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ARE SCIENTIFIC REVOLUTIONS PREDETERMINED?
CRITICAL APPRAISAL OF WOJCIECH SADY'S *STRUKTURA REWOLUCJI RELATYWISTYCZNEJ I KWANTOWEJ W FIZYCE* (THE STRUCTURE OF THE RELATIVISTIC AND QUANTUM REVOLUTION IN PHYSICS)

Abstract

In his book *Struktura rewolucji relatywistycznej i kwantowej w fizyce* (The Structure of the Relativistic and Quantum Revolution in Physics, 2020), Wojciech Sady presents his vision of the two greatest scientific revolutions in the 20th century. The book provides an illuminating account of the way these revolutions proceeded and strongly supports the thesis that, contrary to Thomas Kuhn's famous suggestions, the revolutions involved no breaches in the continuity in scientific development but progressed in an evolutionary (although swift) step-by-step way, and were products of collective interactive processes in the scientific community rather than individual achievements of geniuses. On the other hand, it makes a number of controversial claims. In this article, I contest Sady's claims that new scientific theories (including the most revolutionary ones) logically follow from the theoretical and experimental knowledge already available (the Entailment thesis) or, at least, their emergence is necessary, inevitable, given the available knowledge, the thought style of the scientific community, and some minimally necessary conditions for the development of science (the Necessitation thesis), and that the role of extra-logical and extra-empirical factors, that can be designated as "creative imagination," in the development of science is either negative or neglectable.

Keywords: scientific revolution, relativity, quantum mechanics, underdetermination, imagination

Wojciech Sady's book *Struktura rewolucji relatywistycznej i kwantowej w fizyce* (The Structure of the Relativistic and Quantum Revolution in Physics, 2020) makes a significant contribution to the philosophical debate on the structure

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and engines of scientific revolutions. The topic was brought to the forefront by Thomas Kuhn in his famous book *The Structure of Scientific Revolutions* (1962), which evoked much controversy that involved other prominent epistemologists, such as Karl Popper (1970, 1976), Imre Lakatos (1970, 1971), Paul Feyerabend (1970), Stephen Toulmin (1970), and others. One of the most common criticisms of Kuhn was that he misrepresents revolutionary transitions in science as a matter of irrational “conversion to the new paradigm,” “a transition between incommensurables,” “the Gestalt switch” (Kuhn 1962: 19, 150). Critics argued that the development of science through revolutions is much more continuous than Kuhn’s account suggests, and that new revolutionary theories and their acceptance by the scientific community are better understood as rational responses to problem situations that arise within “pre-revolutionary” science and involve standards of evaluation widely shared by scientists before and after the revolution. Sady’s account of the two greatest scientific revolutions of the 20th century, Einsteinian relativity and quantum-mechanics, asserts this continuity, I will contend, far too radically. Kuhn’s other critics, while maintaining the unbroken continuity of science through revolutions and rebutting Kuhn’s claim about the “incommensurability” of pre- and post-revolutionary scientific paradigms, usually recognize the significance of extra-empirical and extra-logical factors, such as creativity, imagination, guesswork, in the development of science generally and scientific revolutions especially, and the essential role of outstanding revolutionary scientists, such as Einstein. In contrast, Sady explicitly depreciates these factors, expressly denying that imagination plays a considerable part in scientific revolutions and that persons of scientific geniuses are indispensable. Sady counters accounts that recognize the significance of such non-routine, creative, and personal factors with the claim that “it is impossible to be ahead of one’s time, to fill in the gaps in our knowledge by products of the imagination. In science, including revolutionary periods, one has to move step by step, and always affirm nothing but what follows from the available knowledge and results of experiments” (Sady 2020: 34). Sady asserts that this was exactly the way the relativist and quantum-mechanical revolutions in physics proceeded.

