a one-sided view of such "traditional" interpretation. Or he simply does not want to see that interpretation is not an end in itself, but must provide us with a better understanding of a literary work. The real aim of the critic in most traditional models of hermeneutics is not to explain the hidden meaning but to understand a literary work, exposing the meaning in interpretation. But this criticism does

¹ Obviously Iser is speaking here about the Essay of Emil Staiger *Die Kunst der Interpretation. Studien zur deutschen Literaturgeschichte* (Zürich: Atlantis Verlag, 1955).

not seem to be really Iser's point. Iser's aim is not to prove that a text may be understood and interpreted in various ways. He is saying something quite different.

Having criticized in the previous passage the way in which critics read a literary text as an expression of one particular meaning outside the text, in the next passage Iser starts to speak about the specific aesthetic structure inherent in the text. This structure provides the preconditions for the feeling that we, as readers, often have when reading about past ages, that we are transported back into those times and take part in those events. Iser continues, "The preconditions for this experience are certainly provided by the text, but we as readers also play a part in the creation of this impression. It is we who bring the text to life." (Iser 1971: 5.)

What has this passage to do with the previous discussion about the meaning of the literary text? How does it prove that a literary text cannot be an expression of the meaning? At first sight there seems to be no apparent link between those two passages. But let us try to discover what Iser intended to say at this place, for it may be important in finding out the point at which Iser's concept of meaning differs from the "traditional" concept of meaning he is resisting.

In my opinion it is possible to understand this latter passage, cited above, only after having read Wolfgang Iser's most important theoretical book *The Act of Reading. A Theory of Aesthetic Response* (Iser 1976 b)². We can find the link between the two passages, between his criticism of the "traditional concept of meaning" and his own claim that the text only provides preconditions for its experience, if we know the central claim formulated in *The Act of Reading* that "meaning is no longer an object to be defined, but is an effect to be experienced" (Iser 1976 b: 10), (in German: *Sinn ist dann nicht mehr erklärbar, sondern nur als Wirkung erfahrbar* (Iser 1976 a: 22)).

The point is that instead of presenting his own views how meaning should be approached or laid out in interpretation, he denies altogether the possibility of reducing fictional texts to a discursive meaning. He does not see the possibility of meaning being verbalized in interpretation, how we could speak about the meaning of the literary text as every attempt to say in words what we have experienced makes the literary text plain. This view is supported by the claim that meaning is not a hidden object but an effect (*Wirkung*) of the text.

Iser's argument in *The Act of Reading* is the following: if meaning is an effect, it cannot be hidden in the text or lie behind the text as, according to Iser's another claim, a text has its response only in reading. If the text comes into life only in reading and has its response only through the

² First published in German as: Iser 1976 a.

reader, evidently there cannot be any meaning without the reader, outside the act of reading. Iser's premiss is that the reader has an important role to play in the reading process and that is the reason why he wants to prove that meaning cannot be hidden in the text: "If texts actually possessed only the meaning brought to light by interpretation, then there would remain very little else for the reader. He could only take it or leave it." (Iser 1971: 5.) Meanings are generated in the act of reading, they are the product of a complex interaction between the text and the reader. If it can be shown that the text can only have meaning when it is read (that meaning cannot be hidden in the text), then without the reader's subjective contribution there is no such thing as meaning.

But the question — why should we think that meaning is an effect of the text — still remains. Iser argues only why meaning cannot be an object of explanation: as "meaning as effect is a perplexing phenomenon, and such perplexity cannot be removed by explanations — on the contrary, it invalidates them. The effectiveness of the work depends on the participation of the reader, but explanations arise from (and also lead to) detachment; they will dull the effect, for they relate the given text to a given frame of reference, thus flattering out the reality brought into being by the fictional text." (Iser 1976 b: 10.) It turns out that Iser is contrasting two different kinds of approaches to the meaning of the literary text: experience and explanation.

In his *Act of Reading* Iser makes this difference clear in an interpretation of Henry James's story *The Figure in the Carpet* (Iser 1976 b: 3-10)³. This story written almost a hundred years ago helps Iser to reject the critics' search for the hidden meaning of the literary work, to show that "in view of the irreconcilability of effect and explanation, the traditional expository style of interpretation has clearly had its day" (Iser 1976 b: 10). (In German it is more exact: *Angesichts der Opposition von Wirkung und Erklärung hat sich die Funktion der Kritikers als Dolmetscher des verborgenen Bedeutung fiktionaler Texte überlebt* (Iser 1976 a: 23)). By opposing two characters in James's novel — the narrator (whom Iser calls the critic) and his friend Corvick — Iser tries to convince his reader that meaning cannot be reduced to the status of an object.

In James's short story the critic represents the view that the function of interpretation is to extract the hidden meaning from the literary text. The critic is in search of the "open secret", eager to explain the meaning he has discovered. But in the novel the critic fails as the work he is interpreting does not offer him a detachable message. According to Iser this proves that meaning cannot be reduced to a thing. The idea of

³ Here Iser speaks about Henry James's novel *The Figure in the Carpet* (The Complete Tales IX), Leon Edel, ed. (Philadelphia and New York, 1964.)

James's novel is that as long as the critic's mind is fixed on the hidden meaning, he is incapable of seeing the new element that has been brought into the world in the reading act.

The perspective of the critic's friend Corvick is supposed to show how the experience of meaning can change the life of the reader. Iser explains that Corvick experiences the meaning of the literary text, but the effect is so powerful that he cannot find words to express this experience. He cannot explain or convey the meaning as the critic seeks to do. Iser says that the critic gives the key to this different kind of meaning, which the author of the story, Henry James, himself has already underlined by calling his story **The Figure in the Carpet**.

The point is that "meaning is imagistic in character" (Iser 1976 b: 8), (in German: *Sinn hat Bildcharakter* (Iser 1976 a: 20)). It is easy to guess how Iser explains why the critic in James's novel "fails to see" — of course, because the critic mistakenly supposes that his task is to find out the hidden meaning. Iser declares that the critic fails to see because he does not understand that the formulated text represents a pattern, a structured indication to guide the imagination of the reader; and so the meaning can only be grasped as an image. The image provides the filling for what the textual pattern structures but leaves out. Such a "filling represents a basic condition of communication ..." (Iser 1976 b: 9.)

