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ЗБІРНИК НАУКОВИХ ПРАЦЬ

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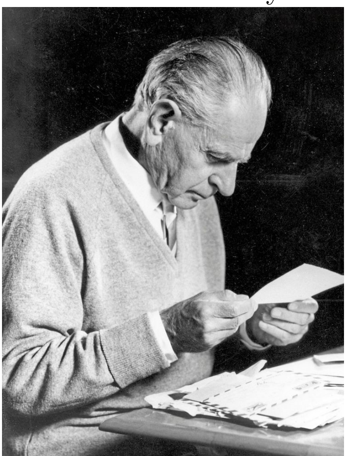

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# Зміст

# Popper's and Lakatos's philosophies of science: main differences

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**Abstract**. The article compares Karl Popper's and Imre Lakatos's conceptions of the development of scientific knowledge. It is argued that the central difference between these conceptions relates to Lakatos's thesis about the structure of scientific research programs as formed by a «hard core», which is «irrefutable» due to the methodological decision of the program's proponents, and a «protective belt» of auxiliary hypotheses. From the standpoint of Popper's critical rationalism, this thesis is unacceptable because it relegates critical discussion to the periphery of scientific activity; moreover, as Alan Musgrave and William Berkson have argued, the thesis is refuted by the history of science. The second important difference between Popper's and Lakatos's conceptions has to do with the definitive characteristics of empirical science (the «demarcation criterion»). An analysis of the debate over this issue suggests that a softened version of Popper's demarcation proposal is preferable. This version takes into account Lakatos's valid point that falsification depends on the emergence of better theories but repudiates his endorsement of «immunization» (against possible falsification) of the core theory of a research program. An interesting difference between Popper's and Lakatos's views concerns the issue of the possibility of a genuine logic of scientific discovery. However, on the one hand, Lakatos's ideas in this regard were not sufficiently developed and clearly articulated in his published works, and on the other hand, attempts to develop these ideas by Elie Zahar and John Worrall demonstrate only the possibilities of rational historical reconstruction of scientific discoveries already made, and do not form a general «logic of scientific discovery» that could guide scientists in producing new discoveries. A number of other apparent divergences between Popper and Lakatos are rather differences in emphasis and focus of interest, and (as John Watkins shows) of Lakatos's artificial and sometimes spurious contrasting of his ideas with Popper's views.

**Keywords**: rational criticism, falsification, scientific research program, demarcation, empirical science, logic of scientific discovery.

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## Introduction

Karl Popper and Imre Lakatos were two of the most distinguished philosophers of science who presented encompassing views on its character and development. Comparing these views, singling out their main differences and analysing arguments that were, or could be, made for each side is still likely to be useful in improving our understanding of science and rationality. In this article, I propose an analysis of several differences that are likely to be important.

The relationship between Popper's and Lakatos's philosophy of science is rather complicated and, one might say, bifacial.

Lakatos began his philosophical career in the West after emigrating from communist Hungary as a follower of Popper and a prominent representative of his school. His first work, which earned Lakatos the reputation as an original and prolific philosopher, was titled *Proofs and Refutations: The Logic of Mathematical Discovery.* The title is a combination of modified titles of two Popper's books, *Conjectures and Refutation* and *The Logic of Scientific Discovery.* In this work, Lakatos advanced and explained a vision of the development of mathematics that is pretty much like Popper's vision of the development of empirical sciences. Popperians usually consider this as the most important genuine philosophical achievement of Lakatos.

However, Lakatos's later works parted their path with Popper's philosophy in a number of important respects. In particular, this applies to his most famous work, *Falsification and the Methodology of Scientific Research Programmes.* In some important respects, this work resonates with Thomas Kuhn's *The Structure of Scientific Revolutions.* In particular, Lakatos's concept of a research program is pretty similar to Kuhn's concept of normal science, and Lakatos's concept of «the hard core» is similar to Kuhn's concept of paradigm. However, there are significant differences as well. First, in the perspective of Lakatos's conception, pluralism and competition between different research programs with different hard cores is a normal state of science, while Kuhn claimed that pluralism of paradigms and competition between them occur only in periods of scientific revolutions. Secondly, while Kuhn described the transition between paradigms as an essentially irrational process similar to religious conversion (because the paradigms are «incommensurable»), Lakatos presented the decline of some research programs and the rise of others as having a rational basis in the productivity and success of their empirical predictions. It can be said that Lakatos's conception of the development of science is midway between Popper's and Kuhn's conceptions, and Lakatos believed that he had synthesized the rational aspects of both of these.