Some claims in the passage quoted above are too vague to be contested. Of course, science develops step by step (regrettably, this tells us nothing as to how big particular steps may be). And surely, there is a sense in which in science “it is impossible to be ahead of one’s time” – for example, Einstein’s relativity theories could not have been produced in Newton’s time, because they were underpinned by knowledge and ideas that were the result of a huge amount of experimental and theoretical work by many scientists over more than two centuries. This was a body of work that would greatly exceed the accomplishments of one scientific genius, or even a group of contemporary scientists. What is far more contestable is Sady's claim that scientists should, and Einstein and the founders of quantum mechanics did, "always affirm nothing but what follows from the available knowledge and results of experiments." Arguably, if "follows" is understood in the strict sense of logical entailment, this claim is straightforwardly false, and I will elaborate on this point in the next section. However, we can interpret Sady's point in a looser sense, so that "follows" can be replaced with something like "necessarily and uniquely comes to the mind of some scientists and gets accepted by the scientific community, given its present style of thinking."[1] In other words, Sady's intended (though not adequately formulated) thesis may be not that new revolutionary theories *logically follow* from the already available scientific knowledge (including the results of experiments) but that they are *uniquely necessitated* by the combination of the style of thinking of the relevant scientific community, the accepted theories, and the known results of experiments, – so that it was necessary that the new revolutionary theories were produced and accepted at nearly that time, and that they had that – rather than some other – content.

Generally, I think that Sady's account can be considered as oscillating between these two theses, stronger and weaker:

> (Entailment) Scientific (revolutionary or not) theories that gain acceptance by the scientific community follow from the available scientific knowledge (theories already accepted and the results of experiments), and so imagination, guesswork, etc. play no considerable role in the development of science.

> (Necessitation) If science progresses, both the emergence and content of new scientific theories (revolutionary or not) are predetermined by the

[1] In his appellations to the *style of thinking* (*thought style*) of the scientific community, Sady follows Ludwik Fleck, a Polish thinker of the first half of 20th century, whose essay *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* (Genesis and Development of a Scientific Fact: Introduction to the Theory of Thought Style and Thought Collective, 1935) anticipated many ideas that were later influentially advanced by Kuhn. (See Kuhn's own comments about the relationship between his and Fleck's ideas in Kuhn 1962: vii; 1979: viii–ix.)

combination of the style of thinking of the relevant scientific community, the theories it already accepts, and the results of experiments.

In the rest of this article, I will argue that the stronger thesis (Entailment) is false and that Sady's case for the weaker thesis (Necessitation) is contestable.[2]

1. THE FALSITY OF THE ENTAILMENT THESIS

The Entailment thesis resembles the naïve notion (which was highly influential before the 20th century among philosophers and scientists, and it is one that still retains considerable influence among the general public even now) that scientific theories *must* follow (with logical necessity) from empirical evidence, and that genuinely scientific theories – those that deserve the name of science – satisfy this requirement. Especially, Newtonian physics was for a long time widely believed to follow logically from empirical evidence.

Contemporary epistemology abandoned this notion long ago, as it is generally recognized that a scientific theory, which involves universal statements (such as statements of the laws of nature) about all objects of a certain kind (where the number of these objects is potentially infinite), cannot follow logically from any finite set of statements that describe facts about particular objects of this kind (such as statements about the results of observations, which are always about particular events that happen at particular places at particular times).

In particular, it was argued as long ago as the first half of the 18th century, by David Hume, that facts about the past reveal nothing about the future or, more generally, that facts about events that were observed do not reveal anything about events that have not been observed as yet. No matter how many white swans we observe (even if we didn't happen to observe swans of other colors), this does not mean that all swans are white, or even that it is more probable that the next swan we observe will be white rather than that it will be blue. No matter how many bodies observed by scientists were attracted to one another with a force that was proportional to their masses and inversely proportional to the squared distance between them (Newton's law of universal gravitation), it still does not follow that all other bodies always, or even most of them for most of the time, will be attracted to one another with such a force.

[2] For a review of other main points of the book, see (Łukasik, Gileta, and Kozera 2022).

