THE CONCEPT OF NORMAL SCIENCE

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Abstract. Normal science is characterized as the central concept of Kuhn's theory, as the major novelty it introduces. What Kuhn expresses regarding themes such as research traditions, scientific revolution, incommensurability, and scientific progress can therefore be well understood only by reference to his concept of normal science. From this perspective, the article examines some critical observations frequently formulated by philosophers of science regarding the theory presented in *The Structure*...

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The Structure of Scientific Revolutions, a book that was published 61 years ago, challenged already entrenched and popular representations of scientific research. A brief outline of some of these representations can provide a good background to the major novelty this book has brought.

At the time, many philosophers were inclined to see scientific research as an enterprise akin to that through which new, previously unknown territories are discovered and explored. Researchers who had made essential contributions to the building and development of modern natural science – Galileo, Newton or Darwin – were compared to explorers of new continents and territories. And important discoveries were compared to the deeds of great explorers. Newton, for example, explored one territory and Einstein another. Then Heisenberg, Dirac and Schrödinger, for the first time, entered a previously unknown world, the world of quanta. From such a perspective, the development of scientific knowledge was portrayed primarily rather as the result of the initiatives of exceptional personalities.

Even more authoritatively, what I would call the "epistemological tradition" dominated the philosophy of science in the middle of the last century. What I mean by this phrase is a certain view of knowledge, a view that had already taken shape in ancient Greek philosophy. It is the view that a major achievement in knowledge

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occurs when two kinds of approach are brought together and combined: that of creative imagination, the product of which are new, bold ideas – hypotheses of great scope – and that of the critical, sober and severe examination of these ideas by confronting them with findings of fact. Such interplay of creative and critical thinking would have given rise to scientific theories such as those developed by Newton, Darwin, and Einstein. Scientific research was thus presented as an exemplary illustration of the creative and critical exercise of the human mind. That image of "heroic science" sketched by Karl Popper (the English version of *The Logic of Scientific Discovery* appeared in 1959, three years before Kuhn's book) – bold conjectures and the most severe tests – can be counted as a polished presentation of this view of scientific research. It was the idealized image that its vehicles – the theories, laws, models, methods, and tools used – are subject, or should be subject, to suspicion, testing and critical examination at all times. From such a perspective, research traditions in which certain achievements and practices are, at least for a period of time, exempt from critical scrutiny were seen as incompatible with supposedly timeless norms of scientific rationality. It was explicitly suggested or stated that in scientific research, unlike in other areas of culture, the authority of the tradition would be misplaced and usually harmful. Such a characterization of the "scientific spirit" had become popular in the middle of the last century through biographies of great scientists, works popularizing science, and the public discourse of some leading scientists.

One more observation about that image of scientific knowledge that was dominant in English-speaking philosophy of science when Kuhn's book appeared. Philosophers of science were, as a rule, philosophically trained people. Naturally, they were inclined to examine scientific research by relating it to themes such as that of justifying the knowledge claims of the hypotheses and theories. Beyond the differences between approaches centered on inductive or deductive approaches, on confirming or refuting hypotheses, the perspective on the rationality of scientific knowledge illustrates very well the interests of those who were looking at it from a general philosophical perspective[1].

Quite different were the starting and supporting points of the approach outlined in Kuhn's book. Immediately after the war, the author studied physics and obtained his doctorate in physics at Harvard under the supervision of the physicist John Hasbrouck Van Vleck, a researcher who was awarded the Nobel Prize in Physics in 1977 together with Ph. W. Anderson and N. F. Mott "for fundamental theoretical research on the electronic structure of magnetic and disordered systems". Kuhn published two articles in the *Physical Review* in 1950, one of them in

[1] I believe that it is precisely the view that Kuhn had in mind when he noticed, in the "Preface" to his book, about the contemporary works in the philosophy of science: "Where I have indicated skepticism, it has more often been directed to a philosophical attitude than to any one of its fully articulated expressions." (*The Structure of Scientific Revolution*, Second Edition, enlarged, Chicago and London, The University of Chicago Press, 1970, p. x).

collaboration with his mentor Vleck[2]. With a brief but significant experience as a researcher, Kuhn turned to research in the history of the physical and chemical sciences. By the time his book appeared in 1962, he had completed more than 10 years of research and teaching in this field. The background to his original conception of scientific research was therefore interests, skills and experiences substantially different from those of the most influential philosophers of science of those years. His approach was not logical and epistemological, but that of a historian of the physical and chemical sciences, backed up by his own brief but conclusive experience as a natural scientist.