The mistake of the critic in James's novel is that he thinks that meaning can be grasped only within some frame of reference. Iser is passionately resisting this view and it seems that he is not describing James's novel any more but presenting his own theory when he argues that the image cannot be related to any such frame of reference. Image in Iser's interpretation does not represent something that exists; on the contrary, it brings into existence something that is to be found neither outside the book nor on its printed pages.

The new understanding of meaning as imagistic in character allows Iser to prove that the reader plays an important role in the reading process and that therefore there cannot be any meaning behind the text, outside the act of reading. Iser concludes, "However, if meaning is imagistic in character, then inevitably there must be a different relationship between text and reader from that which the critic seeks to create through his referential approach. Such a meaning must clearly be the product of an interaction between the textual signals and the reader's acts of comprehension. [...] As text and reader thus merge into a single situation, the division between subject and object no longer applies, and it therefore follows that meaning is no longer an object to be defined, but is an effect to be experienced." (Iser 1976 b: 9–10.)

I think it is now clear that the reason why Iser insists on the primacy of the experience of meaning over the explanation of meaning — as car-

ried out by the "traditional expository style of interpretation" — is that he has a different understanding of what the meaning of the literary text is. Iser's views that meaning is the effect of, or response to the text and that meaning is imagistic in character are very different from the concept of meaning that Iser thinks the traditional interpretation is looking for. How could we define this traditional kind of meaning that Iser is resisting? I have found some help from the *Dictionary of Concepts in Literary Criticism and Theory*, ed. by Wendell V. Harris, where we find nine different definitions of "meaning". In my opinion Iser thinks that the traditional theories of interpretation consider meaning to be "that which the utterance is understood as intended to convey within the full contextual situation of the utterance." (Harris 1992: 215.) Resisting the "traditional expository style of interpretation", Iser also denies the "traditional" concept of meaning, thus giving the concept of meaning a totally new content: speaking about meaning as "effect" or "response" (Iser says that the German term *Wirkung* comprises both effect and response, without the psychological connotations of the English word "response").

It is interesting that Wendell V. Harris in his article "Meaning" describes the second possible definition of meaning as "a response to a sign, that is, to a stimulus that causes its interpreter to take account of something other than, but related to, the stimulus." (Harris 1992: 215.) In his implication of this definition he also refers to the semiotics of Charles Morris (Morris 1964, 1971), who according to Harris has most generally formulated this sense of meaning, although preferring to avoid the term meaning itself.

It may well be that Iser's understanding of meaning as response has been influenced by the semiotic theory of Charles Morris. Iser himself does not cite Morris in relation to the problems of meaning. But where he gives a general explanation of his theory, Iser refers to Morris, saying that his interest "is directed toward the pragmatics of literature — "pragmatic" in Morris's sense of relating the signs of the text to the interpretant. The pragmatic use of signs always involves some kind of manipulation, as a response is to be elicited from the recipient of the signs." (Iser 1976 b: 54.)

All this shows that Iser is not providing new views about old phenomenon called "meaning" but develops a new theory about a new phenomenon that Iser has called meaning. This indicates that the subject matter in the discussion about meaning has really changed. In the following I am going to look more closely at the phenomenon that Iser calls meaning, trying to demonstrate which place the concept of meaning has in Wolfgang Iser's theory of reading.

IV

Iser's theory of aesthetic response (*Theorie ästhetischer Wirkung*) claims that "the meaning is not a definable entity but, if anything, a dynamic happening" (Iser 1976 b: 22). Meaning is understood as aesthetic response which Iser analyzes in terms of a dialectic relationship between text, reader and their interaction. Iser calls it aesthetic response because "although it is brought about by the text, it brings into play the imaginative and perspective faculties of the reader, in order to make him adjust and even differentiate his own focus." (Iser 1976 b: X.) As the meaning of the literary text is assembled in the reading process, we must at first know how Iser describes the relationship between text and reader.

Iser's main idea is that reading is not a one-way process from text to reader, but a "two-way traffic", an interaction between the two poles. The text itself is a "happening" (*Geschehen*), it takes on its reality by being read. The text contains certain response-inviting structures which help the reader to grasp the text. Iser calls them textual repertoires and textual strategies. Every literary text offers a perspective view of the world, it is the author's view. But a literary text in itself is also composed of many different perspectives which carry the author's view. In the novel Iser counts four main perspectives: those of the narrator, the characters, the plot and the fictitious reader. Those perspectives enable the reader to assemble the meaning and allow him or her to place him/herself within the world of fiction. According to Iser, "the literary text has its reality not in the world of objects but in the imagination of its reader." (Iser 1971: 43-45.)

Iser stresses that none of the textual perspectives is identical to the meaning of the text, "What they do is provide guidelines originating from different starting points (narrator, characters, etc.), continually shading into each other and devised in such a way that they all converge on a general meeting place. We call this meeting place the meaning of the text, which can only be brought into focus if it is visualized from a standpoint." (Iser 1976 b: 35.) The role of the reader is to fit diverse perspectives into a gradually evolving pattern. But the point is that although the textual perspectives are given by the text, their gradual convergence and the way in which they finally meet is not determined by the text but has to be imagined by the reader.

The creative role of the reader in the reading process results from the indeterminacy in the text itself. Iser gained inspiration from the Polish phenomenologist Roman Ingarden, who spoke in *The Literary Work of Art* (Ingarden 1973 b) about the places of indeterminacy (*Unbestimmtheitsstellen*) in the literary work of art, especially in the stratum of "represented objects" (*die Schicht der dargestellten Gegenständlichkeiten*). According to Ingarden this stratum consists of an unreal, in-

vented and imagined world that includes people, things, processes and events. While every real object is completely determined, imagined objects are never. As a literary work of art contains these places of indeterminacy, it is not completely determined. Ingarden describes the literary work of art as a schematic formation (*schematisches Gebilde*). The places of indeterminacy are partially removed in the concretizations that arise from individual readings of the work.