It is perhaps even more significant for our discussion, that other formulas are logically possible that would give, within the attainable precision of measurements, *the same predictions with respect to all the cases so far investigated*, although different predictions in some other cases (those not investigated so far). And this holds for all statements that are accepted as statements of the laws of nature in science. There is a well-known simple illustration of this logical situation: if we mark all results of observations relevant to a certain problem as points on a graph, then – no matter how many points there are and how they are located – it is possible to draw an infinite number of different curves through all these points. Every such possible line represents a logically possible scientific theory (law of nature) that entails (in conjunction with the respective initial conditions), as predictions, all these results of observations. Because all these alternative theories are consistent with all of the available evidence and entail it, none is entailed by (follow from) this evidence. This is a trivial logical fact known as the *underdetermination of scientific theory by evidence*.

It can also be illustrated with a paradigmatic example from the history of science: all evidence that can be considered as the "empirical basis" of Newton's physical theory (observable facts that this theory *successfully* predicts/entails under the respective initial conditions, with very high precision), stands in the same relationship with Einstein's theory. (And surely, other theories that stand with this evidence in the same logical relationship are logically possible.) If Newton's theory follows from this evidence, then Einstein's theory follows from it too; however, they cannot both follow from it, because they are alternative (mutually contradicting) theories. Hence, Newton's (or Einstein's, or whatever) theory does not follow from this evidence.

Sady claims that the underdetermination of scientific theory by evidence has no bite against his conception because, on this conception, new theories follow not from empirical evidence alone but *from the conjunction of empirical evidence and scientific theories that are already accepted by the relevant scientific community* (Sady 2020: 32–34). To remind, his imperative for science was "always affirm nothing but what follows from the available knowledge and results of experiments" (Sady 2020: 34). Sady illustrated it by the example of Coulomb's discovery of the law that determines the force of interaction between two electrically charged particles at rest (Coulomb's law). He claims that the following logical relationship holds:

If

> (1) Newton's three laws are true,
>
> (2) in Coulomb's experiments, the moment of force is proportional to the angle of the torsion of the wire,
>
> (3) electric charges accumulate on (electroconductive) balls,
>
> (4) connectors are made from an isolator,
>
> (5) the charges do not change,
>
> (6) no other (unknown) factors influence significantly the results of the experiment
>
> then: in the performed series of measurement, the force of interaction between the charges changes inversely in proportion to the squared distance between them (Sady 2020: 32–33).

Regrettably, Sady only *asserts* this but does not show how the conclusion can be derived, *in a logically valid way*, from the premises. In fact, it obviously *does not follow* from them, because the premises (1)–(6) do not even include the results of Coulomb's experiment. (If (1)–(6) entail the conclusion, then the experiment would be superfluous!) The results of the experiment were the values of force (or some proportional magnitudes) for different distances between the charges. These results can be explained (deduced as predictions) on the basis of the supposition that there is a law of nature on which the force of interaction between electric charges is inversely proportional to the squared distance between them (Coulomb's law). However, if we recall the graphical illustration of the underdetermination of scientific theory by evidence, this means only that if the results of the experiment are marked on the graph (where distance, L, is measured along the horizontal axis and force, F, along the vertical axis) by points, these points can be joined by a parabolic line $F=kL^2$ (where k is a constant). However, as we have already noted, *infinitely many different curves* can be drawn through any finite multitude of points. Obviously, in the case of Coulomb's experiments, a parabola is *the simplest* such line, but it is *not the only logically possible*. So, despite Sady's claim, Coulomb's law follows neither from the premises (1)–(6) nor even from the conjunction of these premises with the observed results of Coulomb's experiment. Rather, it is the simplest hypothesis from which, in conjunction with (1)–(6) and the respective initial conditions, the results of Coulomb's experiment follow as predictions.