The point that cannot be emphasized enough is that the theory formulated in *The Structure of Scientific Revolutions* concerned only what Kuhn called *mature science*. The foundation of this original theory is an examination of the activity which the author designated by the expressions *normal research* or *normal science*. This clarification is, in my view, of particular importance because, in almost all of the immense literature written on Kuhn's book, his theory has been received and discussed on the assumption that it would concern scientific research in general. It must be acknowledged that the author bears the main responsibility for this inadequate reception of the scope of his considerations. This is because, throughout his writing, he refers to "science" or "scientific research". Not only the critics of his theory, overwhelmingly philosophers of science, but also many readers, risk missing such clarifications as the fact that all his statements are based on data from the history of the physical and chemical sciences[3].

I think it is very important to specify the objects of the considerations made in Kuhn's book. The author states and emphasizes that these considerations concern only the "mature science", science whose distinctive feature is the normal research. What he had in mind was mainly the physical and chemical disciplines, as well as the experimental part of some biological disciplines. Kuhn does not dispute, of course, that other disciplines can also reach the stage of development which the term normal research designates. It is basic scientific research that has reached the stage of maturity. On the other hand, it should be noted that the author took no responsibility for attempts to apply generalizations made in his book to other disciplines – namely to various social or humanistic disciplines. He has warned his readers about this. Or if Kuhn's claims relate only to what he has designated by the expression *normal science* then it follows that his generalizations concern a much more limited field than the one usually considered by philosophers of science. This is what seems to have been overlooked by some of the commentators and critics of the considerations formulated in Kuhn's book.

[2] For further data on Kuhn's early years career, see Stephen G. Brush, *Thomas Kuhn as Historian of Science*, in *Science and Education*, 2000, pp. 39–58.

[3] In the "Preface", Kuhn specified that: "Furthermore, that evidence comes from the history of biological as well as of physical science. My decision to deal here exclusively with the latter was made partly to increase this essay's coherence and partly on grounds of present competence." (Th. Kuhn, *The Structure…*, p. ix).

In what follows, I will briefly outline my understanding of the concept of *normal science*, starting from considerations in Kuhn's book, as well as from some clarifications in his later writings. I will try to point out that everything that is said in this book about topics such as anomalies, crises, extraordinary research, scientific revolution or progress in scientific knowledge can only be properly understood by reference to the characterization that has been given to normal science or research.

As it is well known, in the middle of the last century, philosophers of science paid a great deal of attention to the theme of delimiting scientific knowledge by reference to the knowledge claims of speculative philosophy or other intellectual disciplines. The logical empiricists or Karl Popper and his followers developed criteria for the delimitation of scientific knowledge, and had many controversies on this topic. I think it is important to note and emphasize that Kuhn's approach gave a new turn to this discussion. From the perspective of his approach, the search for a general and formal criterion for delimiting scientific knowledge is no longer justified. This is because scientific knowledge, in a restrictive sense of the term, exists everywhere where there is normal research and only there. Normal research, Kuhn says, "clearly distinguishes" scientific work "from every other creative pursuit..."[4]. It is hardly surprising that this turn in the discussion on the delimitation of scientific knowledge was initiated by a historian of exact science of nature with some experience of his own as a researcher. I think we must pay close attention to the observation that, when looking at human activity and the products of that activity, the historian's gaze will be oriented differently from that of the theorist. The theorist of scientific knowledge, i.e. the typical philosopher of science, starts by examining the results of the research – experimental data, theories, laws, models – whereas the historian's gaze is predominantly directed towards the activity that generates and validates scientific knowledge, as well as towards what distinguishes a community of scientists in aspects such as the training of researchers, professional communication, the identification of research topics as well as the evaluation of research results. The general philosophical interest in science will make the past of a field of research to be seen mainly from the perspective of present science, while for the historian of science, the focus of attention will rather be on what is particular and specific, what distinguishes and singles out different historical research traditions[5].

Kuhn delimits and characterizes *normal science*, the central object of his considerations, by two features: a) the formulation and solution of problems of a particular type which he called *puzzle* problems: b) the formulation and solution of such problems takes place by reference to certain exemplary scientific achievements,

[4] *Ibidem*, p. 136.

[5] Perhaps here we can look for an explanation of the difference between the very strong impact Kuhn's book has had in the world of philosophers of science compared to the attention it has received in the field of history of science. I suppose that Kuhn's ideas did not seem to historians as novel and challenging as they appeared to philosophers.

which he called paradigms. "In this essay", writes Kuhn, ""normal" science means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice"[6]. In Kuhn's characterization of normal science, these two features cannot be separated. *Paradigms* are concrete formulations of and solutions to problems; they provide researchers in a given field with guidance and direction in both formulating and solving the problems they are working on. In the author's own words, paradigms are those "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners"[7].

