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THE CHOICE OF INTERPRETATION AS A PROBLEM OF SOCIAL EPISTEMOLOGY

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Social epistemology is a set of attempts to assign a philosophical meaning to sociological studies of cognitive processes within their social context. These attempts are being made through competition of philosophical interpretations. The choice of interpretation is governed by the desire to bring value orientations of science in line with the challenges from a changing cultural-historical environment. Robert Merton's "normative ethos of scientists" is reviewed here as an illustration to this thesis.

Key words: Social epistemology, sociology of science, philosophy of science, culture, values, ethos, interpretation, truth.
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1. *Social Epistemology: Philosophy or Science?*

A concept asserting that the issues that have been always addressed by philosophy are now set and resolved by special sciences, is rooted so deeply that those doubting it would take the risk of being accused of all but obscurantism. It descends from positivism at least, although nowadays it is no longer burdened by its former ambitions. For example, nobody seems to take seriously today once an attractive slogan "Unified Science" and there is almost no one who believes in the universal "demarcation" between "empirically supported science" and all sorts of "metaphysics." Instead, there is a widespread conception of the "interdisciplinary" character of scientific research in those areas, which, for certain reasons, are still jointly "shared" by scientists and philosophers (e.g. studies of the consciousness and cognition processes, methodology studies, social structure definitions or behavior orientations systems etc.), with philosophy striving to remain a fully-fledged member of this alliance along with special sciences, and seeking, when possible, to retain this "membership." Not claiming for the "leading" and "organizing" role, philosophy should be happy with the fact alone that it is tolerated in this alliance for former merits, perhaps, but it draws attention only when it speaks, with great efforts, in badly learned languages of special sciences, translating, to the best of its ability, its own problems into them. However, these attempts are not lost in vain. For this quality, philosophy is sometimes more or less respectfully called a "cognitive science," "theoretical anthropology," "methodology of science" or whatever. Everyone seems to be happy.

Some see social epistemology as philosophy, others view it as a synonym for the sociology of knowledge, while the third ones tend to regard it as something in-between the former and the latter[3]. Whatever the case, attempts are being made in this rapidly developing research area to employ sociology methods to resolve the traditional problems of cognitive philosophy. How successful are they? As Ilya Kasavin remarked, "the issues addressed by social epistemologists, appear philosophical to the extent to which they prove unsolvable; special tasks can be successfully resolved by historical, sociological, psychological and other methods of the study of knowledge."[4] Let it be so, but one is tempted to ask a question: what's the point in wrestling with unsolvable problems if there are so many opportunities at hand for successful solution of special tasks? Or more directly: what's the value of a philosophical study

[3] See a report discussion*: Е. А. Мамчур*. Еще раз о предмете социальной эпистемологии // Эпистемология & философия науки, 2010, т. XXIV, № 2. P.44-74 (E. A. Mamchur. Eshhe raz o predmete social'noj jepistemologii // Jepistemologija & filosofija nauki, 2010, t. XXIV, № 2. P.44-74).

[4] *Касавин И. Т.* Социальная эпистемология: к истории и постановке проблемы // Социальная эпистемология. Идеи, методы, программы. Под ред. И. Т. Касавина. М., «Канон+», 2010. P. 14. (Kasavin I. T. Social'naja jepistemologija: k istorii i postanovke problemy // Social'naja jepistemologija. Idei, metody, programmy. Pod red. I. T. Kasavina. M., «Kanon+», 2010. P. 14).

as such if it does not bring what could be considered a success, in a simple and clear sense of this word?

Such questions may sound naïve. Who, you say, would measure the value of philosophical thinking with the "common sense"? However, this point is not a position from which one can defend the value of philosophy nowadays. Too fiercely it is attacked today, too vague a role does it play in contemporary culture. Discussions on the status of social epistemology could (and should) be yet another cause to advocate the honor and dignity of philosophy.

2. On the Bridge of Interpretations

Let us say this way: social epistemology is a set of attempts to assign a philosophical meaning to sociological studies of cognitive processes within their *social context*. "Context" – generally speaking - is the key word for the understanding of social epistemology's special nature. It does pretend to the right of contextual studying of cognition and knowledge (with all possible and appropriate "layers", correlations, connotations, etc.). Clearly, contexts are endlessly diverse and changeable, so the results of their studies cannot be interpreted once and for all based on some *a priori* definitions. There are as many definitions as there are contexts; moreover, apriority contradicts contextualism as a conceptual framework of social epistemology. After all, contextual are the knowledge of knowledge, reflection over them etc. *ad infinitum*. It may seem that "contextualism" "reconciles" with the logical circle what appears to discredit this principle. The point, however, is that this is "reconciliation" in the quotation marks. In effect, the laying of the *petitio principia* spirit means that the context is constantly problematized, i.e. *a problem*, ostensibly resolved within one context (and thus moving up to the *tasks* level), is regaining its former status but in a different context.