It should also be noted that the example with Coulomb's law is not quite appropriate for a discussion about scientific revolutions, such as the relativity (Einsteinian) and quantum mechanical. The discovery of this law hardly counts as a scientific revolution comparable with Einsteinian relativity and quantum-mechanical ones. Unlike the latter, it didn't require revision of the accepted scientific theories (in particular, Newton physics) but only supplemented them. In contrast, Einsteinian relativity and the quantum mechanical revolutions involved *large-scale revisions of the theories so far accepted, and a renunciation of many of the ways scientists were accustomed to understanding reality, – eventually, the supersession – rather than complementation – of the preceding (Newtonian) theoretical system*.

2. REMARKS ON IMAGINATION, GENIUSES, AND MIRACLES

One thing I find wrong with Sady's account is his derogatory treatment of imagination and the insinuation that the recognition of its role in scientific discoveries is tantamount to a sort of mysticism and susceptibility to "the myth of the genius." Sady dubs is a romantic view of science that overlooks continuity and gradualness in the development of science (especially, scientific revolutions), strong dependence of new scientific theories on the preceding accomplishments, and envisions scientific revolutions as the result of entirely new ways of seeing things, Kuhnian "Gestalt switches," initiated by geniuses.

Sady describes the view he opposes:

> new hypotheses and theories do not appear as conclusions of deductive, inductive, analogical, abductive, or any other reasoning. There is no logic of discovery. It is rather that talented – and especially genial – theoreticians, in the flights of creative imagination insusceptible to logical reconstruction, make a leap from the problematic situation in which the scientific discipline finds itself to new hypotheses and theories. (Sady 2020: 31)

As examples of this view, Sady quotes Karl Popper (1934) and Carl Hempel (1966). However, Sady's description misrepresents the point of the quoted statements (see below). In fact, they are concerned with the logical underdetermination of scientific theory by evidence but have little, if anything, to do with "the myth of the genius" and "flights."[3]

To begin with, there is nothing extraordinary, or mystical, or essentially genius-involving, about creative imagination. Imagination is creative, and it is a perfectly everyday capacity of human beings. What do epistemologists like Popper and Hempel mean when they describe scientific discoveries as products of imagination? Not much. Hardly anything more than that new scientific theories do not follow logically from (are not logically entailed by) the available data (the underdetermination thesis), or generally from the knowledge already available. The point of Popper's and Hempel's statements quoted by Sady is primarily (and with Popper entirely) about this rather than about geniuses:

> the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man . . . may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. . . . there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains "an irrational element," or "a creative intuition," in Bergson's sense. In a similar way Einstein speaks of the "search for those highly universal laws . . . from which a picture of the world can be obtained by pure deduction. "There is no logical path," he says, "leading to these . . . laws. They can only be reached by intuition, based upon something like an intellectual love ('Einfühlung') of the objects of experience." (Popper 1959: 31–32)

> There are, then, no generally applicable "rules of induction," by which hypotheses or theories can be mechanically derived or inferred from empirical data. The transition from data to theory requires creative imagination. Scientific hypotheses and theories are not derived from observed facts, but invented in order to account for them. They constitute guesses at the connections that might obtain between the phenomena under study, at uniformities and patterns that might underlie their occurrence. (Hempel 1966: 15)

In other words, Popper and Hempel negate the Entailment thesis, and, as I argued in the preceding section, Sady's attempt to defend this thesis fails (and it involves confusion with the Necessitation thesis, which is relevantly different).

The point of Popper's and Hempel's references to "creative imagination" is the same as with "conjecture" or "guesswork." Because new scientific theories are not

[3] Similarly, Mateusz Kotowski makes a case that "Sady's argumentation against the thesis of underdetermination is tantamount to attacking a straw man" (Kotowski 2021: 69).

logically deducible from the knowledge already present, they are *genuinely new* ideas – there is no logic or algorithm for their generation. All we can – and all we need – to say about how they are produced is that scientists try to solve scientific problems (of course, in the light of the available scientific knowledge, as well as the rest of their background knowledge, and background capacities),[4] and some new ideas as to how this can be done come to their minds, and some of them turn out felicitous (while many more turn out inept). Some of these may be as simple as Coulomb's conjecture that the results of his experiments are best explainable by the law on which force is inversely proportional to squared distance, some others (perhaps those involved in Einstein's special and general relativity) can be pretty much more unusual and inventive – we can hardly specify just how much more. In this sense, as Hempel suggested, they can indeed "require great ingenuity,[5] especially if they involve a radical departure from current modes of scientific thinking, as did, for example, the theory of relativity and quantum theory" (Hempel 1966: 15).