However, in many passages of Kuhn's book, the term *paradigm* is used in a different way, namely to designate everything that unites the members of a community of researchers: theories and other correlations of a high level of generality, also called by Kuhn "symbolic generalizations", ontological and heuristic models, common values, exemplary problem formulations and solutions. In the *Post Scriptum* of his book, written in 1969, Kuhn considered that the most appropriate term to designate all these common commitments of a smaller or larger group of researchers, in a community characterized by fluent and close communication and by the convergence of professional evaluations, is that of *disciplinary matrix*[8]. *Paradigms*, as exemplary formulations and solutions of research problems, by which members of the group orient themselves in formulating and solving new problems, are the defining element of what Kuhn calls *normal science* or *normal research*. This is because the presence and use of paradigms – exemplary problem formulations and solutions – which provide researchers with the reference system for formulating and solving *puzzle* problems, set the contours of the concept of *normal science*. Referring to numerous critical reactions to the considerations contained in *The Structure of Scientific Revolutions*, Kuhn considered *paradigms*, in the narrow sense of the term, to be 'the newest and least understood aspect" of his book. More than a decade after the book's publication, Kuhn would describe the use of the term *paradigm* for all commitments shared by a community of researchers as "confusion". This was a usage that "obscured the original reasons for introducing a special term"[9]. In the *Post Scriptum*, written in 1969, Kuhn stated unequivocally that the view widely accepted by philosophers of science, namely that scientific knowledge is embodied in theories and rules, and that the formulation and solution of research problems would be an application of these theories and rules constitutes a wrong localization "of the cognitive content of science"[10].

[6] *The Structure…*, p. 10.

[7] *Ibidem*, p. viii.

[8] In communities welded by disciplinary matrices, the professional communication is relative fullness and the professional judgements have relative unanimity (*Ibidem*, p. 182).

[9] See Th. Kuhn, "Second Thoughts on Paradigms", in *The Essential Tension. Selected Studies in Scientific Tradition and Change*, Chicago, The University of Chicago Press, 1977, p. 319.

[10] Th. Kuhn, "Postscript", pp. 187–188.

Problems in the formulation and solution of which researchers follow certain paradigms, i.e., certain problem formulations and solutions that they consider exemplary, have been called *puzzle problems* by Kuhn because they are much like the problems that confront those interested in games such as crossword puzzles, in reconstructing coherent pictures by assembling a large number of pieces, or in endings in chess games. Two features distinguish these problems, as anyone engaged in solving them knows well. First, the existence of solutions is guaranteed. Secondly, finding the solution is difficult, requiring talent, ingenuity and a great deal of perseverance. Kuhn states that the problems that researchers formulate and solve in the activity called normal science have precisely these characteristics. On the one hand, the exemplary scientific achievements by which they conduct themselves support the conviction that these problems have a solution. On the other hand, success in the search for a solution depends above all on the researcher's ability to find an unapparent, hard-to-grasp similarity between the problem he is trying to formulate and solve and the scientific achievement that serves as his paradigm[11]. The typical approach of normal research is therefore not from the general to the particular, from explicitly formulated theories and rules to the formulation and solution of a particular problem, but from the particular to the particular. That is to say, from an exemplary formulation and solution of a problem – the paradigm – to the problem that the normal scientist tries to formulate and solve. Unlike knowledge contained in theories, laws and general rules, knowledge contained in paradigms can be characterized as largely "tacit knowledge". It follows that the initiation into scientific research that is practiced as normal science will resemble, in some respects, that apprenticeship that prepares the next craftsmen and artists for an activity that involves a significant coefficient of creativity[12].

What is particularly striking in Kuhn's book's characterization of normal science is that this research does not pursue major theoretical or methodological innovations, that the research results are broadly predictable, that they offer no major surprises. At first glance, this feature seems hard to reconcile with the enthusiastic engagement specific to many practitioners of normal science. With respect to this observation, Kuhn argues: "Although their results can be anticipated,

[11] What happens in actual research, Kuhn observes, resembles situations that are typical in education that prepares for research activity in disciplines such as physico-chemical ones. Here the difficulties the student is facing in trying to solve problems formulated at the end of a textbook chapter are usually overcome when he manages to see the problem "as *like* a problem he has already encountered". So it is with the problem formulation and solving in the case of researchers practicing normal science. The key to success often lies in their ability to use the guidance provided by a paradigm, i.e. an exemplary scientific achievement. Talent in normal research therefore lies mainly in the ability to follow such an achievement by detecting similarities that are difficult to establish. (See *ibidem*, pp. 189–191).