Iser took on Ingarden's idea that the "filling-out" of the places of indeterminacy is not sufficiently determined by the text of the literary work and therefore will vary with different concretizations. Iser sees in indeterminacy the peculiarity of all literary texts. He uses Ingarden's term "schematized views" (Ingarden 1965: 261 ff.) to express his own view that literary objects — which do not present any concrete object in the real world — come into being through the unfolding of the schematized views. Unlike Ingarden, however, Iser sees the indeterminate sections or gaps in literary texts as a basic element for the aesthetic response, as precisely those gaps in the text invite the reader to participate in the construction of the imaginary object.

Iser postulates that "the reader fills in gaps by a free play of meaning—projection" (Iser 1971: 12). While Iser sees the places of indeterminacy as indispensable parts of literary texts, Ingarden is worried about how arbitrariness in filling out the gaps can be avoided, and how the correct concretization of the literary work can be guaranteed. He repeatedly emphasizes that the literary work, although it may be fleshed out in concretizations, is different from the concretizations, "It is only expressed in them, it develops in them, but each such development (as long as it is not a mere reconstruction of the work) necessarily goes beyond it. On the other hand, none of the developments goes as far as the work itself..." (Iser 1973 b: 337.)

The different attitudes of Iser and Ingarden toward the places of indeterminacy could be explained by their different understanding of the mode of being of the literary work. Iser, who instead of "literary work" uses mostly the word "literary text", stresses that the text comes to life only through the act of reading. Therefore there cannot be any right concretization of the literary work (or literary text, in Iser's terms). Ingarden, contrary to Iser, believes in the intersubjective accessibility and the identity of the literary work which is in his mind guaranteed by the meaning-units that form the second stratum of the literary work. In *The Literary Work of Art* Ingarden calls verbal meanings ideal entities, in the spirit of his teacher Edmund Husserl. In *The Cognition of the Literary Work of Art* (Ingarden 1973 a) he characterizes them as the objective intentional correlates of the mental acts which have the same structure at every time when the same meaning is intended.

According to Ingarden, verbal meaning, the meaning of a sentence and the meaning of the text are something objective, which remain identical in their core, transcendent to all mental experiences. On the other hand, meaning is an intentional configuration of appropriately structured mental experiences. In the phenomenological approach, the first step in understanding a verbal sound is finding the precise meaning intention. As the meaning is "conferred on" the word in an intentional mental experience, the word is recognized and used according to the kind of intention it has.

While understanding the text, the reader must do something analogical, find the meaning intention, which basically is an actualization of that intention. Ingarden explains how we understand the text, "... when I understand a text, I think the meaning of the text. I extract the meaning from the text, so to speak, and change it into the actual intention of my mental act of understanding (Husserl would call this a "signitive act"), into an intention identical with the word or sentence intention of the text. Then I really "understand" the text." (Ingarden 1973 a: 32.)

While Ingarden speaks of the whole stock of intentional sentence correlates which he calls the "portrayed world" of the work (Ingarden 1973 a: 31), Iser's starting point is to describe how these correlates intersect. He supposes that the intersection of sentences in a literary text gives rise to semantic fulfilment. He assumes, "The fulfilment, however, takes place not in the text, but in the reader, who must "activate" the interplay of the correlates prestructured by the sequence of sentences [...] the sentences set in motion a process which will lead to the formation of the aesthetic object as a correlative in the mind of the reader." (Iser 1976 b: 110.) Iser's aim is not to grasp the literary work of art itself but to discover how the reader translates and transfers the text to his own mind.

While Ingarden describes the process of reading as an uninterrupted flow of thinking the sentences, Iser is convinced that the sequence of sentences is full of surprising twists and turns. He explains that the wandering viewpoint permits the reader to travel through the text, "thus unfolding the multiplicity of interconnecting perspectives which are offset wherever there is a switch from one to another. This gives rise to a network of possible connections. This network of connections potentially encompasses the whole text, but the potential can never be fully realized; instead it forms the basis for the many selections which have to be made during the reading process ..." (Iser 1976 b: 118.) The wandering viewpoint is a means of describing the way in which the reader is present in the text. It divides the text up into interacting structures and these give rise to a grouping activity. So Iser does not describe the reader's activity as finding and actualizing the meaning intention, he speaks about the grouping activity as fundamental to the grasping of the text.

V

Iser points out that psycholinguistic experiments have shown that meanings cannot be grasped merely by decoding of letters or words, but can only be compiled by means of grouping. In *The Act of Reading* Iser claims that meaning is at a level of language where words do not belong. As meaning is not manifested in words, the reading process cannot be a mere identification of individual linguistic signs. Therefore the apprehension of the text is dependent on *gestalt* groupings which he defines with the help of Abraham A. Moles as the "autocorrelation" of textual signs (Iser 1976 b: 120).

In confronting textual signs, readers try to establish connections between them. But at the same time there is originally already some potential correlation between the signs. According to Iser the reader's task is to make these signs consistent. The whole process is called "consistency-building". By identifying the connection between the textual signs the reader will constitute the *gestalt*. The *gestalt*-formation entails both the selection of certain elements and the exclusion of others.

On the other hand, Iser describes the *gestalt* coherency as the perceptual *noema* of the text. Here Iser borrows a term from Aron Gurwitsch, who in his book *The Field of Consciousness* develops this concept in conjunction with Husserl's concept of the sense of perception. The perceptual *noema* of the text means for Iser that the reader "identifies the connections between the linguistic signs and thus concretizes the references not explicitly manifested in those signs. The perceptual *noema* therefore links up the signs, their implications, their reciprocal influences, and the reader's acts of identification, and through it the text begins to exist as a *gestalt* in the reader's consciousness." (Iser 1976 b: 121.)

Iser's central claim here is that through *gestalt*-formation the reader participates in the text and is "caught up in the very thing produced". The product of consistency-building is the meaning of the text. Iser affirms, "... as we read, we react to what we ourselves have produced, and it is this mode of reaction that, in fact, enables us to experience the text as an actual event. We do not grasp it like an empirical object nor do we comprehend it like a predicative fact; it owes its presence in our minds to our reactions, and it is these that make us animate the meaning of the text as reality." (Iser 1976 b: 128.)