Similar criticisms of Sady's treatment of imagination, with a somewhat different emphasises, were advanced by Andrzej Łukasik, Marcin Gileta, and Sebastian Kozera (2022) and Łukasz Mścisławski (2021). Łukasik, Gileta, and Kozera (2022: 228) complain that Sady understands the term "imagination" "very narrowly, namely as a definition of a visual, imaginative, mechanical model of phenomena," and suggest that the role of imagination in science is far more important if we understand it, "more broadly, without limiting the scope of this term to a purely mechanical picture of phenomena" – there are many ideas advanced by physicists that, "have nothing to do with presenting mechanical models of phenomena, but play an important heuristic role in theory-building, ahead of any equations and experimental results." They especially emphasize the role of thought experiments, which are "processes carried out only in the imagination," in physics. Mścisławski proposes to distinguish "free products of fantasy" (that figure in Sady's derogatory examples) from "the specifically understood imagination, which seems to be necessary when practicing a given discipline, a kind of feeling or intuition" (Mścisławski 2021: 62). It seems to me that Popper and Hempel, when talking about the role of imagination in scientific discovery, meant the latter rather than the former (at least, this construal fits well with the quotations to which Sady

[4] On "background capacities," see (Searle 1992).

[5] In the Polish translation by Barbara Stanosz, quoted by Sady (2020: 31), "ingenuity" is translated as "wyobraźnia," which is the Polish equivalent of "imagination."

refers, reproduced above), so Sady's criticism of their views seems to be attacking a straw man.

3. DOUBTS CONCERNING THE NECESSITATION THESIS

The Necessitation thesis is more plausible than the Entailment thesis, and it better fits Sady's appeal to the style of thinking of the relevant scientific community (which, presumably, is not reduced to any set of theoretical and experimental premises).

If we recall, the Necessitation thesis is that in a mature science in progress, revolutions are necessarily determined by the combination of the available scientific knowledge (theoretical and experimental) and the style of thinking of the scientific community, so that the revolution should have happened at nearly the same time and should have had essentially the same content. The problem situation and the thought style of the thought community necessitated just this – rather than some – revolution, and no alternative possibilities of development (short of stagnation or decay if external conditions were too bad) are ever open. If Einstein had not been there, some other scientist, or a group of scientists, would have arrived, not much later, at the same solution to the problem situation, and this solution would have been accepted by the scientific community. The problem situation and the style of thinking of the scientific community were ready for this solution, so that a scientist or scientists should have come across it, and they were ready only for this solution, so that no scientist could have come across any other satisfactory (from the point of view of the thought style of the scientific community) solution.

Prima facie, Sady's analysis provides a weighty support for this view or makes it plausible. Sady reconstructs the internal scientific context (relevant preceding achievements, discovered anomalies, developed means of research and analysis, debated ideas and tentative solutions, etc.) of the two greatest scientific revolutions in physics of the 20th century, so that they appear as the natural and expected development of the process of scientific research and debate rather than Kuhnian Gestalt switches and irrational conversions. From the perspective of this reconstruction, it seems that the solutions that were eventually proposed by Einstein and the founders of quantum mechanics were as if hovering in the intellectual air and could not fail to be discovered by someone.