One of the central theses of the Lakatos's conception is about the structure of a scientific research program – that it consists of a permanent «hard core» and a modifiable «protective belt». This is, I suggest, the hub of Lakatos's divergences with Popper. It is also essentially connected with another important, and more specific, discord – about the distinctive character of empirical science, or criteria of demarcation between what is, and what is not, empirical science. Yet another interesting difference, although not articulated clearly in Lakatos's published works but emphasized by his pupils Elie Zahar and John Worrall, is concerned with the possibility of a genuine logic of scientific discovery. Other apparent divergences seem to be less important and probably merely apparent – differences in emphasises and focuses of interest rather than in substantial matters.

## 1. Unlimited critical attitude *versus* immunisation of central theories. How hard are «hard cores» of scientific research programs?

Karl Popper argued that the basis of science is the rational critical attitude – the willingness to learn from arguments and experience, to detect and eliminate mistakes in our theories, to recognise that any of them may be mistaken (the principle of fallibilism), and the readiness to abandon or revise any of them if arguments and experience show that it is mistaken. Considering in this perspective Lakatos's conception of the methodology of scientific research programs, we have to note that it radically limits this rational-critical attitude and relegates it to the periphery of scientific activity. Centrally, this is manifested by Lakatos's thesis that scientific research programs have «hard cores» that are immunised from possible refutation (and so held beyond criticism) with the help of «protective belt» of auxiliary hypotheses: whenever conflicts arise between the predictions generated by a program and the results of observation, responsibility is put upon some auxiliary hypotheses. In Lakatos's own formulation, the «hard core» «is "irrefutable" by the methodological decision of its proponents: anomalies must lead to changes only in the "protective belt" of auxiliary, "observational" hypotheses and initial conditions» [7, p. 133].

This claim is very objectionable (and was much criticised), both in factual and normative respects.

To begin with, how the methodological decision is supposed to be taken? Surely, it is not the case that scientific research program is a (church-like) organisation with a ruling body that takes decisions (about dogmas that should be held sacred) that are obligatory to the members of the organisation, under the threat of expulsion and harassment. It also cannot be the case that in fact, each scientist who at a time happened to adhere to a program decides individually to hold a certain set of positions come what may, and does carry out this decision, and it so happens that all adherents of the program choose for this purpose the same set of positions, which by this token qualifies as the «hard core» of the program. If we construe Lakatos's claim as normative rather than factual – that scientists *ought* to do this, supposedly because it is conductive to scientific progress – it is also clearly wrong. Indeed, if all scientists took this course, then for a once successful and universally accepted research program (such as Newton's physical system), it would be impossible that any scientist abandoned this program and founded an alternative research program (as Einstein, for example, did). As a last resort, it may be proposed that Lakatos should be understood in the sense that if a scientist abandons some of «core» tenets of a research program, he or she, by that very token, drops out of the program. However, this would make Lakatos's claim an empty tautology – a matter of an arbitrary definition of «research program» that has no bearing on reality at all.[1]

Lakatos's claim about the irrefutability of «hard core» was amply criticised by Alan Musgrave [13, p. 457-467] and William Berkson [6, p. 52-53]. Musgrave argues that «if this is meant as a *historical* claim, it is refuted by what Lakatos himself calls "the classical example of a successful research programme", while if it is meant as a piece of *advice* to scientists, it has obvious defects» [13, p. 457]. In particular, in the cases of the two most important «anomalies» for the history of the Newtonian research program

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[1] Cf.: Alan Musgrave:

> It might be objected that Clairault, Euler, Lagrange, Airy and Bessel, in contemplating modifications to Newton's law of gravity, merely showed themselves not to be true Newtonians ... But this would turn Lakatos's historical claim «Newtonians rendered Newton's theory unfalsifiable by fiat» into a tautology: «Newtonians (that is, those who refused to amend Newton's laws) refused to amend Newton's laws» [13, p. 460].
>
> As John Watkins pointed out to me, Lakatos's views risk falling into the following circularity: the «hard core» of a research programme consists of those propositions held irrefutable by its adherents, and an adherent of a research programme is one who holds its hard core irrefutable [13, p. 483].

– first, of the orbit of Uranus and second, of the perihelion of Mercury – there were several respected «Newtonian» scientists who tried to solve the problem by making changes in the very «core» of Newton's theory, his law of gravitation. Berkson is even more scathing:

> The notion of a «hard core» does not in general find any substantiation in the history of science... [2] Every scientist had his own «hard core», and some times more than one, which shows that the «cores» are not exactly hard in that they were protected from change at almost all cost. Scientists actually ... have had enough intellectual independence to make up their own minds about what to take as fundamental, and enough independence to *change* their minds also, or to keep an open mind and try different alternative ideas as fundamental [6, p. 52].