But if so, the ongoing reassigning of the problem status to the issues addressed by sociology and bound with knowledge and cognition is possible only if it is directed by philosophical sense as horizon of the problematization process (otherwise the process would turn into a monotonous alternation of questions "why" with no direction, a kind of children's "whyer" game, but without joyful and inexplicable fun, so typical of children). Hence, social epistemology is a sort of bridge between the sociology of cognition and philosophy. Who and why needs this bridge?

This bridge, as it seems, is unlikely to become a walking place for sociologists, who have enough problems to research in their own areas, saving themselves for a more successful application, nor for philosophers kindheartedly relying on their self-sufficiency. But even if

both of them, for any reason, would walk towards one another, once stepping on this bridge they will feel how shaky it is. Both bridge piers are very unstable. On the one hand, philosophy is "pluralistic," which means that its cognition-related problems are set so differently that they become mutually exclusive or lose any sense. On the other hand, social scientists cannot refer to "facts" as something philosophically neutral. The selection of information and the methods for obtaining it and, most importantly, generalizing and conceptualizing, depends on ontological and gnosiological prerequisites, no matter how it was challenged by those wishing to think in the "demarcation" mode. These prerequisites vary depending on the philosophical stance the social scientists stick to (explicitly or implicitly).

If this meeting will ever occur indeed, it will either become a ceremonial exchange of niceties or, conversely, boil down to complaints of "mutual pains, troubles and grievances." But it can be the other way around – this meeting will be necessitated by the real problems experienced by both parties and resolvable only through joint efforts. For instance, when sociologists find their surveys and methods yielding contradictory conclusions or they see controversial interpretations of the same cognitive processes (primarily in research areas), whereas philosophers, exhausted by endless disputes, are seeking proof to their conceptions in sociological data. In this case, the bridge is not a stage set, but something really important. I call it "The Bridge of Interpretations" – the term is easy to remember. I wouldn't mind against a different term if something better is suggested.

The Bridge is definitely not intended for walking; rather, it is a "firing ground" for a research strategy – a study of *cognition in context* (first of all, in the cultural-historical one). As the diversity and changeability of contexts are virtually endless, the results of such studies cannot be interpreted once and for all, based on some accepted philosophical definitions. This is the meeting point for two "pluralisms": a range of philosophical cognition concepts and the spectrum of contextual descriptions and interpretations of cognitive situations. Moreover, the meeting itself, as long as also occurs within a certain context, is an event that can receive different philosophical interpretations and acquire different (again contextually dependent) sociological descriptions. These events can be explored through philosophical self-reflection and/or methodological analysis of sociological results. However, these efforts would eventually end in the logic circle or *reduction ad infinitum.* Which of the two evils is better? Both are worse.

3. Robert Merton's "Normative Ethos of Science" as a Socio-Epistemological Conception

How can one get out of the logical trap? To build a connection model between the meeting participants on the Bridge of Interpretations, which would make it possible *to rationalize* this connection, that is, to withdraw its description from the logical circles and endless reductions.

The contours of this model are suggested by an example of interdependence between sociological studies and philosophical interpretations of their results, offered by Robert Merton's conception of the scientific ethos[5]. This conception is well-known, so I see no point in setting it forth. I would like to remind only that that within its framework the principles of the "imperatives of professional behavior" (CUDOS[6]) were formulated, which scientists should follow for science to be successful. These principles, without any special remarks, were reproduced on the pages of philosophical discourses on science, the more especially as Merton himself was not particularly concerned about differences between the sociology of science (one of the founders of which he is justly considered) and the philosophy of science. Perhaps for that reason his conception was often put in one row (or bound in meaning) with Karl Popper's "Big Science" theory, and there was some reason to do so. Both conceptions set forth the fundamental guidelines, which the scientists ought to be governed by in their scientific research; they emphasize their axiological character, as well as the fact that these very principles comprise a set of conditions under which science lives and develops "correctly," that is, accomplishes its goals most effectively. In effect, both thinkers were close to the definition of "transcendental" conditions of scientific cognition, but their obvious (and radical!) difference from the classical transcendentalism was that these conditions are being created by people and are valid only as long as they are accepted by at least the members of scientific elite. However, the difference in their approaches is substantial. For Popper, there is no doubt that the universality of these conditions by itself results from the universality of methodological rules constituting the "falsificationism" doctrine; therefore he saw no need in creating any special theory of scientific ethos, because he believed that the best ethos for a scientist is to follow the rules of a scientific

