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Ethical Nodes in the Philosophy of Science

Shevtsov S*
Odessa I.I.Mechnikov National University, Ukraine

***Corresponding author:** Sergii Shevtsov, Odessa I.I.Mechnikov National University, Ukraine, Email: sergiishevtsov@gmail.com

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Abstract

Ethical issues are becoming increasingly central to contemporary philosophy of science. This is evidence of a change in the nature and forms of activity of scientific communities. This also shows the presence of a certain crisis in science itself, because its own methods are not enough to solve the growing problems. The article offers an analysis of some of the problems for which scientists turn to deontological ethics: the reliability of the results of scientific research, the influence of values on ongoing research, the responsibility of the scientist.

Keywords: Philosophy of science; Scientific knowledge; Ethics; Reliability; Value

In this article, I intend to consider the presence of ethical arguments in modern philosophy of science and science itself. The philosophy of science arose in its modern form in the 1920s and is largely connected with the range of problems raised by the Vienna Circle [1].[1] At this stage, scientific knowledge was considered mainly as unitary, objective, and bound by a single scientific methodology (later it was formalized in R. Merton's four institutional norms of science: universalism, communism, disinterestedness, organized skepticism) [2].[2] The criterion of scientific knowledge was its verifiability, somewhat later K. Popper also proposed the criterion of falsifiability. With this approach, there was no place for ethics within the framework of science. Most logical positivists expressed doubts about the possibility of justifying any ethical system or even individual ethical principles, although they recognized their importance for life outside of science and philosophy. A new stage in the development of the philosophy of science occurred in the 60s of the 20th century, it is associated primarily with the appearance of new works by K. Popper and T. Kuhn. As a result, two positions were gradually formed, which continue to dominate the philosophy of science today (of course, with very significant modifications).

In the last decade of the last century, a situation of so called "scientific wars" arose between the supporters of these approaches (we will notionally designate them as essentialists and constructivists), [3-5][3] in the center of which were questions of the applicability of the concepts of "meaning", "aim" and "purpose" to scientific knowledge, the question of the legitimacy and consequences of science [6].[4] The catalyst for the "wars" was Paul Gross's and Norman Levitt's book *Higher Superstition: The Academic Left and Its Quarrels with Science* (1994), in which the authors accused critics of the traditional understanding of science (that is, as a universal, objective and disinterested pursuit of truth) of misunderstanding the essence of science. The opponents of this position (constructivists) really denied science some kind of hidden essence, and saw in its basis not norms, but scientific practices. They argued that science acquires these qualities through the implementation of certain scientific procedures, and these procedures have social, cultural and

1 Pfeifer, Sarkar (2006).

2 Sismondo (2010), 23–24.

3 Segerstråle (2000); Brown (2001); Leane (2007), 61–80.

4 Helsing (2019), 102.

ethical dimensions. In this article, I would like to show that ethical problems (I will designate them as ethical nodes that require to be untied), as well as problems of values, appear as an integral and even connecting part of the purely methodological and epistemological problems of modern science.

In the modern philosophy of science, it is still possible to distinguish two directions in understanding the process of scientific knowledge, which can be designated as the search for truth through trial and error (goes back to the concept of science by K. Popper) and as a social phenomenon that serves the needs and requirements of society (in based on T. Kuhn's *The Structure of Scientific Revolutions*). There is no doubt that science combines both, if only because the possession of knowledge of the true state of affairs (let's imagine that it is possible) will give its owner – state, corporation, scientific school – a serious advantage over competitors. But a theoretical model of understanding science is not able to combine these basic provisions into a single complex and is forced to be built on one of these provisions. T. Kuhn, considering science as an evolutionary process, as you know, persistently pursues and substantiates the idea of the illegality of the conclusion about the orientation of science to something (primarily to the truth) [7].[5]