[12] For further considerations and comments on this topic, see Mircea Flonta, *Despre natura consensului științific* [*On the Nature of Scientific Consensus*], în M. Flonta, *Imagini ale științei* [*Images of Science*], București, Editura Academiei Române, 1994.

often in such detail that what remains to be learned becomes, in itself, uninteresting, the manner of obtaining these results is extremely uncertain. To solve a normal research problem is to obtain the anticipated in a new way, and to do this, all sorts of complex puzzles, instrumental, conceptual and mathematical, must be solved."[13] Clearly, a problem that reputable researchers, sometimes from different generations, have failed to solve is a challenge, and success is a great satisfaction and a generous source of professional reputation. The pursuit and attainment of a goal, which is at least roughly foreshadowed, but whose attainment requires imagination, ingenuity and a great deal of competent work, is the feature that completes the picture of *normal science* sketched by Kuhn. We are far from the often invoked image of scientific research as the discovery and exploration of an entirely new world, an approach that often confronts and overturns the habits of common thinking, revealing surprising, astonishing, unforeseen things. We have to admit that the most successful researchers who have achieved great successes in activities that are representative of normal science do not possess the halo of abstract power that those researchers who have discovered and explored new worlds, the leading actors in the literature of science for the educated public, exercise. We will have to admit, Kuhn suggests, that what seems less spectacular and exciting, more prosaic, is largely characteristic of the current scientific life in the age of maturity, i.e. of *normal science*.

Representative to illustrate this feature of *normal science* are smaller or larger groups of researchers in a close interaction, aiming to achieve an ambitious but also very clearly foreshadowed knowledge goal. In such groups, there is division of labor and, at the same time, close communication and interaction. Experimental work often makes a decisive contribution to achieving the objectives of a normal research project. Collaboration is especially important in the conception of experiences and the division of labor in their realization. In this respect, a good illustration provides a group of researchers who have undertaken, since the '50[s] of the last century, systematic research of genetics at the cellular level. The aim of this research was to reproduce a certain species of viruses, called *bacteriophages*, in the host bacterium. The group was called *Phagegroup*, meaning the group of bacteriophages. Most members of this group, led by microbiologist Max Delbrück, were physicists who engaged in experimental microbiology research. It is reported that only eight researchers attended the first meeting of the group in 1947. In 1952, the first international symposium of researchers working on this project was organized. The group's paradigms were works that paved the way for the study of heredity at the elementary level, the cellular level. The research mainly sought an answer to the question "How the virus uses the elements of the host cell to replicate in a short time and how this replication takes place." In the case of the group of

[13] Th. Kuhn, *New Reflections on Paradigms*, p. 98.

bacteriophages, it can be very well seen how paradigms ensure communication and cooperative and critical interaction between researchers, even when working hypotheses and their expectations are very different[14].

The contrast between the very clear outline of the research objective, on the one hand, and the talent, imagination and perseverance that are necessary to achieve it, on the other, a contrast that distinguishes normal science, stands out well in a field of basic research called “high energy physics”. The central objective of this research group was to experimentally identify certain effects predicted by theory. A well-known science philosopher, Jan Hacking, appreciated that the physics of high energies exemplifies a major feature of the problems of normal science: the results pursued in the research are clearly foreshadowed but they are very difficult to obtain[15].

The features of normal research, as Kuhn characterizes, can very well be deduced from a broad account of a particularly ambitious research project, which has spread over many years, the project to recover the Neanderthal genotype from fossil remains. This goal seemed very difficult if not impossible to achieve, given a wide variety of obstacles that had to be overcame for its realization. The initiator and coordinator of this project was the Swedish researcher Svante Pääbo, recently awarded the Nobel Prize in Biology. He recounted the long history of this discovery in a book published in 2014[16]. It’s a story about which Eduard O. Wilson, a big name in the world of biological research, wrote that it is „...a correspondence from the front and if you want to know how science is really made then I suggest you read it”[17]. A careful reader of Pääbo’s book will find that the long research that led to this discovery very well exemplifies the description of normal research as an activity in which *puzzles problems* are formulated and solved. Let’s now see only a few indications in this respect from Pääbo’s account.

Paleontological research had highlighted some important approaches and differences between Neanderthal man and Homo Sapiens with consequences of the most important for writing and rewriting the history of our species. However, the establishment of the Neanderthal genotype and its comparison with that of the present man and some close relatives promised an incomparably broader and more precise knowledge of these similarities and differences. Extracting DNA from cells of Neanderthal fossils has proven to be an extremely difficult goal to achieve. His accomplishment required a large number of stages, first for the DNA in the mitochondria and then in the nucleus. During this research, answers to a considerable number of specific questions had to be sought and found, by imagining and using a

[14] For further considerations and comments on this topic, see note 12 above (Mircea Flonta, *Imagini ale științei*, pp. 175–180).

[15] See I. Hacking, *Introductory Essay*, in *The Structure of Scientific Revolutions*, 4th Edition, Chicago, University of Chicago Press, 2012.

[16] See S. Pääbo, *Neanderthal Man*, New York, Basic Books, 2014.