[5] *Merton R.* The Institutional Imperatives of Science // Sociology of Science. Ed. by B. Barnes, L., Penguin Books, 1972. P. 65-79; his: The Normative Structure of Science // *Merton R.* The Sociology of Science. Chi., Univ. of Chicago Press, 1973. P. 267-268; *Мертон Р*. Социальная теория и социальная структура. М., АСТ, 2006 (Merton R. Social'naja teorija i social'naja struktura. M., AST, 2006).

[6] The abbreviation stands for Communalism, Universalism, Disinterestedness, Organized Skepticism; their contents was analyzed countless number of times (see, for example: *Мирская Е. З.* Этос науки: идеальные регулятивы и повседневные реалии; *Демина Н. В*. Мертоновская концепция этоса науки: в поисках социальной геометрии норм // Этос науки. Под ред. Кияшенко Л. П., Мирской Е. З. М., «Academia», 2008. Р. 123-143, 144-165) (Mirskaja E. Z. Jetos nauki: ideal'nye reguljativy i povsednevnye realii; Demina N. V. Mertonovskaja koncepcija jetosa nauki: v poiskah social'noj geometrii norm // Jetos nauki. Pod red. Kijashhenko L. P., Mirskoj E. Z. M., «Academia», 2008. P. 123-143, 144-165).

method (Imre Lakatos even called Popper's methodology "the moral code of a falsificationist"[7]). But Merton's principles of ethos are based on the *historical* and *sociological analysis of scientific institutions* (in their interaction with social, ideological and religious institutions). He performed his analysis from the standpoint of functionalism and subsequently used it to explain various social dysfunctions, especially those triggered by a gap between culture's goals and the social institutions' ability to ensure their achievement.

If Merton's findings were perceived merely as sociological (rather than philosophical), the philosophers would have only to rejoice or grieve, depending on whether these (scientific!) conclusions correspond to philosophical hypotheses on the character of scientific-cognitive efforts or, conversely, do not agree with them. But Merton's concept was perceived by philosophers in a different way: they immediately saw the philosophical sense in it, having agreed with or challenged it as it was accepted in a pluralistic philosophical environment. This is reminiscent of the case of the *science historian* Thomas S. Kuhn, who was canonized as a philosopher of the 20th century science, and even proclaimed one of its coryphaeuses: some did it to seek rationale for relativism in his conceptions, others wanted to demonstrate through its example the latter's malignancy for scientific rationality, while the third ones translated abstract philosophical reasoning into the language of historico-scientific research.

In reality, the reading of Merton's conception by philosophers was nothing but an *interpretation* (and therefore such readings could be as numerous as there are philosophical views[8]).

For example, a norm of scientific ethos, such as "organized skepticism" could get different interpretations. For Merton, it means an ethically founded willingness of a scientist to question even the fundamentals, let alone the particulars, of his science, if it is suggested by experience and logic. Even if these fundamentals are the opinions of the highest academic authorities, renounce their opinions for the sake of *truth*. The latter term is not borrowed from the dictionary of sociology. It marks the eternal philosophical problem, which is set and dealt

[7] See: *Лакатос И.* Фальсификация и методология научно-исследовательских программ// *Лакатос И.* Избранные произведения по философии и методологии науки. М., «Академический проект», 2008. P. 287 (Lakatos I. Fal'sifikacija i metodologija nauchno-issledovatel'skih programm// Lakatos I. Izbrannye proizvedenija po filosofii i metodologii nauki. M., «Akademicheskij proekt», 2008. P. 287).