Over the past thirty years, the philosophy of science has identified a number of issues that can be summarized as the question of the prevailing foundations of modern scientific knowledge. It is extremely difficult in the conditions of the social basis of the activities of scientific institutions identified by Thomas Kuhn to present a methodology that turned out to be able to eliminate the social dimension from the practice of scientific research. The view of Karl Popper, according to which science is an approximation to the truth through attempts, errors and their correction, increasingly requires correction for the current state of science, in which the struggle is not only and not so much for testing or supporting this or that theory, but for obtaining funding, scope of a project, implementation of certain results. The problem of the foundations of scientific activity and their influence, therefore, can be defined as the question of the basic positions and methods of research searching of solutions. This problem can be solved only by identifying a methodology that can provide reliable knowledge, but it is obvious that such a solution should be recognized, if not entirely utopian, then extremely difficult to achieve. Nevertheless, the efforts of philosophers and theorists of scientific knowledge are focused on two areas that are organically inscribed in the general problem of searching for the nature of scientific knowledge: firstly, this is the state of research ethics and, secondly, this is the inconsistency of expert assessment. In the following, I will illustrate this state of philosophy of science by outlining some of the most influential trends and analyzing proposed solutions to overcome the identified problems.

Heather Douglas (then a professor at the University of Tennessee) critically examined the ideal of value-freedom (the search for truth as the highest goal) [8].[6] She gave reasons to demonstrate that the search for truth cannot be sufficient guidance for scientific decision-making, despite frequent declarations of this. Douglas stressed that the numerous methodological decisions required to conduct a single research are not determined by the actual circumstances of the situation and should be based on an assessment of the consequences of being wrong. In this statement, she drew on her earlier study of inductive risk [9].[7] Scientists should take steps to mitigate the impact of inappropriate values in order to protect science from their harmful effects. It is necessary for this, firstly, to distinguish between the direct and indirect roles of values, and secondly, to clearly articulate guidelines for individual scientists. According to Douglas, the direct role of values is their strict provide of the decision to accept or reject a theory; the indirect role is to assess the consequences of accepting or rejecting a theory, that is, influencing what will be considered sufficient evidence for acceptance or rejection of a theory. Douglas concludes that it is the duty of scientists to ensure that values do not play a direct role in their work and to be transparent about the indirect roles of values. This decision of the American researcher can be regarded as typical to the so-called decision theoretic approach. This view has received many criticisms from various quarters. Most of all, scientists expressed doubts about the very distribution of the direct and indirect role of values, and in addition, its formulation and validity were also disputed.

For example, with respect to the first objection, Kevin Elliot has shown that some issues of social benefit can shift from a direct role of values to an indirect one depending on considerations of scale and time [10].[8] Daniel Steele and Kylie Whyte in their article substantiated, based on the analysis of scientific activity in pharmaceuticals, that the same decision can be motivated by different values – according to Douglas, both direct and indirect [11].[9] Thorsten Wilholt [Torsten Wilholt 2013] approached this

5 As Wenda K. Bauchspies, Jennifer Croissant, and Sal Restivo states in introduction to *Science, Technology, and Society: A Sociological Approach*: "What we do deny is the idea that there is an already and always existing description of reality that we approach through closer and closer approximations". Bauchspies, Wenda, Croissant, and Restivo (2006), viii.

6 Douglas (2009).

7 Douglas (2000)

8 Elliott (2011).

9 Steel, Whyte (2012).

problem from a completely different angle, and opposed the decision-theoretic approach as a whole, since the latter, in his opinion, is capable of considering a complex research situation solely as a compromise between epistemological and non-epistemological factors. He argues that the question of credibility in the results of the study should be raised not only through the prism of the reliability of the results, but taking into account the values that guided scientists. In his opinion, the decision is always a compromise between the reliability of positive results, the reliability of negative results, and the power of the study. In addition, the question of trust is significantly determined by the assessment of the consequences of possible results (and their implementation), which should also be taken into account. There must be a unity of values, moral and epistemological norms here.