Iser speaks about the image-making activity of the reader. While we read, we are continuously and unconsciously constructing images in the process which Iser calls (referring to Husserl) "passive synthesis", the basic element of which is the image (in German *das Bild*). Iser stresses that "the image brings something to light which can be equated neither

with a given empirical object, nor with the meaning of a represented object, as it transcends the sensory, but is not yet fully conceptualized." (Iser 1976 b: 136.) Iser reports that he does not use the word "image" like the empiricists, and neither considers he the image an optical vision. It is rather "the attempt to ideate (in German *vorstellen*) that which one can never see as such. The true character of these images consists in the fact that they bring to light aspects which should not have emerged through direct perception of the object. "Imagining" depends upon the absence of that which appears in the image." (Iser 1976 b: 137.)

Iser distinguishes between perception and ideation. Perception (*Wahrnehmung*) occurs only when an object is present to be perceived, while ideation (*Vorstellung*) depends upon the absence or nonexistence of an object. Reading entails ideation because, aside from the marks on the page, the reader must ideate the object. Image is basic to ideation. It is important to notice that these mental images have optical poverty, which shows that they do not serve to make the object physically visible as it happens to the characters of a literary work when we see them in a film. Iser refers to Gilbert Ryle who has pointed out that we "see" something in our image of an object which we cannot see when the object is actually here. The process of synthesizing enables us "to produce an image of the imaginary object, which otherwise has no existence of its own." (Iser 1976 b: 139.)

The role of the "imaginary" in the reading process results from the fact that literature is fiction, which means that the fictional objects do not have any reference in the reality. In this peculiarity of the literary texts — that they do not denote any objects in the given reality — Iser sees the possibility of a new kind of communicative relation between fiction and reality. Instead of seeing fiction and reality as opposites, Iser speaks about their coming together in literature. He assumes that the literary text is a mixture of reality and fictions, hence it brings about an interaction between the imagined and the given.

In his most recent book *The Fictive and the Imaginary. Charting Literary Anthropology*, Iser claims that "we might do better to discard the old opposition of fiction and reality together, and to replace this duality with a triad: the real, the fictive and what we shall henceforth call the imaginary. It is out of this triad that the text arises ..." (Iser 1993: 1.) Here we see how the new content that Iser has given to the concept of meaning allows him to attach central importance to the imaginary in the act of reading, and move his interest from the theory of aesthetic response to literary anthropology.

The difference between image-building in literature and image-building in every-day life is that in every-day life our knowledge of the real object preconditions our image of it, but the literary text does not

denote any objects outside the text. In literature there is no empirical outside object with which to relate the image.

The process of image-building begins with the schemata of the text, which according to Iser are aspects of a totality that the reader must assemble. "In assembling it, he will occupy the position set out for him, and so create a sequence of images that eventually results in his constituting the meaning of the text." (Iser 1976 b: 141.) Imagistic in character, meaning is dependent upon the reader's imagination; it is to be found neither in the words printed on the page nor outside the book (i.e., referentially).

The meaning of the literary text can only be fulfilled in the reading subject and does not exist independently of him. In constituting the meaning, the reader him/herself is also constituted. Iser says that "this experience is what underlies the reader's desire to comprehend the significance of the meaning. [...] in assembling the meaning we ourselves become aware that something has happened to us, and so we try to find out its significance." (Iser 1976 b: 150.)⁴ Iser continues that meaning (*Sinn*) and significance (*Bedeutung*) are not the same thing, although the classical type of interpretation has identified them.

Iser cites Gottlob Frege as an authoritative source, emphasizing that in "On Sense and Reference" Frege writes, "The fact that one has grasped a meaning does not yet make it certain that one has a significance." (Iser 1976 b: 150.) In the next sentence Iser claims, "The significance of the meaning (*die Bedeutung des Sinnes*) can only be ascertained when the meaning is related to a particular reference, which makes it translatable into familiar terms." (Iser 1976 b: 150-151.) The relation between Iser's claim and the citation from Frege is not very clear to me.

Frege never uses such an expression "the significance of the meaning" and neither does he speak about meaning and significance (I would prefer "sense" and "reference" as the equivalents of Frege's *Sinn* and *Bedeutung*) as different stages of understanding. Frege is only concerned with the connection between a proper name and its sense and reference, "A proper name (word, sign, sign combination, expression) *expresses* its sense, *stands for* or *designates* its reference. By means of a sign we express its sense and designate its reference." (Frege 1993: 27.) The reference of a proper name is the object itself which we designate by its means. Frege says that we want every proper name to have not only a sense but also a reference because we are concerned with its truth value. To cite Frege, "It is the striving for truth that drives us always to advance from the sense to the reference." (Frege 1993: 29.)

⁴ Iser cites from: Frege 1892: 28.

When Frege says that in grasping a sense one is not certainly assured of a reference, he is stressing the fact that, while every expression is in correspondence with a definite sense, the case is not so with reference. There are expressions which may have no reference, for example, the expression "the least rapidly convergent series", which has a sense but no reference. And also the proper names in the works of fictional literature (Frege says *Dichtung*) often have only sense but no reference.

Iser is trying to prove his claim — that meaning and significance are two different things — also with the help of Paul Ricoeur. I cite the whole sentence of Iser, to show how he reads Ricoeur, "As Ricoeur has written, with regard to ideas advanced by Frege and Husserl: "... there are two distinct stages of comprehension: the stage of "meaning" ... and the stage of "significance", which represents the active taking-over of the meaning by the reader, i.e., the meaning taking effect in existence." (Iser 1976 b: 151.)⁵ We see that Iser cites Ricoeur incompletely, leaving some gaps in his sentence. I think that it is important to discover in which context Ricoeur speaks about Frege and Husserl. Only in this way it will become possible to fill out these places of indeterminacy in the sentence cited above.

Here is the full sentence of Ricoeur from the English translation of his "Preface to Bultmann": "The moment of exegesis is not that of existential decision but that of "meaning", which, as Frege and Husserl have said, is an objective and even an "ideal" moment (ideal in that meaning has no place in reality, not in the psychic reality). Two thresholds of understanding then must be distinguished, the threshold of "meaning", which is what I just described, and that of "signification", which is the moment when the reader grasps the meaning, the moment when the meaning is actualized in existence." (Ricoeur 1974: 397.)