[8] Lyudmila Mikeshina dedicated her major works to studying philosophical interpretation as an epistemological issue. She remarks: "In philosophy, we deal with two main objects of interpretation – "things" (real events, the objects of nature and human activity) and texts. Interpretative philosophical knowledge of "things" – "primary" interpretation - is akin to empirical scientific knowledge as it offers an interpretation of specific data and concepts, i.e. "works" in a layer of factual knowledge, facilitating its understanding. "Data" becomes "facts" only as a consequence of interpretation. However, the proximity to the facts cannot guarantee the validity and reliability of interpretation itself, as they are situated at different logic levels; facts are only a material for interpretation, whose nature is determined by "extramaterial" - pre-reflective or reflective methodological attitudes and other principles." (Микешина Л. А. Специфика философской интерпретации // Вопросы философии, 1999, № 11. P. 7-8). See also her article: «Интерпретация как фундаментальная операция познания» (Эпистемология & философия науки, 2008, т. XVIII, № 3. P. 5-13) (.Mikeshina L. A. Specifika filosofskoj interpretacii // Voprosy filosofii, 1999, № 11. P. 7-8). See also her article: «Interpretacija kak fundamental'naja operacija poznanija» (Jepistemologija & filosofija nauki, 2008, t. XVIII, № 3. P. 5-13).

with differently in different philosophies. Hence, the norm of scientific ethos, if it is interpreted philosophically, acquires different meanings.

For example, it can be interpreted as follows: scientists follow this norm, because they believe in the *existence of the truth*, no matter what meaning is put in this term. A scientist's belief in the truth is in no way a theologian's faith, yet both of them have something in common: they believe that the truth is the *goal* of cognition, which exists regardless of approximation to it. Therefore, the success of cognition is measured by whether it brings or doesn't bring closer to this goal or even takes us away us from it. That is why the functionality of institutions, designed for implementing the cognitive processes, depends on what exactly will outbalance: the comfort of scientists' consent with the academic authority or the extent of approximation to the truth, even if this approximation is fraught with problems and troubles.

Slightly deviating from the point, I would say that it is the "classical science," which sought to emancipate rationality from religious dogmas that in a sense may be considered the legitimate successor of theology, having borrowed from it the vitally substantial – the confidence in that cognition is approximating the truth and that this goal is above all the others. Clearly, such an interpretation is also applicable to other Mertonian norms of the scientific ethos.

Not obvious is the following: do such norms suggest an interpretation other than the above-mentioned? For example, the interpretation, whereby it is not faith in the truth but the omnipotence of the critical method that would be proclaimed the conceptual rationale of "organized skepticism" enabling science to get rid of delusions, or belief – the cornerstone of Karl Popper's "critical rationalism" (as we remember, the Popperian conception of the cognitive goals substantially differs from the "classical" definition). Isn't "organized skepticism" compatible with "dogmatism" reigning in the Kuhnian "normal science"? It is true that this dogmatism applies only to the key points of the theory, accepted, along with its methods as a "paradigm," but even the most loyal adherent of this "paradigm" who resolves conventional problems as part of his regular efforts, can well criticize himself or like-minded persons; moreover, all doubts, inevitably emerging in a painful search for solutions of scientific "puzzles," must be attributed to the researcher himself and his ability to take advantage of the opportunities hidden in the "paradigm." But if true, then the "universal" principles of the ethos, as it turns out, can be "adjusted" to certain philosophical interpretations.

4. On the Competition of Philosophical Interpretations

As we can see, on the Bridge of Interpretations is a place where a complicated interaction between the interpreting and the interpreted provisions can occur; a philosophical interpretation

of sociological data can change if not the data itself, then at least data selection and its further generalization, imitating "strictly scientific" conclusions. Hence a question: in what sense is it permissible to speak about such conclusions constituting the "norms" of the scientific ethos? In other words: are these "norms" *set on the basis of sociological studies* or *are they the consequences of a certain philosophical interpretation* of the latter?

Sociologists can certainly attempt to avoid such questions (it is indeed *hard* for a scientist to admit that he *philosophizes* rather than seeks solid arguments). Merton did make such attempts, too. In his early works, he treated as a norm all what he thought as being *useful and reasonable for the performance of science*[9]. But this gives room for different interpretations. There are different norms in science: "models of practical activity, on the one hand, and the examples of their perception as cognitive acts, on the other," "verbal communication norms", "the examples of systematization of the knowledge"[10]. A scientist's ethos is based on the unification and interchange of those norms, to which, of course, one can assign an ethical sense. What can a sociologist of science do by exploring these norms?

He will note that a scientist's methodological actions are normative if they comply with the samples that proved themselves to be guarantors of success. Such norms "regulate the building of different types of theories, observation and the forming of empirical facts"[11]. As a rule, these norms are accepted "automatically": if a scientist conducts research within a scientific tradition, he follows it as something self-evident. To some extent, this is reminiscent of what Ludwik Fleck termed "the style of thinking"[12] and Thomas Kuhn called the adherence to a "paradigm"[13]. For that reason, a sociologist pays attention to the norms not when they are unconditionally fulfilled but when they are infringed, or followed, but not automatically and under the pressure of some social obligations (say, when some social or political institutions impose on scientists their axiological concepts; in this case, resistance to this pressure might become a force protecting the sovereignty of science). Under these circumstances, compliance with or infringement of the norms is due to someone's specific deeds, decisions or choices. This implies that a sociological study of such situations must be specific and fact-based. Can universal "imperatives" of scientist behavior be drawn from it?