First of all, I would like to draw your attention to the nature of the steps proposed by H. Douglas to overcome the identified problem. In fact, its solution remains within the framework of deontological ethics (ethics of duty, normative ethics), which is a weak argument and a dubious means to overcome crisis phenomena anywhere. I quoted above the objections of the opponents of her approach to show that they do not consider this aspect or that I am not aware of such comments. Normative guidelines for what scientists who conduct scientific research should do are useful and even necessary to a certain extent, but it seems at least naive to hope to change the situation of the crisis in science by introducing some guidelines for research and researchers. This situation in science has developed as a result of a certain state of affairs, therefore, its causes should be established and strive to change them; not everything can be fixed by imposing ethical restrictions.

Research ethics and the problem of trust imply their consideration through the issues of integration and differentiation, since the very concept of trust implies an attempt to achieve integration in a situation of differentiation. Attempts to find a measure of integration and differentiation for scientists who work within the same project can be considered the main subject of discussion in modern philosophy of science. Another thing is that the idea of structures above organisms (population, biogeocenosis, biosphere) requires raising the question of other, much more complex forms of communication, in which the whole cannot be considered as a simple sum of constituent elements, and the elements themselves as simple constituents. This situation is unfolding more and more, embracing even those sciences that were considered exemplary in terms of the form of evidence and the severity of the development of provisions and arguments. The English mathematician Brian Davis, in an article in 2005, examining the trends of the current state of affairs at that time, made rather sad conclusions: « Pure mathematics will remain more reliable than most other forms of knowledge, but its claim to a unique status will no longer be sustainable. It will be seen as the creation of finite human beings, liable to error in the same way as all other activities in which we indulge. Just as in engineering, mathematicians will have to declare their degree of confidence that certain results are reliable, rather than being able to declare flatly that the proofs are correct. (…) Formal verifications of complex proofs will be commonplace, but there will also be many results whose acceptance will owe as much to social consensus as to rigorous proof» [12].[10]

Brian Davis does not give recommendations, his conclusions do not go beyond the emerging processes and trends and are limited solely to mathematics. At the same time, his view is extremely important from the point of view of understanding the transformations in modern scientific knowledge. He notes the growth of “subjective” and “collective” factors in the branch of science, which for a long time was a model of objective and accurate scientific knowledge of the world. In addition, the fact that mathematicians are increasingly forced to work collaboratively to solve scientific problems means that they are becoming increasingly aware of the common “ethical” problems of scientific communities - trust, interaction, the influence of values, and cultural institutions (prejudices?).

The problem of trust or public consent of scientists gives rise to the problem of understanding the role of the two most common methods of proving the reliability of research results – peer review and replication. This is a kind of creation of integration at a higher level, but the influence of the ethical and ideological aspects of the problem is especially noticeable here. Karol Lee, Cassidy Sugimoto, Guo Chang, and Blaise Cronin documented the presence in modern science of forms of peer review bias, language bias, gender bias, nationality bias, prestige bias, and content bias [13].[11] The conclusions and solutions offered by Lee and her colleagues seem to me very revealing. They see a way out of this situation in increasing the requirements for authors on the part of editors of scientific journals. We are again entering the realm of deontological ethics. The authors of the article do not provide a rationale for why editors of scientific journals should do this. They see the only solution in changing the nature of the reviews and impose the obligation to change it on those who have the power of regulation. There are doubts to what extent such regulatory measures can be implemented, and to what extent their implementation can change the situation. Peer review and replication are the most important tools of the scientific community, not only in finding ways to solve scientific problems, but also to counter

10 Davies (2005).

11 Lee, Sugimoto, Zhang, and Cronin (2013).

fraud and scientific dishonesty. It is hardly correct or efficient to entrust this task as a normative duty to scientific editors alone.