It comes out that Ricoeur does not refer to Frege and Husserl in relation to the two stages of comprehension (meaning and significance), as Iser makes us believe, but in relation to the meaning as an objective and an ideal moment. Secondly, although Ricoeur really distinguishes between meaning and significance ("signification" in the English translation of Peter McCormick) as two thresholds of understanding, he stresses that the entire route of comprehension goes from the ideality of meaning to existential significance. Iser's citation allows us only to understand what Ricoeur means by "significance", but there is not a slightest hint to Ricoeur's "meaning". At this place where Ricoeur speaks about the ideality of meaning as the objective stage of comprehension, there is a gap.

⁵ The German version of the sentence of Paul Ricoeur that Iser cites here is in: Ricoeur 1973: 194.

Everything that Ricoeur speaks about the objectivity and the ideality of meaning is contrary to Iser's understanding of meaning. Therefore it is no wonder that he does not use these words of Ricoeur, "It is the objectivity of the text, understood as content-bearer of meaning and demand for meaning — that begins the existential movement of appropriation. Without such a conception of meaning, of its objectivity and even of its ideality, no textual criticism is possible." (Ricoeur 1974: 397.)

I do not see any reason why Iser needs the authority of Ricoeur, Frege and Husserl to make his distinction between meaning and significance as two different stages of comprehension. Iser's own definition is the following, "**Meaning is the referential totality which is implied by the aspects contained in the text and which must be assembled in the course of reading. Significance is the reader's absorption of the meaning into his own existence.**" (Iser 1976 b: 151.) (The same in German: *Sinn ist die in der Aspekthaftigkeit des Textes implizierte Verweisungsganzheit, die im Lesen konstituiert werden muß. Bedeutung ist die Übernahme des Sinnes durch den Leser in seine Existenz.* (Iser 1976 a: 245.))

Iser's claim is that meaning-assembly has the intersubjective structure as its basis. Does this intersubjective structure make meaning objective and ideal as it is for Frege, Husserl and Ricoeur? If Iser remains true to his theory of aesthetic response, the answer can only be — "no". The intersubjective structure as the basis of the constitution of meaning is only one part of the process that, according to Iser, brings meaning to the existence in the consciousness of the reader. Meaning as the product of interaction between the text (which carries an intersubjective structure of meaning-production) and the constituting activity of the reader must certainly be individual and subjective. Hence Iser's theory of meaning does not coincide with the theories of meaning developed by Frege, Husserl and Ricoeur.

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ART, MAN AND SOCIETY: THE FUNCTIONS OF ARTISTIC ACTIVITY

Leonid Stolovitš

The idea of the polyfunctionality of art has been firmly rooted in Soviet aesthetics since the nineteen-sixties. However, alongside a "centrifugal" tendency to increase the number of functions performed by art¹, there is also a "centripetal" tendency to reduce their number to three, and sometimes even to single one. The growing dissatisfaction by Soviet philosophers during the last decades of the USSR with the bare statement of the polyfunctionality of art has reasonable grounds, for without a common source to unite all its various and diverse functions, art itself would lose its unity and be reduced to a mere conglomerate of separate aspects. There is no need to argue whether it is preferable not to see the wood for the trees or the trees for the wood. We need both of them. It is possible to do this if we use the systematic approach, giving a concrete explication of the principles of dialectics for the study of system-forming objects.

A systematic approach to the study of the functional meaning of artistic activity is indispensable because the concept of 'function' is a concept of systemic approach. In terms of a systemic approach a function is the result of an essential activity of a system and its components. But at the same time we can also regard such functions as forming a system themselves. This system itself is dependent on the interaction of two systems, the first one being the system whose functions are described, and the second one being the system to which the actions of the first system are directed. Understanding the functions of art as an integral system is of paramount importance in determining the direction of its activity, for the correct interpretation of the social direction of any single one of these functions is impossible if they are taken separately, without relations to each other.

A work of art can have functional significance in different systems, for most diverse "environments". Nevertheless, these various functions are mediated by those functional meanings which art has for the social character and unique individual manifestation of human personality. The

¹ The word 'art' is used to cover also literature, music, etc. This has been a stock use of the word in Soviet/Russian philosophy. Therefore, 'artistic' and 'artist' are derived from this wide meaning.

system of functions specific to art is determined by the specific characteristics of art itself and by the specific needs of human personality and her relation to art.

I believe that any functional meaning specific to art is bound to be mediated by its aesthetic nature, and in this sense these functions are aesthetic. It is the participation in the mediation of aesthetic relations which unites the most diverse, seemingly entirely united functions of artistic activity, for example, cognitive and compensatory, communicative and heuristic, social-organizational and hedonistic, etc. The aesthetic relationships constitute the system-forming source of these different functional meanings of art, which make possible diverse "transitions" from a work of art to human personality and from the latter to society.