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[9] *Мирская Е. З.* Ibid. P. 130.

[10] *Розов М. А.* Нормативная структура науки // Этос науки. Ibid. P. 197-198 (Rozov M. A. Normativnaja struktura nauki // Jetos nauki. Ibid. P. 197-198).

[11] *Степин В. С., Горохов В. Г., Розов М. А.* Философия науки и техники. Гл. III. Структура и динамика научного знания // М., «Гардарика», 1996 (Stepin V. S., Gorohov V. G., Rozov M. A. Filosofija nauki i tehniki. Gl. III. Struktura i dinamika nauchnogo znanija // M., «Gardarika», 1996).

[12] See: *Флек Л.* Возникновение и развитие научного факта. Введение в теорию стиля мышления и мыслительного коллектива. М., «Дом интеллектуальной книги», 1999 (Flek L. Vozniknovenie i razvitie nauchnogo fakta. Vvedenie v teoriju stilja myshlenija i myslitel'nogo kollektiva. M., «Dom intellektual'noj knigi», 1999).

[13] See: *Kuhn T.* The Function of Dogma in Scientific Research // Scientific Change (ed. by A. Crombie), L., Heinemann, 1963. P. 347-369; Second Thoughts on Paradigms // The Structure of Scientific Theories (ed. by F. Suppe), Urbana, University of Illinois Press. 1974. P. 459-482.

For example, if a driving force for a scientist is getting a good compensation for his work (e.g. an invention), he will stop at nothing to achieve his goal without feeling any moral scruples, and his chances for success can be higher than with an "unscrupulous" truth guardian having a critical eye towards intermediate results of his research and therefore often missing priorities and losing competition battles. Clearly, the very concept of success is interpreted differently, and these differences depend on what philosophy is chosen to interpret it.

The normative ethos of science, according to Merton, is, in fact, the consequence of some "ideal science" conception, standing behind the scenes of his sociology, which "filtered off" sociologically meaningful facts.

We have to admit, however, that his concept is *an interpretational scheme of sociological facts*, and therefore it ought to be attributed to social epistemology. This means that its criticism (explicit or implicit) on the part of alternative, e.g. "historical," interpretations of the evolution of science (Thomas Kuhn, Stephen Toulmin and Joseph Agassi, Paul Feyerabend and others) is a dispute between philosophical theories, rather than attempts to empirically prove or refute it. Clearly, the same can be said in relation to some similarities between Merton's characteristics of science and Popper's "code of scientific honor" [14]. In Kuhn's conception, a "normal" scientist who follows his moral obligations, suppresses his skepticism, doing his best to demonstrate the omnipotence of the "paradigm" (and consequently the correctness of prevailing opinions) in solving puzzles. But Popper saw in it *moral deformity* that might have deserved indulgence, but only on the assumption that a scientist is "poorly trained" or a "victim of indoctrination"[15]. Moreover, he called such deformity a great danger "for science, and possibly for our civilization," [16] meaning, of course, the relationship between "critical rationalism" and democracy, which he believed was being destroyed by the dogmatism of Kuhn's "normal science" and its reverse side, relativism.

All of this, I repeat, is a competition of philosophical interpretations of sociological, historical, and socio-psychological data. The dispute is held on the rocking Bridge, which, due to debaters' efforts, is rocking even stronger. Not being able to endure the toss, some of the dispute participants would escape from the Bridge to the shore, where scholars (sociologists, psychologists, historians etc.) are gathering data concerning science and scientists' behavior, borrow this data from the scholars (or take part in data collection) and then, going back to the Bridge and referring to this data, include some other interpretational scheme in the dispute. Such

[14] See: *Лакатос И.* Фальсификация и методология научно-исследовательских программ // *Лакатос И.* Избранные произведения по философии и методологии науки. М., «Академический проект», 2008. P. 287 (Lakatos I. Fal'sifikacija i metodologija nauchno-issledovatel'skih programm // Lakatos I. Izbrannye proizvedenija po filosofii i metodologii nauki. M., «Akademicheskij proekt», 2008. P. 287).