However, I think that we better be cautious about the Necessitation thesis. The retrospective appearance of the process that has already happened as not merely natural (such that is not some miracle but is well understandable if we know enough about the relevant aspects of the preceding development) but necessary (so that there was no alternative possibilities of development) can be a hindsight illusion produced by the combination of our knowledge of the solution proposed and accepted with our inability to imagine another possible solution, – the inability due to the fact that in order to imagine a solution, one needs to produce it, and in order to produce a solution alternative to and as good as that proposed in fact by Einstein (or the founders of quantum mechanics collectively) one needs to be at least as competent and resourceful and lucky as Einstein was (or the founders of quantum mechanics collectively were). On the one hand, because we (unlike physicists on the eve of the revolutions at issue) know the content of the revolutionary theories proposed and accepted, and are accustomed to them, we can (and Sady successfully did) reconstruct the problem situation and aspects of scientific debate, so that in the light of this reconstruction these theories seem natural (and not so revolutionary) solutions. On the other hand, because *we do not know other possible alternative solutions, and have no idea as to how the pre-revolutionary problem situations could have been solved satisfactorily otherwise*, so *it seems to us that they couldn't possibly be solved otherwise*. However, if we make "the logic" of this quasi-reasoning explicit, –

(1) I do not know an alternative solution to a certain problem.

(2) I cannot propose such a solution.

Hence, an alternative solution is impossible.

– we can see that it is invalid.

Moreover, we have good reasons to think that unknown alternative possible solutions do exist *as a matter of logic*, whether or not they are *possible for* the scientific community of the time, given its thought style. In the 18th and 19th centuries, successful solutions for problems in physics were found within the framework of Newton's theoretical system. However, when this system was superseded by Einstein's, it turned out that the same problems found their successful solution within Einstein's theoretical system just as well. And we have no reason to think that Einstein's system, unlike Newton's, cannot be superseded.[6] Now if

[6] On the contrary, during two centuries before the end of the 19th century, people had much more reason to believe that Newton's theory is absolutely true than we now have to think this of Einstein's – or any other – theory. As Karl Popper pointed out, Newton's theory was the most successful theory in

Newton's theory was not the only *logically possible* solution for those problems, then there is no reason to think that Einsteinian relativity or quantum mechanical theories are the only logically possible solutions to the problems that gave rise to them.[7] Admittedly, Einstein's solution could not be produced by scientists of Newton's times (much work had to be done during two centuries by many scientists to prepare the path for this solution). And it may even be the case that no other logically possible solution, except the one proposed by Newton, could have been proposed at that time and recognized by the scientific community as satisfactory. However, we do not know this for sure. Perhaps we just fail to imagine how physics could have developed otherwise because we have no idea about the alternative theory that would guide this development. Likewise, it is possible that at the beginning of the 20th century, the thought style of the scientific community of physicists made only one solution to the extant problem situation feasible – the one of Einsteinian relativity and the quantum-mechanical theoretical systems. But it is also possible that some other solutions were feasible; we just do not know what these solutions were. Perhaps, if Einstein had not proposed the solution he in fact proposed, someone else somewhat later (or perhaps even considerably later, so that the thought style of the scientific community would undergo some relevant change, with some new knowledge and conceptual tools developed in the meantime) would have proposed some other solution that the scientific community would have accepted and that would have directed further scientific research in a somewhat different direction than Einstein's theories did.[8]

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the history of science, and if it was eventually superseded by Einstein's theory, then this can happen with any scientific theory.

[7]See (Stanford 2001: S9) for a more general argument that "we have, throughout the history of scientific inquiry and in virtually every scientific field, repeatedly occupied an epistemic position in which we could conceive of only one or a few theories that were well-confirmed by the available evidence, while the subsequent history of inquiry has routinely (if not invariably) revealed further, radically distinct alternatives as well-confirmed by the previously available evidence as those we were inclined to accept on the strength of that evidence. . . . Thus, the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically are alternatives to our best theories equally well-confirmed by the evidence, even when we are unable to conceive of them at the time."