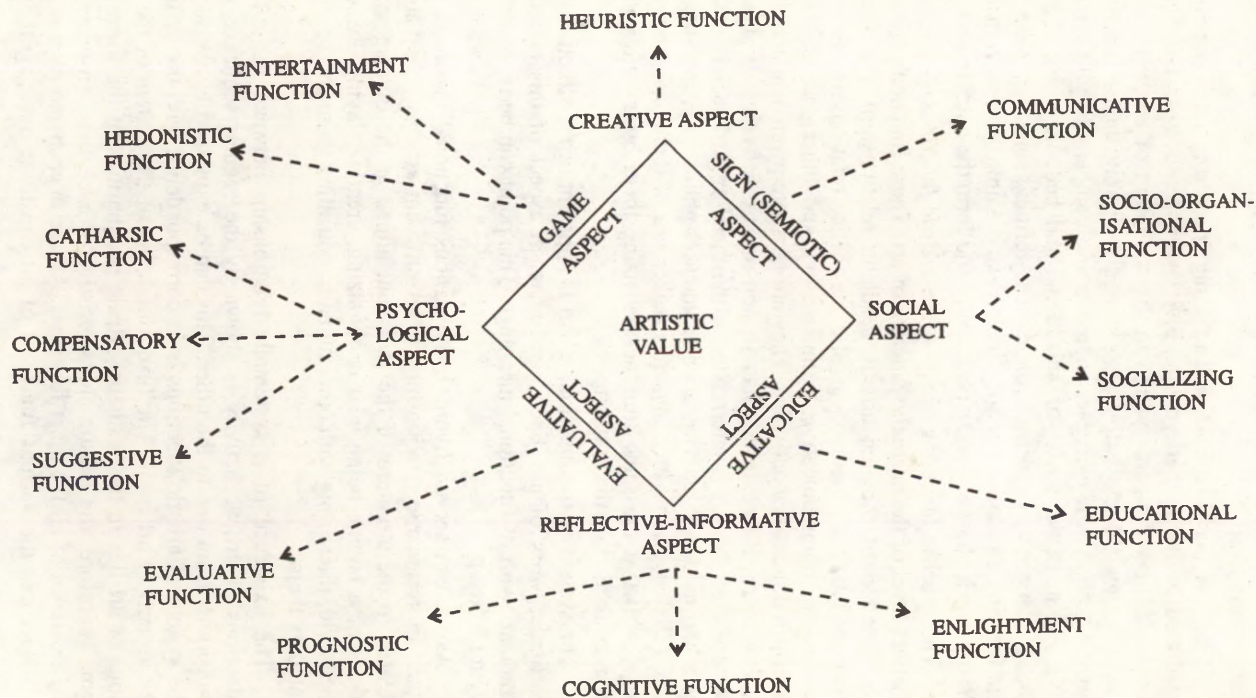
The specific functions of art — the *artistic functions* — constitute a distinct level of the *aesthetic function* of art. These functions differ from the function of the non-artistic aesthetic relationship. The functions characteristic of the non-artistic aesthetic relationship (cognitive, evaluative, communicative, compensatory and other) are deliberately intensified in artistic activities. They are far more prominent and considerably more differentiated than in non-artistic activities. It should be remembered that art is interrelated with other forms of social consciousness and modes of activity, such as science, philosophy, morality, politics, religion, sports, etc., and thus makes use of their functional meanings, whilst at the same time aestheticizing them by transforming the non-aesthetic into the aesthetic.

The system of the functions of art is apparent in the structure of any particular work of art. Each element, each aspect of this structure is "radiant" with its functional meanings. This point can be represented by figure 1 (see p. 91)

As we can see on figure 1, the various functional meanings of art stand in systematic relationships to its different aspects. They are also related to the structure of the recipient, aimed at the diverse spiritual needs of a human being who is cognizing, creating, laying, evaluating the world, interacting with other humans, educating them and being educated by them.

The existence of a systematic relationship between the different functions of artistic activity is shown by the regular sequence in the transitions from one to the other. This can be explained by the fact that the system of artistic functions is in correspondence with the system of the aspects of art itself. The firm foundation of the system of the functions of art lies in the aesthetic-artistic peculiarities of the former system. Therefore, the artistic characteristics of each function presuppose the existence of all the other functions. All are in each and each is in all. The fact that the various functions of art constitute an integral system

Fig. 1



forms the basis for its many-sided and all-embracing influences on human personality, and through the latter on different kinds of social relations and on the development of society.

O'LEARY, MARX AND ASIA

Eero Loone

Brendan O'Leary, *The Asiatic Mode of Production: Oriental Despotism, Historical Materialism and Indian History*. Foreword Ernest Gellner. Oxford: Basil Blackwell, 1989, xvi + 394 pp., £35.00.

Karl Marx's great project was to produce a scientific theory of human salvation. His writings combined penetrating academic studies with highly emotional rhetoric. He had certainly mastered the art of stimulating passionate responses to his own texts. For more than a hundred years, humanity has been infested with obsessive condemnation and obsessive hero-worship of Karl Marx. Both denominations have shown extreme aptitude in twisting the words of the Arch-Demon or of The Holy Teacher. Of course, intellectual or emotional influence does not always stem from greatness. Moreover, being a great thinker does not mean being always right. Aristotle was a great thinker. Nevertheless, there are few Aristotelians today who would unconditionally accept Aristotle's physics or biology. Marx deserves appreciative and critical studies no less than Aristotle, Locke or Machiavelli. Cultural Bolshevism (one party rule within the intellectual community) can be transcended only if we try to avoid prejudging the case of Dr. Karl Marx.

There is a claim that Marx was right about mid-nineteenth century Western Europe but wrong about the world at large. This contention is an over-simplification. Marx might have been a Eurocentrist in the practical meaning of the word, but he was certainly aware of the possibility that his historical sociology might have had only local applicability (e.g., Marx 1881). He tried to ask questions and develop theories about other regions. What we have to ask is (1) are they stimulating valuable research programs? and (2) if they happen to be false empirically, has Marx then been falsified in the Popperian sense? One cannot logically preclude affirmative answers to both (1) and (2). Judgement on both issues belongs to the world of *a posteriori* empirical studies.

Marx designed an economics to justify the transition from capitalism to communism and to introduce his thoughts about communism itself. Alec Nove has (to my mind, successfully) argued that Marx's economic reasoning about the running of the communist society was as utopian and unfeasible as that of his socialist predecessors (Nove 1983). This leaves

the production of the economic case for socialism open to all newcomers. But Marx had also invented the historical sociology. He produced a research program for a new discipline and a paradigmatic example to embody the requirements of this program. Research programs can be fruitful or barren, paradigms can be right or wrong or just confused. A particular theory can be proven wrong, but this does not entail the necessary rejection of its paradigm and research program. If Marx is proved wrong on his particular theories but right on paradigm and research program, then *his* case for socialism would be decisively weakened, although his methods and insights in social research would remain profitable for academic studies. His writings would confirm to the Popperian criteria of scientificity. There would be a Hegelian transcension (negation of negation) of *Marxism*.

A religion can cope with enemies who denounce their prophet as Satan. It starts to encounter real troubles with the secularization of the Holy Fathers into intelligent but fallible human beings. O'Leary argues convincingly that a particular theory proposed by Marx was wrong. He puts forward a good case against the paradigmatic theory, and he is somewhat uncertain about the research program. Marx is pictured as an intelligent human being with many human failings (and this was, undoubtedly, the case).

Marx provides us with an obvious counterexample to the generalization that all German academic writings are dull. In this respect, Brendan O'Leary is comparable to Karl Marx. O'Leary combines passion with academic study, and academic study profits from O'Leary's style of writing. While some of Marx's texts read as if they were written for the *Private Eye*, O'Leary has profited from post-Wittgensteinian analytic philosophy. This has been the best tool produced so far for the purpose of the study of ideas (it becomes vacuous if turned upon itself or upon language divested of extra-linguistic purposes). O'Leary has succeeded in combining admirable clarity of writing with a natural ability to communicate his feelings, thus superbly substantiating that academics are human and ought not to be ashamed of being human and that serious ideas can be discussed in texts intended for humans and not only for highly logical subhuman machines.