[15] *Поппер К.* Нормальная наука и опасности, связанные с ней // *Кун Т.* Структура научных революций. М., АСТ, 2001. P. 529, 530 (Popper K. Normal'naja nauka i opasnosti, svjazannye s nej // Kun T. Struktura nauchnyh revoljucij. M., AST, 2001. P. 529, 530).

[16] *Ibid.*

escapes and exercises would not take place if there were no important reasons for that. The clarification of these reasons is the way to rationalize this process.

The key reason possibly lies in what Imre Lakatos called degenerative research programs, that is when any of such research programs receives is too fiercely criticized, when it self-justifies rather than explains facts, and therefore is forced to lose to opponents. But Lakatos implied by that the program's inability for such self-development that would enhance its "empirical content", i.e. the volume and range of explainable and predictable scientific facts, by a higher speed rate and a more distant prospective than those available to its competitors. However, both the positive growth and degeneration of scientific programs can be understood in a broader sense than Lakatos's. For instance, the trust in research programs (and more generally in science as such) can grow and fall depending on how its work is being axiologically interpreted. In other words, it depends on whether its value orientations can meet the challenges a cultural-historical environment makes in relation to science.

For example, searching for the truth as the major and absolute value of cognition is typical for the historical stage of development of the European culture at which science was inspired by the idea to read and understand the Book of Nature, written by the Creator in the language of mathematics, in Galileo's words. However, in the twentieth century, a theological "background" of this value substantially "thinned" if not vanished forever. In the changed cultural environment, Kuhn's criticism of archaic understanding of science as "an enterprise that is constantly coming closer and closer to some goal, pre-established by nature"[17] (let alone the Creator, of course, for it would simply make no sense within Kuhn's conceptual framework), is perceived fairly positively. Moreover, the very concept of the "scientific truth" is moving from the set of values that once constituted the "hard core" of the cultural universals system to auxiliary concepts of the philosophy of science, the very use of which requires linguo-analytical and logical constraints. In any case, it does not apply to those concepts without which one cannot determine "scientific progress" or justify "scientific ontology." Kuhn himself explained this transition by methodological arguments, referring to the famous thesis of "theoretical loading" of facts and the "incommensurability" of scientific theories that it entails (or rather, the "radical shift" of ontological assumptions that follows the change of "paradigms" in scientific research). It is clear, however, that the admissibility of these arguments directly depends on the change of demands that culture addresses to science, as well as the latter's change in value orientations. Nowadays, the progress of scientific knowledge can be understood without appealing to the cultural value of the "truth" and be expressed in the terms, such as "prediction accuracy," "a growing number of issues resolved by new theories compared to their predecessors" etc. It

[17] Ibid. P. 266.

follows then that scientific success can be expressed in these terms as well. Which cannot but affect the mindsets of scientists and philosophers, responding to changes in its socio-cultural status. This, in turn, entails change in the comprehension of conditions of "successful action" in science. That is one of the key issues addressed by social epistemology.

If so, it can also formulate the rules of "successful science development" that do not necessarily match the norms of the Mertonian scientific ethos. This means that one would have to choose which "norms" or "rules" can or cannot be considered conditions of successful scientific and cognitive activity. But which criterion or criteria to take in making this choice? Scientists' and philosophers' mindsets?

The affirmative answer to this question would seem strange to some people. But there is nothing strange about it. It only means that a scientific (sociological, but not only) study of the same scientific processes occurs within a cultural context and bears its influence. What's more, the norms and rules of scientific success are formulated in this study's conjunction with value and philosophical attitudes that help to interpret its results.

5. *The Cultural Role of Myths on Science*

If we accept the fact that the choice of interpretations depends on the challenges that the cultural environment addresses to science, as well as on how the interpreters evaluate these challenges, we shall have to agree with the fact that these normative conceptions of science can be called *myths on science*, whose purpose is to maintain stability of science's cultural role and protect the system of cultural universals from undesirable shocks or even collapse, which serious transformation of implications of these universals are fraught with. This term may irritate those who take the word "myth" as something contradicting the very nature of science and which science itself is supposed to overcome. But wouldn't it be it better to ponder quietly over what culture experts, psychologists and even science philosophers call the "culture-forming role of the myth"? Or at least over the fact that this role is performed by different myths in different ways in specific historical-cultural periods, and that this role hasn't exhausted itself until present. Science and myth is an issue that cannot be resolved through flat oppositions in the spirit of naive "progressism".