[8]As for quantum mechanics, there is an interesting question about the significance of the dominance of the Copenhagen interpretation for its development. James Cushing makes the case that this domination was historically contingent in the sense that "an entirely plausible reordering of historical factors could reasonably have resulted in the causal program [i.e., David Bohm's interpretation] having been chosen over the Copenhagen one" (Cushing 1994: xi). Although these two competing programs (interpretations) are "observationally equivalent," we can wonder whether the dominance of Bohm's program (or perhaps some other observationally equivalent, so far as present

The Necessitation thesis gets some support from the historical cases when important scientific discoveries were made independently and almost simultaneously by two scientists, – for example, the invention of differential calculus by Newton and Leibniz, or the discovery of electromagnetic induction by Michael Faraday, Francesco Zantedeschi, and James Clerk Maxwell. This seems to show that *at least sometimes* important scientific discoveries are necessary products of preceding development rather than contingent products of individual minds of geniuses-scientists. However, the question is open whether this is always (rather than only sometimes) the case, in particular, whether this holds for such scientific revolutions as the Einsteinian and quantum-mechanical ones.

The possibility of alternative revolutionary developments in science (when some problem situation serves as sort of a "bifurcation point," from which the movement is possible in somewhat different directions) is intelligible from the point of view of fallibilistic epistemological realism with respect to the purposes and capabilities of science. Epistemological realism generally sees the purpose of science in terms of truth as a correspondence between scientific theories and reality. From this point of view, scientific progress should consist either in the discovery of new truths (including the replacement of false theories with new ones) or in getting closer to the truth (discovery of new theories that in some sense approximately correspond to reality in some important areas or replacement/supersession of theories that are farther from the truth with theories that are closer to the truth). *Fallibilistic* epistemological realism (such as Popper's, for example) takes the second option – seeing the purpose of science as getting nearer to the truth rather than achieving it. It learns from the history of science, especially that of physics in the 20th century, that we have no reasons to hold that the presently accepted scientific theories are true in the strict sense (absolutely true): if such a hugely successful theory as Newton's has eventually turned out false (in the strict sense), we have no reason to think that Einstein's theory, or any other presently accepted scientific theory, is absolutely true; however, the predictive and explanatory successes of Newton's theory provide good reasons to consider it as a good approximation to the truth in the area where it succeeds, and the successes of Einstein's theory in an even wider area provide good reasons to consider it as an even better approximation to the truth. However, if we abandon the idea that the presently accepted scientific theories are absolutely true and

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knowledge is concerned, program) could influence the direction of research and, accordingly, the further development of quantum mechanics in the 20th and 21st centuries.

rather consider them as approximations to the truth, then we should allow for the possibility of alternative theoretical approximations to the truth – theories that near the truth, so to say, from different directions.

Sady, like Fleck and Kuhn, sees the development of science from a different perspective, as determined by the thought style of the scientific community, whether or not it has something essentially to do with truth (correspondence to reality). However, from this perspective, we have even fewer reasons to rule out the possibility of alternative developments. There is no reason to consider thought styles as rigid deterministic machines. There are no principal reasons why the same thought style cannot be comfortable with two or more alternative theories, or why it cannot develop in different directions. And accidental factors, including intellectual creativity (imagination, guesswork) of individual scientists, can play a considerable role. (Unlike this perspective, epistemological realism imposes certain restrictions – although different developments are intelligible, they should converge to the truth. No such convergence requirement is inherent in the conception of development directed by thought styles.)

The general conclusion seems to be that it remains an open question, how much scientific revolutions depend on individual outstanding scientists and whether or not they are rigidly predetermined, both in their occurrence and in their contents, by the preceding scientific development – whether or not the preceding development allows some possible alternatives to the revolutions that actually occur. At the same time, Sady's work shows persuasively that a scientific revolution is a product of collective work of many scientists rather than of an individual genius (such as that of Newton or Einstein) to a much greater extent than the dominant popular notions imply. Thus, the book considerably enriches our understanding of the development of science and stimulates further fruitful philosophical ideas and debates about scientific revolutions.