A major virtue of the book under review is the introduction of solid scholarship into an area which has been for too long prone to sectarian disputes. O'Leary furnishes an account of the whole corpus of Marx's and Engels's writings relevant to his subject. He does justice to the history of the texts and, thus, allows for the evolution or change of views. He places the texts in their proper context and proper extratextual background. He rejects the idea of absolute coherence of Marx's views as a precondition of study. Any coherence has to be proved by discussion of evidence. Thus, the 1857-58 *Grundrisse* are given their proper place

not proclaimed to be the one and real authentic Marx. All Marx is authentic. Unfinished drafts cannot be assigned precedence over the published writing. Moreover, O'Leary accepts that (a) Engels was an independent writer and, nevertheless, (b) Engels has to be treated as a co-author of Marxism. Any study of Marx (after 1844) has to be a study of Marx and Engels.

O'Leary's argument is that the words 'Asiatic Mode of Production' (AMP) were not a substitute for the term 'Oriental Despotism'. Marx referred to a social order which certainly included a mode of production.

Even where the idea of the AMP overlaps with that of oriental despotism there are significant differences of emphasis and conceptual purpose (O'Leary 1989: 134).

Marx and Engels evidently thought of the AMP as the form of primitive communism, or as a form of primitive communism, or as a transitional order from primitive communism to class-divided societies, or as an independent social order. There have been some shifts in Marx's position between these four interpretations, but textual evidence is available to support all four (O'Leary 1989: 135). This is definitely not a case of absolute coherence. Moreover, Marx was undoubtedly a nineteenth-century writer, but given sources available to him, he was still highly selective and avoided evidence contrary to his own views, for example, in descriptions of Indian society which underlay his concept of the AMP (O'Leary 1989: 262–267).

O'Leary's achievement is not confined to his account of Marx and Engels on the AMP. He provides an interesting explication of the Marxian theory about the linkages between the relations and the forces of production. He shows that it is coherent for Marxists to portray at least all class-divided modes of production as inherently limited in their developmental capacities (O'Leary 1989: 180–181). This is a better exegesis of Marx's theories than that by Jon Elster who has asserted that the difference between capitalism and communism lies in the respective velocities of change in the levels of productive forces (Elster 1987: 258–260, 288–292). Elster assumes that (for a Marxist) the level of productive forces in a given mode of production has no upper boundaries, while O'Leary is right on insisting on those boundaries (O'Leary 1989: 180–181).

There is also a whole chapter on Marx's antecedents. Marx's writings on the AMP were more immediately indebted to the best known political economists of Victorian England than they were to the tradition of political theorizing inaugurated by Aristotle (O'Leary 1989: 81). Marx's most obvious rival on the AMP has been Karl Wittfogel. O'Leary advances a detailed criticism of Wittfogel's substitute for Marx's AMP and argues that Wittfogel combined flawed theoretical contentions with empirical deficiencies (O'Leary 1989: 235–261).

Real troubles for Marx and the Marxists, according to O'Leary, are both empirical and theoretical. Few societies can be identified with the AMP. Pre-conquest India was not a case of the AMP but of feudalism (in the Marxian meaning of the term). O'Leary claims that productivist Marxism is ambiguous and badly operationalizable. Structuralist Marxism provides a highly problematic reading of the concept of the mode of production. Mechanisms through which transitions of the modes of production occur remain obscure in structuralist accounts. Hegelian Marxism can be saved only by turning it into an unfalsifiable (and implausible) set of axioms. Such a turn is contrary to the avowed aim of Marxism to produce a science. The Asiatic Mode of Production seems to deserve its place in the intellectual graveyard of past theories. The King is certainly dead, his Kingdom has disintegrated, and the competing lineages have to face the fact that the throne has been relegated to a provincial museum. But not all is lost for the AMP.

Its periodic exhumation and interrogation prompts important questions about the nature of agrarian societies, and therefore teaches us something about the distinctiveness of our world (O'Leary 1989: 335).

The Orient was, indeed, somewhat different from the Occident and the differences in their respective features might be significant for explanations of the development or absence of capitalism, especially in comparative historical sociology (O'Leary 1989: 234). Marx was wrong but he somehow started a research program which has not yet exhausted itself. There is room for a Parliament without the King. While the majority of O'Leary's criticisms of Marx and the Marxists are brilliant, he overreaches himself in some cases. He opposes multilineal readings of Marx's theory of history on the grounds that they remove necessity from Marx's theory, converting it into redescription rather than explanation (O'Leary 1989: 175). This is a mistaken stance. To abandon unilinealism is to abandon Hegelian necessity. But multilinealism is compatible with causation and a quasi-Hempelism. Any sequence of the modes of production can be treated as an *explanandum*. The problematic of necessity is thus removed to the *explanans*. One encounters no notable difficulties in designing, for example, a formal description of the linkages between the forces and the relations of production compatible with the multilineal sequences of the modes of production (Loone 1992: 197). Multilinealism is incompatible with eschatology, but Marx abandoned eschatology in the latter half of eighteen forties.

In any case, the topic of non-logical necessity is in a need for more sophisticated treatments. Available conceptual means are patently inadequate for the purpose of expressing practically interesting distinctions. Let us consider the following statement:

If A, then either B or C. (A)

What is necessary here is either B or C, although B in "then B" and C in "then C" can be treated as contingent.

Let us now consider the much-beloved example of throwing dice. Let X stand for "I throw a die", and Y stand for "I get a die with either 1 or 2 or 3 or 4 or 5 or 6 up". We write:

If X, then Y. (B)

Is statement (B) expressing non-logical necessity? In the case of throwing dice it is always true, and it cannot be otherwise. In the real world we do not get dice changing into cows while being thrown, although this possibility seems to have been suggested in some discussions about the implications of quantum physics. We get always only one of the six possibilities realized.