The distinguishing feature of such myths (I allow myself to use this word, but if it shocks anyone, I beg you to take it... at least as a metaphor!) is that they are "dressed up in scientific togas," that is duly formalized in compliance with scientific practices, but are distinguished by "engagement" and dependence on philosophically defined goals. That is why socio-epistemological conceptions are selective in relation to the interpreted facts (to the material

providing them with a complex of scientific disciplines on cognitive processes). They are often criticized for this, but this criticism – in light of what has been said above – is pointless. For example, if certain facts do not fit themselves in Merton's schemes, it is easier to label them as "pathology" or "signs of dysfunction". As a matter of fact, Merton used to do it himself. When there were too many "dysfunctions" or they played too conspicuous a role, he had to declare scientists' behavior "ambivalent," that is, oscillating between the norms and deviations from them. For Merton, however, the success of science is guaranteed by the fact that the scientists finally find some *middle way* thus, that their actions can favor the "normal" development of science. [18] This allowed him to retain adherence to the "Big science" myth as a value, which he could not and did not want to part with, although he was making some amendments and additions into it. Some of these myths are no less famous today than the concept of the "normative ethos of scientists", such as "the Matthew effect," which is classical in cognitive sociology: a respected scholar and a rank-and-file researcher are unequally credited for equally significant work results (be it honors and scientific community recognition or various benefits)[19]. Clearly, "the Matthew effect" hardly complies with the "disinterestedness" norm (unless we assume that the respected scholar granted with honors and money reluctantly agrees to accept them out of sense of modesty or shyness). Well, what of that? One can always say that "community recognition can serve a scientist's personal interest in receiving moral and material benefits, but it can be achieved only through meticulous compliance with the scientific ethos norms."[20] The facts contradicting it should be either ignored (the myth not sensitive to the facts!), or we should again resort to the concept of "ambivalence," which clarifies little, yet is quite consonant with the myth.

All goes well as long as the myth "works," that is, meets certain cultural challenges. But the challenges can change, and the time comes for new myths to emerge. In the last quarter of the twentieth century, the Mertonian conception was vividly criticized and "improved" to make it get "closer to reality" (or was even rejected outright)[21]. It was stated that the Mertonian model of scientists' behavior, which ostensibly always ensured best performance of science, was erroneous "universalization" of some of its specific features that existed only at certain phases of science development. In addition, "it describes a static set of norms and does not take into account variable premises that can affect the productivity of intellectual processes"; the changed historical conditions in which science performs is better described by Griffith's and Mullins'

[18]See: *Merton R.* The Ambivalence of Scientists // Science and Society (ed. by N. Kaplan). Chicago, Rand McNally. 1965.
[19] *Merton R.* The Matthew Effect in Science // Science, 1968, Vol. 159 , p. 56-63; The Matthew Effect in Science, II. Cumulative Advantage and the Symbolism of Intellectual Property // ISIS, 1988, vol. 79 , p. 606-623.
[20] *Коннов В.И.* Принципы научного самоуправления в современной социологии науки // Философские науки. 2007, № 4. P. 3 (Konnov V.I. Principy nauchnogo samoupravlenija v sovremennoj sociologii nauki // Filosofskie nauki. 2007, № 4. P. 3).
[21] See: *Barnes S., Dolby R.* The Scientific Ethos: a Deviant Viewpount // Archives Européennes de Sociologie. P., 1970, vol. 11 № 1. P. 13.

"model of theoretical groups"[22] which could have been "integrated into a more general picture of scientific innovation, if it included the structure of the competing theoretical groups and a long succession of stages through which they pass"[23]. These remarks seemingly concern Merton's methods of sociological studies, rather than their philosophical interpretation. But if you look closer, you will see behind them a dispute between different epistemologies.