[17] *Ibidem*, p. 2.

wide variety of experimental processes and techniques. At each stage, the result pursued was clearly outlined, at least broadly, but obtaining this result required a lot of inventiveness in imagining paths of research, as well as in finding and producing appropriate techniques and tools. All this is presented and described in Pääbo's book on more than 200 pages. The emphasis is on presenting the emergence of main questions that led to the establishment of research objectives, as well as on presenting ways to obtain the necessary information, in each of the great stages that the realization of the project went through. Often, alternative ways and methods to achieve one or another of the intermediate objectives of the research had to be tested and evaluated step by step. The author highlights not only the many difficulties that arose at each stage of the research submission but also the impasse that occurred during its development, those periods when the difficulties seemed invincible and doubt, disappointment and discouragement threatened to become dominant. They were those states of deep frustration that resemble those experienced by a climber who knows very well that the peak exists, that it can be reached, but he comes to doubt that he has the resources to reach it. Normal research is portrayed in Pääbo's account as an activity in which the ability to continuously imagine new approaches, new remedies, invent new strategies and produce new techniques and tools, with tireless attention to detail, make it possible to continue moving towards a goal that is clearly outlined from the beginning and remains firmly fixed. The title of one of the chapters of the book records what conditioned the success at every stage of the research through the words: *The devil in the details*. Only partial successes can support confidence in the possibility of achieving the ultimate goal, despite repeated disappointments caused by the finding that promising approaches have proved ineffective. Relating to the many impasses of the various attempts to recover DNA from Neanderthal fossils, Pääbo emphasizes that the key to the ultimate success was maintaining confidence, the ability to imagine and explore, after each failure, new ways forward, and explore these paths with great patience and attention to detail. The contrast between the anticipated results and the difficulty of obtaining them proved to be huge. This contrast sheds a vivid light on one of Kuhn's remarks, quoted in note 13: „Solving a normal research problem means getting the anticipated in a new node and, for this, all kinds of complex, instrumental, conceptual and mathematical *puzzles* must be solved."

The challenge of core ideas in Kuhn's book has been characterized in different ways. I believe that one of the major challenges was the author's statement that the presence of normal science is an indication that a field of research has reached maturity. Kuhn argued that the presence of normal research is what clearly distinguishes science from proto-science, speculative explanations of natural phenomena, as well as pseudo-science. And since we are able to determine whether or not one field of research has reached the stage of normal research, therefore the concern to draw a general criterion to determine scientific knowledge cannot be justified. However, we may wonder what was behind Kuhn's choice of the phrases *normal science* and *normal research*. One answer could be that these expressions designate scientific research in its best appearance, i.e. scientific research *par excellence*. As soon as a field of research reaches this stage, discussions on its foundations cease. A strong consensus is being inaugurated, both in terms of identifying important research issues and in assessing the solutions to these problems. The inauguration of paradigm-driven research relieves the research community of the need for critical examination of foundations and thus promotes a focus on formulating and solving those problems that are defining for this type of research. The consequence is a saving of resources and a corresponding increase in yield. Kuhn did not bother to insist that the consensus that is provided by common paradigms is distinctive for scientific research in the strict sense of the word, i.e. *mature science*. All qualified researchers usually share answers to questions such as: „Based on which indices can be identified an important and promising research topic?", „Where can suggestions be sought for guidance in research on this topic?", „What are the indications that allow us to determine if a certain research is on the right track or is at a standstill?", „How can we prioritize research results and determine what constitutes a major scientific achievement?". It is the agreement of all those involved on such questions and answers that distinguishes normal science, i.e. the research that has reached full maturity.

Let us now ask ourselves: how will the history of science present the history of a discipline that has reached the stage of maturity? Kuhn's response was: as a succession of relatively long periods of normal research, periods in which consensus on foundations is firm, and efforts are focused on formulating and resolving *puzzles problems*, and shorter periods, that he called periods of *extraordinary research*, when this agreement is shaken. These are the periods that usually precede the scientific revolutions, which Kuhn characterized as those episodes in which a certain tradition of normal research is replaced with another. In mature scientific disciplines, this would be the difference between normal times and exceptional episodes, between those in which the research is in the best condition and those in which the most important indicator of normality, the agreement on the foundations, is affected. Restoring this agreement after a scientific revolution will initiate a new era of normal research. In other words, in mature scientific research, the consensus on foundations is normal and its discussion characterizes those exceptional periods that precede scientific revolutions. Naturally, Kuhn could not withhold the observation that many philosophers of science were inclined to identify scientific research to its exceptional state, not to its normal one.

It is known that the critical discussion of Kuhn's book focused heavily on scientific revolutions, on what is being said here about the incommensurability of normal research traditions and the progress of scientific knowledge. However, it cannot be highlighted enough that in Kuhn's considerations the stress is on emphasizing the fact that in a mature science revolutionary changes are rare, and are preceded and followed by long periods of "convergent research"[18].