Statement (B) assumes there are only and only six possibilities. Theories about the real world are interesting just because they claim that not everything is possible (dice changing suddenly into cows), even if philosophers in the looking glass worlds assume otherwise. Multilineal Marxism can still remain non-vacuous and non-redescriptive and even retain some necessity.

I have used the word 'necessity' up till now in the sense I believe is closer to the traditional usage by Marx than to some modern usage within some communities of professional philosophers. This procedure is justified by the reference to the central subject of our discussion, the thought of Karl Marx. For him, 'notwendig' and 'Notwendigkeit' had certainly extralogical relevance analogous to that of the expression 'laws of nature' (as distinct of 'law statements'). If one accepts that there are laws of nature, then one probably assumes that there is something outside his own thought which can be naively characterized by the expression 'non-logical necessity'. Marx, of course, asserted that there are societal laws in the same sense as there are natural laws, therefore the discussion of necessity and natural laws is applicable to the issue of necessity and (Marxian) societal laws.

In sophisticated treatments, some of the best analytic philosophers have argued that law statements are generalizations and that there is no necessity involved in natural laws (Mellor 1980). It is still reasonable to talk about deterministic and non-deterministic laws, about law statements involving real universals, chances, etc. (Mellor 1990). A Marx-compatible multilinealism is certainly explainable (even Hempel-explainable) by what can be designated *law-statements* by a Mellorian. If the concept of natural law can be explicated without recourse to natu-

ral necessity, then the objections by O'Leary to multilineality lose their force.¹

It is socialism and not *Notwendigkeit* that has some serious troubles with multilineality. If there are postcapitalist alternatives to socialism, then the arguments about there being unavoidable ('necessary') upper limits to development within capitalism are not sufficient to justify the desirability of socialism even for those exploited (in the Marxian sense) under capitalism. It might be interesting to discover how socialism could overcome troubles of this sort, if it is able to do this at all. But this (alongside with all forays into the metaphysics of necessity) lies outside the scope of the book under review.

It is unfortunate that O'Leary does not read either Russian or German. There are some good studies in the latter language about both the AMP and the Marxist discussions about the problem, and Russian-speaking authors have² made many contributions towards solving the issue. The Godelier reading of the superstructure (O'Leary 1989: 12–16) has been based on a late letter by Frederick Engels. The standard Soviet Marxist reading was that of the *A Contribution to the Critique of the Political Economy: Preface* and it was analogous to G. A. Cohen's use of basic Marxist terms. There were certainly more Marxist scholars in the former Soviet Empire than in all Western countries, therefore one is not allowed a claim that the Godelier reading is the standard one. By the way, English translations of even the Preface do not always preserve the actual terms used by Marx and O'Leary's arguments could have sometimes profited from checking with the German original, e.g. (O'Leary 1989: 105). O'Leary has certainly extracted everything present in these translations. Any further profitable study of Marx has from now on to depend on the original texts and transcend the present English tradition of quoting translations on issues of meaning and usage in Marx (or in any non-English texts).

¹ Although, obviously, *Mellor 1990* was not yet available to O'Leary at the time of writing his book, but *Mellor 1980* was already published. Connections between the concepts of natural necessity and natural law were indicated in accessible popular reference books, e.g. *A Dictionary 1983*.

² I do happen to sympathize with anybody claiming there are conceptual difficulties with the notion of the AMP, and have even tried to invent some ideas for the theory of something which could be named AMP but the results are at present available only in Estonian *Loone 1983*: 69–70. Obviously, it is not reasonable to expect neither O'Leary nor 99.9% of the authors dealing with the issue to know all languages. Most of what Marx wrote was in German, and a large part of writings claiming to be Marxist is in Russian, therefore these two languages (or, at least, German) have to belong to the intellectual equipment of anybody engaged in serious academic research about Karl Marx.

There are some flaws in O'Leary's arguments on the incompatibility of Marxist theories of the state and the concept of the AMP. He is right, given his own articulation of the concept of the AMP. This articulation is no more than one of the many possible Marx-interpretations which have been outlined by O'Leary himself. Given that the AMP is a variety of primitive communism, then there were no state and no classes. Given that the description of Indian society was wrong, as claimed by O'Leary (and he is probably right on the issue), there cannot be any problems.

The supposed inability of the AMP to achieve endogenous development, which supposedly provides an argument against Marx and the Marxists is another case of avoidance of some essential questions by O'Leary. A Marxist really needs to accept only that *if* the level of the forces of production surpasses a certain boundary, *then* the relations of production have to be changed. A unilineal theory of history is, indeed, refuted by the inability of the mode of production to reach its upper compatibility boundary between the forces and relations of production. The ascription of unilinealism to Marxism is just O'Leary's pet theory. Writings in Russian since nineteen sixties have intermittently dealt with the issue of inherent inability to develop, with applications primarily to the theory of slave-owning societies. In any case, an author who supports operationalizability should be careful with claims about inherent stagnation. Given two or more entities with different velocities of evolution, the first past the post can impose its solution on the other competitors and produce an appearance of their inherent stagnation even if the difference was no more than a few historical seconds.

Disputes on the AMP might not help us much in understanding why capitalism developed in Western Europe. This is O'Leary's question. Marx's question was, why did capitalism develop at all? There are other questions of legitimate interest to historians and historical sociologists. Did the economic and social system of Minoan Greece differ from that of Classical Greece of the 5th century BC? What are the typological similarities and differences between Maya societies, the Inca Empire, Ancient Egypt and Ancient Mesopotamia? Marx was wrong but discussions about the AMP suggest an exciting comparative research program for historical sociology. I am in full agreement with O'Leary's final verdict about the AMP:

Its periodic exhumation and interrogation prompts important questions about the nature of agrarian societies, and therefore teaches us something about the distinctiveness of our world (O'Leary 1989: 335).

There has been an overkill of Marxism and O'Leary has not quite succeeded in extracting himself from its rhetoric. Although the King is dead and some lineages are tainted by zealots and murderers, it is still reasonably possible that the best one of them — the productivist historical materialism — can be joined in marriage with a solid republican

family. Nevertheless, O'Leary has produced a book of superb scholarship, lucid and well-argued about history and validity of an idea which certainly merits to be studied. The argument about the Asiatic Mode of Production will never be quite the same as it was before O'Leary.

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