Let us take, for example, Ian Mitroff's critique of Merton's conception. In the 1970s, he published the results of an inquiry into the psychology of the Apollo Moon scientists, suggesting, as he believed, that the ethos of a successful research team is in direct opposition to what Merton believed to be inherent in science as such ("particularism" instead of "universalism," "greed" instead of disinteredness," "organized dogmatism" instead of "organized skepticism" etc.) [24] It seems unlikely that over the thirty years that passed after Merton's publication of his works, the scientists' ethos norms have changed for completely the opposite ones and that a more meticulous (or, *horribile dictu*, a more honest!) sociological analysis has revealed such changes. Rather, Mitroff offers a different philosophical interpretation of sociological data (clearly affecting its selection) than does Merton. It is no coincidence that these very years saw changes in the theories of science that can be traced in philosophical disputes of the 1960s – 1970s, in which the voices of the critics of the abstract-rationalistic ideal of science (Feyerabend etc.) sounded very loudly. Can we speak seriously of normativity, if we can easily replace norms with "antinorms," in other words, show the woeful symmetry of positive and negative modes of scientist conduct? Steve Fuller argues that the Kuhnian "normal science was a politically primitive social formation that combined qualities of the Mafia, a royal dynasty and a religious order"[25], that the scientists at all times were double-dealers and hypocrites, that they accompanied their appeals for intellectual freedom with complete irresponsibility, when it came to social consequences of their activities, flattered authorities with the expectation to get approval for publication, and so on[26]. It appears that it's high time to forget about the normative ethos as a factor of the functional success of science, when science at large or some of its branches have found themselves in the times of crisis, as it happened in Russia after the system of imperial patronage collapsed, the financing shrank, and science helplessly faced wild market with all that that entails for it and the national culture.

Paradoxically, given this general functional crisis of science, scientists' behavioral norms remain a more or less stable condition for survival and even inspire hopes that Russian science

[22] *Griffith B., Mullins N.* Coherent social groups in scientific change // Science, 1972, vol. 77. P. 959-964.
[23] *Коллинз Р., Рестиво С.* Пираты и политики в математике // Отечественные записки, 2002, № 7 // Интернет-ресурс: www.strana-oz.ru/numbers/2002_07/ 2002_07_42.html (Kollinz R., Restivo S. Piraty i politiki v matematike // Otechestvennye zapiski, 2002, № 7 // Internet-resurs: www.strana-oz.ru/numbers/2002_07/ 2002_07_42.html).
[24] *Mitroff I.* The subjective side of science. A psychological inquiry into the psychology of the Appolo Moon scientists. Amsterdam, 1974.
[25] *Fuller S.* Kuhn vs. Popper. Revolutions in Science. Cambr., Icon Books, 2003. P. 46.
[26] See.: *Fuller S.* Science. Buckingham, Open Uni Press and Uni of Minnesota Press. 1997

would emerge from coma (providing, of course, that "innovative strategies of the national development" would not become a propagandistic trick of the authorities, but get a real prospective). It is likely that the crisis would prove a crucial test for the scientists' "selflessness" as the ethos principle, and irony is hardly permissible in this case. It makes no point in referring to the statistics (clearly, facts are not difficult to select so that Merton's normative ethos becomes a helpless target; but there does exist a chance that the cases (and not rare ones) of scientists complying with these norms – even though it looks impossible – inspire hopes that Russian science will have a future it deserves.).

Myths on science, devoid of normative guidelines - the science, perhaps even more immoral than a society in which it exists - that replaced the myth on science as a model of moral and democratic society, were in demand when the veil of naive optimism had to be taken off the concepts of science. However, time moves along, and culture needs new myths, in some way reminiscent of the old ones but different at the same time.

The above, in my view, demonstrates the cultural role of myths on science. "Myths on science are useful illusions without which its morality could not exist."[27] Authors of the cited work suggest that scientific ethos norms be viewed as some model guidelines rather than "imperatives," whose aim is to diminish potential evil resulting from the deviation of these norms. There is a substantial gap between the Mertonian norms and the scientists' real behavior; the recognition of this fact helps to protect oneself from ethical rigorism and ethical relativism. Although this does not comply with the models of science, based on the "absolutist" conception of scientific rationality[28], it corresponds to the "functional" methodology of scientific research (including sociological and psychological studies).

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On the ground of social epistemology there emerge opportunities for the rationalization of what is often taken out of context of rational explanations, namely, the influence of cultural environment and particularly culturally meaningful values, on the development of the corps of the "knowledge of knowledge." This, I believe, is a sound argument in favor of assigning a philosophical status to this research area.

[27] *Аллахвердян А. Г., Мошкова Г. Ю., Юревич А. В., Ярошевский М. Г.* Психология науки. М., 1998. P. 290 (Allahverdjan A. G., Moshkova G. Ju., Jurevich A. V., Jaroshevskij M. G. Psihologija nauki. M., 1998. P. 290).
[28] See: *Порус В. Н.* Парадоксальная рациональность // Рациональность на перепутье. Книга 1. М., РОССПЭН, 1999. P. 338-365 (Porus V. N. Paradoksal'naja racional'nost' // Racional'nost' na pereput'e. Kniga 1. M., ROSSPJeN, 1999. P. 338-365).

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