It cannot be said that philosophers are not looking for ways and means to overcome the negative phenomena in the field of reviewing and replication, but they approach this problem mainly from other angles. For example, John Ioannidis, professor of statistics at the Stanford School of Arts and Sciences, one of the most frequently cited authors today, along with his collaborators carefully studied during his work at Taft, and then an internship at Harvard the nature of publications regarding replications and checks of already conducted studies (publications), both for fraudulent studies and dissemination of effective results. He saw this as a question of the effectiveness of reward and sanction structures. In his opinion, it is possible to stop or reduce the number of unscrupulous publications only if the authors are confident that their results will be verified by other researchers. The group led by this researcher was able to demonstrate how rarely attempts are really made to repeat studies and how conflicting and contradictory results persist in the scientific literature for decades [14,15].[12] He saw this as a problem of the gap between the ideal of replication, which gives confirmation, modification, or refutation, and reality. In his opinion, it should be noted that the solution of this problem should be entrusted to philosophers. We again see here the introduction of an ethical component into the situation of the activity of the scientific community and the proposal for a solution to correct it by means of deontological ethics. Many other researchers have also addressed the issue of lack of or insufficient replication of studies; it once caused a discussion in the field of psychology and biomedical research, in which it was considered the cause of the scientific crisis in these areas of science [16,17].[13] I am not aware of proposals for its correction that do not lie within normative or deontological ethics.

As a preliminary conclusion, it can be noted that although Karl Popper's view of science still retains its value, it is increasingly revealing its limitations: it focuses exclusively on the technical side, and is unable to determine the essence of science as a social phenomenon, as well as to reveal the main directions of both philosophy of science, and the way of modern scientific knowledge. As for the theory of Thomas Kuhn, here the question should be put differently: the theory of the American philosopher itself is predominantly the subject of research in the history of the philosophy of science, but at the same time it provides methods and a theoretical apparatus for the modern stage of the philosophy of science, it is thanks to it that philosophers of science today highlight the painful points of the processes of scientific research. The approach proposed by T. Kuhn allows in most cases to identify those problems that become the subject of close attention of theorists. To a large extent, these problems involve a complex interweaving of processes of integration and differentiation, and perhaps the problem of the so-called "Big Science" is the most revealing and acute in this sense.

The concept of "Big Science" was probably introduced by John Hartwig to denote the organization of a large number of scientists, bringing together various expert groups into a single research project [18].[14] The classic model for this type of research is the famous Manhattan Project; since the second half of the 20th century, this model of research by large groups of scientists has become increasingly widespread in various fields of science. Of course, modern researches of this type in other areas, such as the Human Genome Project, have gone quite far from the original model. There has been a gradual shift from small university or amateur research groups and laboratories to institutionalized research supported by national funding bodies and often extending beyond the borders of one country. It is important for us to note that work in such projects necessarily required special economic conditions and new moral thinking from its participants. Hartwig was the first to draw attention to this and formulated the philosophical dilemma that confronts the staff of such projects: each member or subgroup has a certain part of the experience necessary for the project as a whole, because each has a crucial bit of expertise not possessed by any other, but there can be no one in the project who would have experienced every element and every detail of such a project. Due to the absence of anyone who would have knowledge of all the elements and sections of the project, the relationship between its employees must be based on trust. In the 21st century, the advantage of Big Science has become more obvious, as a result of which the number of such projects has increased significantly. Due to this state of affairs, the issues of research thinking and ethics, interactions between different groups have become much sharper and have become extremely relevant.

Hardwig singled out two main issues of the problem: the status of the evidence of the testimony and the nature of the knowing subject. The researcher noted that these questions are extremely complex, and he failed to offer an answer to them that could be considered decisive. The first question, according to Hardwig, should be considered within the framework of a more general discussion of the epistemological value of testimony. Most of the knowledge that is considered common knowledge comes to us from