In a certain tradition of normal research, the research that Kuhn calls *extraordinary*, i.e. the research that precedes the scientific revolution is inaugurated by what he calls *crisis*. For Kuhn, however, the crisis does not designate a state that can be objectively identified, according to some signs, but rather the researchers' reaction to developments that take place in the process of normal research. Accumulating important issues that have not received a satisfactory solution, despite the efforts of talented and motivated researchers, sometimes over several generations, it will not inevitably generate a crisis, i.e. a loss of confidence in finding a solution in a certain tradition of normal research, Kuhn observed. In this respect, confidence may persist for some time, despite persistent failures. Which, at some point, some researchers are inclined to appreciate as counter-examples, those of their colleagues who are firmly attached to a normal science tradition will consider, on the contrary, that there are *puzzles problems*. The latter will find that there are good grounds for hoping that paradigms will reaffirm their ability to fruitfully conduct the research. Kuhn appreciated that an important part of the normal research is dedicated to the dissolution of anomalies. He even considered it the most interesting and promising part of the normal research, stating that success in such activities is the highest generator of prestige and reputation in the mature scientific research. However, persistent failures will at some point lead to a change in the state of mind and the dominant opinion. In this case, the persistent failure to solve some central *puzzles* will no longer be attributed to the limits of the imagination and inspiration of the most talented and renowned researchers, but it will question the very ability of the paradigm to lead to satisfactory solutions. What for a long time had been attributed to limits of researchers' ability, without affecting confidence in the paradigms, will now appear as a sign of nature that highlights the indelible limits of a research tradition and indicates the need to adopt a new research framework[19].

The description of a research program in a central field of natural science called "fundamental physics" illustrates well Kuhn's description to crisis in the scientific research. Towards the end of the last century, the so-called "standard model" promised the unification of all the results obtained until then by investigating elementary particles. The standard model has led to many experimentally confirmed

[18] This is why I believe that chapters IV and V of the book, entitled "Normal Science as Puzzle-Solving" and "The Priority of Paradigms", can be appreciated as its core chapters.

[19] Such considerations highly refine the claim that in scientific research all statements, including those concerning foundations, would always be subject to fact-checking. As long as the authority of a normal research tradition has not been eroded, what many philosophers of science are tempted to characterize as the continuous testing of theoretical principles by confronting them with facts will be perceived more as the activity of formulating and solving puzzle problems.

predictions. It was a theory generally accepted by theoretical physicists and appreciated as a very successful physical theory. After this major success, the next goal that emerged in fundamental physics was the unification of elementary particles with gravity. This is the project that has been pursued in recent decades by developing the string theory. In this project, in which many of the most prominent theoretical physicists in decades have engaged, the final aim was the unification of all physical interactions. It can be argued that research related to string theory has features specific to normal research. The objectives of the research were configured very clearly, down to the point of detail. It was well known what results were expected to be achieved. On the other hand, it was recognized that obtaining them requires in the highest degree talent, ingenuity, imagination, creativity. And that achieving the objectives pursued could generate prestige and professional reputation to the highest degree. It is therefore not surprising that many of the most gifted researchers in fundamental physics have been attracted to this project.

Despite some successes in the realization of the project, until today it has not become possible to derive predictions that can be confronted with experimental data. The author of a relatively recent paper, where the string theory is addressed from the perspective of the evaluation criteria of theories in physics, reaches verdicts such as: "No real break has been made to allow specific quantitative calculations of the observables based on the fundamental principles of string theory."; "Today it is not possible to derive any quantitative prediction from the basic principles of string theory."[20] It is also recognized that there are no prospects of achieving the required values in reactors that accelerate particles to high energies so that predictions derived from string theory can be tested.

Reactions to this situation indicated that a disagreement was formed in the community of theoretical physicists, a disagreement that can be considered a symptom of the crisis, as described in Kuhn's book. The critics of his theory, including first-rate theoretical physicists, emphasize that the theory is not complete and, above all, that after many decades there is no prospect of empirical testing of some of its consequences. From this perspective, the critics characterize string theory as a fundamental hypothesis that has not been confirmed. They blame the proponents of the theory for overestimating its achievements. "The structural beauty" of this theory, critics argue, cannot compensate for the lack of empirical confirmations and, even more so, the lack of perspective of experimental confirmations. In opposition to the critics, the supporters of the theory maintain their confidence and enthusiasm for the project. Where the former see a project failure, the latter appreciate that there are only problems to be solved. They have no doubt that these problems will eventually be successfully resolved. In other words, in Kuhn's terms, where critics see the crisis emerging, project supporters appreciate that it is a

[20] Richard David, *Strings Theory and The Scientific Method*, Cambridge, Cambridge University Press, 2013, pp. 17, 18.

solution to certain problems, solution that can be found through a considerable investment of talent and creativity. In other words, they appreciate that these are problems of normal research.