## 2. The Popper-Lakatos demarcational debate[3]

Another important disagreement between Popper and Lakatos was about what is distinctive about empirical science – how can it be demarcated from the rest, especially pseudoscience. This issue was a matter of an explicit debate between Popper and Lakatos in the collection *The Philosophy of Karl Popper* (ed. by Paul Schilpp). Among other things, this collection contains Lakatos's article *Popper on Demarcation and Induction*, which criticises some aspects of Popper's philosophy of science, and Popper's response. Most of this debate is concerned with Popper's demarcation proposal. Before analysing the debate, let us get some things clear about the meaning of this proposal.

More often than not, Popper's demarcational proposal is described inaccurately, as though Popper proposed falsifiability as a criterion for demarcating between science and non-science, or «metaphysics», or pseudoscience. Such a description is inaccurate because the word «science» can be used in different ways, broadly or narrowly. For example, is mathematics a science? Are disciplines usually referred to as «social sciences» sciences? Obviously, it depends on what we mean by «science», and there is a pretty common use/meaning of the word «science» according to which mathematics qualifies as a science, and a

[2] Berkson refers to his book *Fields of Force: The Development of a World View from Faraday to Einstein* [5], as one in which he had shown for the development of field theory in the 19$^{\text{th}}$ century that Lakatos's notion of a «hard core» of a research programme, as well as Kuhn's notion of «paradigm» on which all scientists agree, is historically incorrect [5, p. 355; 6, p. 52].

[3] This section is based on the article [23], in which the demarcational debate between Popper and Lakatos is discussed in more details.

pretty common use/meaning that subsumes «social sciences». Popper was not concerned with the correct definition of the term «science» (he often stated his negative opinion about such disputes about words as wrong-headed and fruitless); he was trying to solve an entirely different problem. Popper explained that his problem was «one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method – that is to say, a method which, although it appeals to observation and experiment, nevertheless does not come up to scientific standards» [17, p. 33-34]; it «was from the beginning ... an urgent practical problem: under what conditions is a *critical appeal to experience* possible – one that could bear some fruit?» [20, p. 174]. Accordingly, in *The Logic of Scientific Discovery*, where Popper advanced his demarcational proposal for the first time, he described the demarcation problem as «the problem of finding the criterion of the empirical character of science» [16, p. 34], and falsifiability as «a criterion for the empirical character of a system of statements» [16, p. 66]. Similarly, in *Open Society and Its Enemies*, Popper explained that he «proposed ... that we solve the problem of demarcation by using falsifiability or testability, or degrees of testability, as criterion of the empirical character of a scientific system» [15, p. 283].

As we can see, the issue is not so much about answering the question «What is science?» as about the genuinely empirical character of theories (statements), or genuinely empirical method, as opposed to pseudo-empirical method – about *the way of critical appeal to experience for testing our theories.*

As is well known, Popper proposed to solve this problem on the basis of such a criterion as falsifiability – that is, the potential possibility of empirical refutation, due to the conflict between the predictions made on the basis of the theory at issue and the actual results of observations in the corresponding (most often, experimental) situations. This demarcational proposal has two aspects: logical and methodological.

The logical aspect is that the theory, in conjunction with the specification of initial conditions, should entail predictions that may contradict observed events (in particular, results of experiments).

However, this logical aspect is not sufficient, because of a problem known as the Duhem problem. The problem is that predictions that can be compared with results of observation do not follow directly from the theory itself or (in Lakatos's terms) from the «core» theory such as Newton's system of laws. To derive such predictions in a logically correct way, auxiliary assumptions are needed, such as the specification of initial conditions and the assumption that there are no other relevant factors that have been left unaccounted for. Accordingly, if there is a conflict between a prediction and results of observations, this conflict may be due to the falsity of either the «core» theory or one of these auxiliary assumptions. It may be that the theory is true but some of the initial conditions were incorrectly specified. Or perhaps there is some unaccounted-for and as yet unknown factor that influenced the outcome. This creates the possibility of «immunisation» of a theory – when the blame for *any* conflicts between predictions and observations is placed not on the theory but on auxiliary assumptions, and such conflicts never lead to the recognition that the theory is false, falsified.