12 Tatsioni, Bonitsis, and Ioannidis (2007); Young, Ioannidis, Al-Ubaydli (2008).

13 Loken, Gelman (2017); Redish, Kummerfeld, Morris, Love (2018).

14 Hardwig (1985).

others [18].[15] This is the case not only in research projects, but also in everyday life. Much of what we learn depends on what we learn from our parents and teachers as children. This state of affairs is being reinforced in the future. We rely on experts to determine how accurately our instruments, our cars, even our bodies work. We acquire knowledge about the world through educational institutions, scientific and popular publications, and our own scientific research. The doubts of philosophers, who usually disagree about the status of beliefs thus acquired, do not change the situation as a whole. In the end, it turns out that we don't know much of what we think we know. The second question is even more difficult. We simply have no other model for talking about a group or community holding knowledge as a whole than models of superorganisms and transcendental entities. But Hardwig considers such models unacceptable. After him, other philosophers turned to the consideration of these issues, but they, like him, did not succeed, they did not manage to find a solution that would be able to satisfy at least the majority of specialists.

As noted above, in the 21st century this problem has risen much sharper and deeper than John Hardwig saw it. The problem of Big Science in the modern world encompasses projects that not only involve different groups of scientists, different schools and different specializations, these projects increasingly involve the participation of scientists from different branches, sometimes using different methods and approaches. Integration becomes a major issue when we consider research in compatible sciences, and sciences of completely different types can be combined within the same project. The problem of integration of scientific approaches and schools, that is important to note, cannot be considered outside of problem of differentiation, since they are different aspects of a single process. Whichever way we look at discussions of the trust and authority of experts, Harding's dilemma can find its theoretical solution – both within the framework of a single physics experiment and for a large interdisciplinary research project – only in a much more general way, within the awareness of relationships and separation functions and powers in general. This problem for each individual project or experiment is usually resolved through specific people, specific circumstances, and specific initial conditions. The reliability of the results obtained is a special issue, and it also has an integration-differential dimension, because the epistemic reliability that is the subject of the search is largely determined by checking independent sources. It would seem that the theory of Karl Popper prevails here, who insisted that independent repetition ensures the scientific character and reliability of research. But we have already noted above that the issue of repetition and review of scientific results today gives rise to many difficulties. We see an even more complex picture in practice: only a small number of results are verified, most are simply taken for granted, and sometimes there is a declaration that it is impossible to reproduce the results of other scientists' research. Replicating and validating the scientific results of significant costly projects also requires significant funding, but in an environment where most scientific research exists on the basis of funding from industrial corporations, much less money is allocated for verification and reproduction than for independent scientific research. Corporations have no interest in the development of scientific knowledge in general (quite the opposite), they are focused on developing a unique product that can provide them with profit. All this contributes to limiting the number of checks and reproductions, and generally speaking, it significantly changes the nature and forms of the existence of science and the activities of scientific institutions that we are able to observe with our own eyes.

To understand this situation, we are forced to recall those provisions of the theory of Thomas Kuhn, where he considers the features of the activities of scientific institutions and make some adjustments to it in relation to the current state of science. The perception of information based on simple (including irrational) faith is growing not only in the non-scientific world, but also knowledge in science is growing in direct proportion to other evidence. On the other hand, this also contributes to the growth of distrust in scientific research and results, which is easy to see today in the example of attitudes towards vaccination in the context of the spread of the COVID virus.

The indicated situation contributed to a new growth of attempts to draw a demarcation line between science and non-science (or pseudoscience), was almost stopped which at one time due to the well-known work of the American philosopher of science and epistemologist Larry Laudan, in which the author cited evidence of the impossibility of finding criteria for something as heterogeneous as scientific methodology [19].[16] But due to the changing situation in science and the use of new research approaches, some philosophers of science have returned to this problem in recent years, arguing that the old concept can be clarified by other means than necessary and sufficient criteria [20,21],[17] or that such a definition is indeed possible, although it must be supplemented by discipline-specific criteria in order to

15 "Scientists, researchers, and scholars are, sometimes at least, knowers, and all of these knowers stand on each other's shoulders in the way expressed by the formula: B knows that A knows that p". Hardwig (1985), 345.