In describing the development of a normal research tradition, as Kuhn sets forth, the persistence and deepening of the state that he calls crisis inaugurate a period of research characterized by the willingness of the research community to carefully examine and adopt, finally, radical alternatives to established paradigms. These are the episodes that Kuhn calls *scientific revolutions*. The title of his book suggests that its core topic would be the scientific revolution. In fact, the paper sheds new light on topics such as anomalies, crises, extraordinary research, scientific revolution and the progress of knowledge, from the perspective grounded in the development of the concept of normal research – formulation and solution of *puzzle* problems – research that is guided by paradigms.

From this perspective, what makes scientific knowledge knowledge par excellence is, on the one hand, firm agreement and fluent communication between those involved in this type of research and, on the other hand, the existence of progress. Each of these features acquires their clearest and most emphatic expression in the *normal science*. This is what Kuhn stressed when he labeled extraordinary research and scientific revolution as "exceptional episodes" in the history of a mature science[21]. In scientific revolution, characterized as a transition from one normal research tradition to another, progress is much more problematic than in normal science. A careful examination of scientific revolutions, with the tools of the historian of science, Kuhn pointed out, does not confirm the idealized representation of progress according to the scheme rejection – takeover – overtaking. Such a representation may be maintained, at least to some extent, if the scientific revolution is characterized as the replacement of one theory by another, but not if it is examined as a transition from one normal research tradition to another. If it is considered and characterized as a change in the paradigms guiding normal research, then the scientific revolution will no longer appear as an episode that conforms to the popular scheme of progress. As soon as these episodes are viewed with the eyes of the historian and sociologist of science, it will be found that in the transition that takes place through scientific revolutions there are not only gains, but also some losses, and this is because the change of paradigms will lead to significant changes in those criteria that guide the researchers when they appreciate what distinguishes a good description or a scientific explanation. For example, the characterization of the progress of scientific knowledge as a gradual approach, through successive approximations, of truth, an accentuated cumulative representation, will appear completely unacceptable from the standpoint of the outcomes provided by an

[21] This is how he argued in a text he wrote while he was working on his book: "Almost none of the research undertaken by even the greatest scientists is designed to be revolutionary, and very little of it has any such effect… revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them." (*The Essential Tension*, p. 227)

historical research. Such research shows that, not infrequently, scientists guided in their work by different paradigms cannot agree on what is an important research problem or an acceptable solution to such a problem[22]. Many of the accusations of "irrationalism" or "subjectivism" to the author of the *Structure of Scientific Revolutions* can hardly be maintained if current representations of topics such as the rationality of scientific knowledge, scientific revolution, or the progress of knowledge will be confronted in a careful and persistent way with conclusions resulting from careful research into a number of significant historical episodes. What Kuhn claimed and emphasized both in his 1962 book and in other texts he published during this period is that progress, as an evolution with a cumulative character, is more obvious in current normal research than during the crisis of a research tradition or the revolutionary transitions from one research tradition to another.

In conclusion, I will briefly set forth two remarks.

The first one concerns the reception and use of the phrase *paradigm* in the comprehensive literature dedicated to the presentation and critique of the conception outlined in the *Structure of Scientific Revolutions*. In this book, Kuhn acknowledged, the phrase was used in different ways: often to designate everything that a community of researchers or a smaller group of researchers shares, and sometimes, only with reference to concrete scientific achievements, those achievements that provide guidance in formulating and solving *puzzle* problems. Returning to this topic in the *Postscript* of the 1969 edition of his book and in another text, published in 1974 and entitled *Second Thoughts on Paradigms*, Kuhn made some unequivocal clarifications. He acknowledged that he used the phrase *paradigm* in two different ways, and therefore he was responsible for the misunderstandings generated by this ambiguity: "The paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect of this book."[23] A few years later, Kuhn provided further clarifications. He wrote that the term paradigm "… entered *The Structure of Scientific Revolutions* because I, the book's historian-author, could not, when examining the membership of a scientific community, retrieve enough shared rules to account for the group's unproblematic conduct of research. Shared examples of successful practice could, I next concluded, provide what the group lacked in rules. These examples were its paradigms, and as such essential to its continued research. Unfortunately, having gotten that far, I allowed the term's applications to expand, embracing all shared

[22] Based on such findings, Kuhn formulated some conclusions that sparked the protest of those philosophers of science still attached to more idealized views on the progress of scientific knowledge. Here is one of these: „To the extent ... that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent." (*The Structure...*, pp. 109–110)

[23] See *ibidem*, p. 187.

group commitments, all components of what I now wish to call the disciplinary matrix. Inevitably, the result was confusion, and it obscured the original reasons for introducing a special term. But those reasons still stand."[24]

Why have clarifications like these seemed so important to the author of the *Structure of Scientific Revolutions*? Because it is only with respect to this use of the phrase *paradigm* that his characterization of normal science as puzzle problems solving is supported. Therefore, it is only in relation to this use of the phrase that a satisfactory reading of his book becomes possible.