Popper discussed this problem and believed that the solution lies in that scientists should follow certain methodological rules preventing such «immunisation». What are these rules and do they really solve the problem?

In *The Logic of Scientific Discovery* Popper gives an explanation that can be formulated as the following rule: it is OK to «save» a theory with the help of a new auxiliary hypothesis only if the resulting theoretical system (the theory together with the auxiliary hypothesis) is more falsifiable than the original system.

We can explain this in more detail by formulating three key conditions:

1) the new system should entail those successful predictions that followed from the old system;

2) the new system should entail those results of observations that were in conflict with the old system;

3) the new system should entail some additional falsifiable predictions.

If these additional predictions turn out to be true, then the new system is to be (provisionally) accepted as true; if not, then it should be recognised as false, falsified.

An exemplary case from the history of science that fits this scheme is the case of the «anomalous» orbit of Uranus, the discovery of which led not to the rejection of Newton's theory but to the discovery of a new planet, Neptune. Scientists found that the orbit of the planet Uranus deviated significantly from the predictions based on Newton's theory and the available knowledge about the celestial bodies of the solar system. Accordingly, scientists were looking for ways to resolve this contradiction. And of course, they were in no hurry to recognise that Newton's theory is false. First, they tried the hypothesis that there is some unknown planet that causes these deviations. Next, they were able to calculate where this planet should be located to cause the observed deviations. And with the help of advanced observational tools, they did indeed find a hitherto unknown planet at that location, which they named Neptune. Thus, the «anomaly» that initially threatened to falsify Newton's theory became its impressive corroboration.

This course of development perfectly fits Popper's recommendations. However, we should ask the question: what if scientists had not been able to come up with this kind of successful auxiliary hypothesis for a long time – decades or even centuries. Should they then have recognized that Newton's theory is false, even if it had been successful in all other cases and if they had no alternative, better theory? How long, in terms of Popper's demarcational proposal, could they have remained faithful to Newton's theory in such a situation, hoping that there would still be some mistake in the auxiliary assumptions, so that this would not undermine the empirical-scientific character of Newton's theory? Twenty, fifty, one hundred, two hundred years? It seems that fixing any such term would be arbitrary.

In fact, this is just what happened in the second most famous classical example of an astronomical anomaly relative to Newton's theory – the case of the precession of Mercury's perihelion. For more than 50 years, scientists could not find an explanation for this anomaly, and this ended with Einstein proposing a new theory, an alternative to Newton's theory, which proved to be more successful, and which scientists, accordingly, accepted, thus recognizing that Newton's theory is false. But what would have happened, and how scientists *should have acted*, if such a better alternative theory was not invented for a very long time?

Popper gives one more explanation of how the requirement of falsifiability of scientific empirical theories should be understood. In *Conjectures and Refutations*, he wrote that «[c]riteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted» [17, p. 38n3]. In other words, for a theory to have an empirical scientific character, scientists must specify in advance the results of observations that they would take as a decisive evidence that the theory is false.

Lakatos described this requirement, «that the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions» as «Popper's basic rule» [8, p. 111; 9, p. 246] and criticized it. Lakatos claimed that

> Popper's criterion ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it. They normally either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain

> the anomaly, they ignore it, and direct their attention to other problems. Note that scientists talk about anomalies, recalcitrant instances, not refutations. History of science, of course, is full of accounts of how crucial experiments allegedly killed theories. But such accounts are fabricated long after the theory had been abandoned. Had Popper ever asked a Newtonian scientist under what experimental conditions he would abandon Newtonian theory, some Newtonian scientists would have been exactly as nonplussed as are some Marxists [10, p. 3-4].

Developing this view, Lakatos writes:

> Contrary to naive falsificationism, *no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory.* But then the distinctively negative character of naïve falsificationism vanishes; criticism becomes more difficult, and also positive, constructive. But, of course, if falsification depends on the emergence of better theories, on the invention of theories which anticipate new facts, then falsification is *not* simply a relation between a theory and the empirical basis, but a multiple relation between competing theories, the original «empirical basis», and the empirical growth resulting from the competition. ... This shows that «*crucial counterevidence*» – or «*crucial experiments*» – can be recognised as such among the scores of anomalies only *with hindsight*, in the light of some superseding theory [7, p. 119-120].

Proceeding from this criticism, Lakatos proposes an alternative explanation as to what distinguishes empirical science from non-science and, generally, how scientific knowledge develops.