16 Laudan (1983).

17 Pigliucci (2013); Mahner (2013).

acquire usefulness.[18]

Eric Winsberg, Bruce Huebner, and Rebecca Kukla attempted to approach the problem of trust and relationship in multiple authorship research in a slightly different way [22].[19] They abandoned the term Big Science, suggesting the term "radically collaborative research" as a more accurate way to address the supra-empirical and ethical issues of collaboration between researchers from fundamentally different branches of science and with fundamentally different forms of knowledge that are combined to produce an experimental result. The focus of their attention is not the question of the reliability or validity of the results obtained, but above all the question of responsibility. How is it possible to talk about the integrity of the study if it was carried out by researchers not only with different interests, but also with different methodological standards? So they ask the question. The old *laissez-faire* model of crowd wisdom (which assumes that local differences in methodological standards balance each other out) can only be seen as adequate, in their view, in terms of the question of the reliability of the results of the study, but does not meet the requirements of solving problems of responsibility. But such a new model of social cooperation - like the model of combining different instruments - they not only did not propose, but could not even find the basis for its construction. They only insist that such a model must be found in the moral dimension of research cooperation, and they understand the issue of responsibility in this way. And we return again to questions of ethics as the basis of the methodological and epistemological problems of modern science.

As a prelude to the results of this article, it should be recognized that the modern philosophy of science recognizes the existence of significant changes in the activities of scientific communities and the structure of scientific institutions since the beginning of the new century. On the one hand, the fact of the spread and the advantage of group, project research in the modern world is beyond doubt, on the other hand, such a change in the nature of scientific research is perceived by the philosophy of science as a certain challenge, to which it still has not found an answer, but continues to intensely search for it. The philosophy of science mainly notes a certain crisis in the traditional ethical forms of organizing research work, and concentrates on searching for the beginnings of a new practical ethics of cooperation between scientists, which would be able to provide, firstly, a reliable research result, and, secondly, (but not with less value) a clear delineation of responsibility in relation to both the results and methods of scientific research. At the same time, the reliability and methodology of decisions in the field of ethics remains uncertain and causes numerous disputes among philosophers. We can observe a paradoxical situation: the reliability and validity of methodological and epistemological decisions of even the most rigorous branches of modern science are increasingly beginning to depend on the much looser and lacking a single foundation of the provisions and recommendations of modern ethical philosophy. Strict scientific knowledge, which challenged traditional ethics at the beginning of the last century, is now increasingly turning to it to solve its own problems.

We have identified several consecutive ethical nodes that fundamentally determine the process of growth of scientific knowledge today. First of all, this is the problem of the relationship of values (and methodological principles) and trust in professional competence between employees in the framework of a research project, secondly, the problem of verifying the reliability of research results and their effectiveness when applied in practice (peer review and replication), thirdly, the question of the responsibility of the researcher himself. One should also take into account the complex issue of the integrity of the published results, which we almost did not consider.

It can be assumed that the modern world philosophy of science is at a turning point in the same way as its subject, namely, modern world science: society's faith in science is significantly reduced (although science remains the main source of knowledge about the surrounding world), because science is unable to solve the basic problems of human existence, at the same time, science itself is in a situation of heightened search for the latest criteria of reliability and truth. Such a situation denotes periods of crisis or paradigm shifts (according to Thomas Kuhn), they can only find their solution through new principles for organizing relations between the scientific community and social structures.

And the last thing I cannot say. I am writing this article in a situation when Ukraine, my country, is waging a fierce military confrontation with the invasion of its territory by the Russian army. The support that the world community provides to Ukraine and the sanctions that are applied against Russia are largely due to a certain ethical understanding of this situation on the part of the world community. It is ethical motives that prompt the population of many countries to put pressure on numerous political, governmental, financial and other structures and institutions, forcing them to overcome numerous obstacles and even often go against their own benefit in solving problems in support of Ukraine, which is waging a just war against an aggressor that is much superior to it. This fact, outwardly far from the philosophy of science, emphasizes the vital importance and incredible power of ethics, no matter how weakly this ethics is methodologically

18 Hansson (2013).

19 Winsberg, Huebner, Kukla (2014).

substantiated and no matter how doubtful its ontology may seem.

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