It is truly surprising that Kuhn's warning has had so little echo. In a comprehensive literature devoted to the presentation and critical examination of his conception of science, the phrase *paradigm* has been and is used in the most varied ways, with the consequences that follow for the understanding of what he actually means by normal science or normal research. I will support this claim by referring to some texts published after 2000.

In a monograph devoted to Kuhn's philosophical work, the well-known philosopher of science Alexander Bird describes normal science as a theory-driven research, a research in which a certain theory is applied, and naturally concludes that it is "... an intellectually uninteresting activity"[25]. The author's tacit assumption is that what is interesting in scientific research are, first and foremost, theories with a high level of generality and a great explanatory power, such as those of Newton's, Maxwell's or Einstein's. Bird illustrates paradigm shifts in Kuhn's sense by indicating discoveries such as Roentgen rays and the theory of the expanding universe[26]. The author further speaks of "small paradigm changes" and "revisions of the paradigm that are not entirely revolutionary", of "moderate changes that revise paradigms, but not in a dramatic way involving crisis, resistance, etc."[27] Based on such considerations, Bird aims to show that there would be in fact no demarcation between the periods designated by Kuhn as normal research, respectively extraordinary research and scientific revolution. And in a contribution to a volume based on the proceedings of a symposium devoted to the 50th anniversary of the first edition of the *Structure of Scientific Revolutions*, Michaela Massimi makes the following observation: "Normal science is characterized by non-critical acceptance of a theory in a community of researchers."[28] Many other references to normal science and paradigms could be cited in writings devoted to the conception outlined in the *Structure of Scientific Revolutions*, references that distort in an often blatant way what I have endeavoured to show that Kuhn had in mind when he introduced and used these expressions.

[24] Th. Kuhn, *The Essential Tension*, pp. 318–319.

[25] Alexander Bird, *Thomas Kuhn*, Chesham, Acumen Press, 2000, p. 38.

[26] *Ibidem*, p. 51.

[27] *Ibidem*, pp. 52–53.

[28] See M. Massimi, "Walking the Time: Kuhn within Realism and Relativism", in William J. Devlin, Alisa Bokulich (eds.), *Kuhn's Structure of Scientific Revolutions - 50 Years On*, Springer, 2015, p. 178.

The second observation is that the last decades of Kuhn's life marked a sharp change in his interests and concerns in the philosophy of science. It is a period in which his concerns were focused on the topic of the incommensurability of scientific research traditions. Kuhn set out to develop this topic using the results of recent research in evolutionary biology, developmental psychology as well as linguistics and philosophy of language. What might explain this reorientation?

First of all, I think, we must keep in mind that the *Structure of Scientific Revolutions* was a work addressed to both historians of science and philosophers of science. However, its echo was relatively small in the former community and very large, as is well known, in the latter. Why? We can assume that what the book advocated was not nearly as novel and surprising to historians of science as it was to philosophers of science. On the other hand, Kuhn was not satisfied with the level of elaboration of the topic of incommensurability of research traditions in the book that made him famous. Naturally, he decided to focus on this topic, a particularly challenging one for philosophers of science. Until the end of his life he worked intensively on a book on this topic, which he never finished. In the manuscript he left behind, scientific traditions are characterized as lexicons, that is, as distinct ways of conceptualizing the data of experience. The structure of the lexicon, which encompasses a variety of object genres and object properties, determines the field of possible experience in a scientific research tradition. Professor Paul Höyningen-Huene provided many important insights and comments on this project, which he characterized as Kantian-inspired[29].

Through this reorientation of concerns, the concept of normal science ceased to occupy in Kuhn's thinking the central position that it held in the *Structure of Scientific Revolutions*. The evolution of Kuhn's thinking and concerns thus marks both continuity and pronounced thematic shifts. It is an evolution that can be compared, to a certain extent, with the evolution of Wittgenstein's thinking. To some topics of Wittgenstein's later thinking, Kuhn was perhaps closer than he realized[30].

Such observations deserve, I think, attention because they can, among other things, explain why the concept of normal science, paradigm-driven research, no longer occupies the place it held in the philosophy of science half a century ago, i.e. after the publication of Kuhn's book. However, the concept retains its relevance and importance for understanding contemporary scientific research in its core areas. This is what the analyses and observations I have made have sought to highlight in the first place.

[29] See, for exemple, P. Höeningen-Huene, *Kuhn's Development after Structure*, in W. J. Devlin, A. Bockulich, *op. cit.*

[30] For an attempt to read *The Structure of Scientific Revolutions* from the perspective of Wittgenstein's later philosophy, see M. Flonta "Cum va fi înțeleasă *Structura revoluțiilor științifice* de către un cititor al lui Wittgenstein" ["How *The Structure of Scientific Revolutions* will be understood by a Reader of Wittgenstein"], *Revista de Filosofie*, July-August 2012, pp. 496–523.