REFERENCES

Cushing J. (1994), *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, Chicago–London: University of Chicago Press.

Feyerabend P. (1970), "Consolations for the Specialist" [in:] *Criticism and the Growth of Knowledge*, I. Lakatos, A. Musgrave (eds.), Cambridge: Cambridge University Press, 197–230. https://doi.org/10.1017/CBO9781139171434.010

Fleck L. (1935), *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv*, Basel: Beimo Schwalbe & Co.

Hempel C. (1966), *Philosophy of Natural Science*, Englewood Cliffs, N. J.: Prentice-Hall.

Kotowski M. (2021), "Czy teza o niedookreśleniu teorii przez dane doświadczenia czyni sukcesy nauki cudem?," *Studia Philosophica Wratislaviensia* 16(3), 69–74. https://doi.org/10.19195/1895-8001.16.3.8

Kuhn T. (1962), *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press.

Kuhn T. (1979), "Foreword" [in:] L. Fleck, *Genesis and Development of a Scientific Fact*, Chicago–London: University of Chicago Press, vii–xi.

Lakatos I. (1970), "Falsification and the Methodology of Scientific Research Programmes" [in:] *Criticism and the Growth of Knowledge*, I. Lakatos, A. Musgrave (eds.), Cambridge: Cambridge University Press, 91–196. https://doi.org/10.1017/CBO9781139171434.009

Lakatos I. (1971), "History of Science and its Rational Reconstructions" [in:] *PSA 1970: In Memory of Rudolf Carnap Proceedings of the 1970 Biennial Meeting Philosophy of Science Association* (*Boston Studies in the Philosophy of Science* 8), R. C. Buck, R. S. Cohen (eds.), Dordrecht: Reidel, 91–135. https://doi.org/10.1007/978-94-010-3142-4_7

Łukasik A., Gileta M., Kozera S. (2022), "Wojciech Sady: The Structure of the Relativity and Quantum Revolution in Physics," *Journal for General Philosophy of Science* 53, 223–229. https://doi.org/10.1007/s10838-021-09597-9

Mścisławski Ł. (2021), "Między geniuszem, rewolucją i matematyką," *Studia Philosophica Wratislaviensia* 16(3), 59–67. https://doi.org/10.19195/1895-8001.16.3.7

Popper K. (1934), *Logik der Forschung*, Vienna: Springer Verlag. https://doi.org/10.1007/978-3-7091-4177-9

Popper K. (1959), *The Logic of Scientific Discovery*, London: Hutchinson and Company. https://doi.org/10.1063/1.3060577

Popper K. (1970), "Normal Science and Its Dangers" [in:] *Criticism and the Growth of Knowledge*, I. Lakatos, A. Musgrave (eds.), Cambridge University Press, 51–58. https://doi.org/10.1017/CBO9781139171434.007

Popper K. (1976), "The Myth of the Framework" [in:] *The Abdication of Philosophy: Philosophy and the Public Good: Essays in Honor of Paul Arthur Schilpp*, E. Freeman (ed.), La Salle: Open Court, 23–48. https://doi.org/10.1007/978-94-009-3779-6_2

Sady W. (2020), *Struktura rewolucji relatywistycznej i kwantowej w fizyce*, Kraków: Universitas.

Searle J. (1992), *The Rediscovery of the Mind*, Cambridge, Mass.: The MIT Press. https://doi.org/10.7551/mitpress/5834.001.0001

Stanford P. K. (2001), "Refusing the Devil's Bargain: What Kind of Underdetermination Should We Take Seriously?," *Philosophy of Science* 68(3), S1–S12. https://doi.org/10.1086/392893

Toulmin S. (1970), "Does the Distinction between Normal and Revolutionary Science Hold Water?" [in:] *Criticism and the Growth of Knowledge*, I. Lakatos, A. Musgrave (eds.), Cambridge: Cambridge University Press, 39–48. https://doi.org/10.1017/CBO9781139171434.005