According to Lakatos, a theory is scientific if it is part of a research program that produces unexpected predictions. In Lakatos's terms, such a program is called «theoretically progressive». For this program to be progressive generally (not just «theoretically»), it must also be «empirically progressive», which means that its unexpected predictions (at least, their significant part) come true. A research program that produces more *successful* unexpected predictions is more empirically and theoretically progressive and gradually ousts other, degenerating or less progressive, research programs.

Accordingly, a scientific theory that was successful for a long time and accepted as true because it was the «core» of the most progressive research program at the time (as was the case with Newton's theory, for example) is later recognized as false only if another, more successful research program emerges with a different «core» theory (such as Einstein's theory).

Popper rejected Lakatos's criticisms and made a number of objections in his response [18]. I analysed them in details in [23], so here I will just outline my general evaluations and conclusions.

In my view, while Popper's responses point out a number of flaws in Lakatos's argument, they leave at least one key thesis of Lakatos plausible, namely:

> *There is no falsification before the emergence of a better theory.* ... falsification depends on the emergence of better theories, on the invention of theories which anticipate new facts ... But then the distinctively negative character of naïve falsificationism vanishes; criticism becomes more difficult, and also positive, constructive [7, p. 119].

However, Lakatos's statements that were quoted above contain another thesis that, I think, should not be accepted. The thesis is that if the predictions of a theory contradict observed facts, the appropriate reaction of scientists is that they «either invent some rescue hypothesis to explain what they then call a mere anomaly or, if they cannot explain the anomaly, they ignore it, and direct their attention to other problems» [10, p. 3-4].

This claim reflects a key thesis of Lakatos's conception of scientific research programs that we discussed in section 1 – that the «hard core» of a scientific research program «is "irrefutable" by the methodological decision of its proponents: anomalies must lead to changes only in the "protective belt" of auxiliary, "observational" hypotheses and initial conditions» [7, p. 133].

This indicates that Lakatos underestimated the importance of the rational-critical attitude in empirical science and goes too far in endorsing the immunization of the «core» theory. To repeat, if all scientists acted as Lakatos suggests, it would be impossible for a once successful and widely accepted scientific research program (such as Newton's physical system) that a scientist abandons this program and founds an alternative research program (as Einstein did, for example).

Taking this analysis into account, I propose that Popper's demarcational proposal was modified into a softened version that takes into account Lakatos's thesis that «falsification depends on the emergence of better theories» but rejects his immunisational thesis.

In this modified version, the logical aspect of falsifiability is that an empirical scientific theory combined with a specification of known initial conditions (on the assumption that this specification is exhaustive, i.e., includes all relevant factors), should entail predictions that can potentially clash with the results of observation.

The methodological aspect of falsifiability (or genuine empirical method) can be formulated as two rules:

1) If there is a conflict between some intersubjectively verifiable (reproducible) results of observation and predictions that follow from a

theory in conjunction with the specification of known initial conditions, scientists should consider this as a *potential (possible) falsification* of the theory and explore, among other possibilities for resolving the contradiction, the possibility of this theory being false and of replacing it with another theory.

2) There should be no appeal to pseudo-confirmations, such as the capacity of a theory to «explain» events with hindsight. Only the cases of successful *risky* predictions (i.e., predictions that can possibly conflict with the results of observation) based on a theory count as its genuine empirical confirmations, or corroborations.

Note that the rule 2) is very relevant with respect to one of the main motives of Popper's demarcational proposal. According to Popper's own testimony, this proposal largely arose from a sense of the fundamental difference between, on the one hand, the theories and critical attitude of Newton and Einstein and, on the other hand, the theories of Freud, Adler, and Marx and the attitude of their authors and supporters, who saw confirmation in the capability to explain, in terms of these theories, anything.

## 3. Elie Zahar and the nature of divergences between Popper and Lakatos

Elie Zahar suggests that «apart from the important question about the possibility of a genuine logic of discovery, the divergences between Popper and Lakatos reduce in the last analysis to differences in emphasis» [26, p. 21].

This suggestion involves two claims:

1) There was a substantial difference between Popper and Lakatos on the issue of the possibility of a genuine logic of discovery.
2) There was no substantial difference between Popper and Lakatos on other issues.

Let us consider these claims sequentially.

### 3.1. Is a genuine logic of discovery possible?

Many authors remark with irony that in his *magnum opus*, titled (in English edition) *The Logic of Scientific Discovery*, Popper argued that there is no such thing as the logic of scientific discovery. The German title, *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft*, is renderable by English *Logic of Research. On the Epistemology of Modern Natural Science.* That German «Forschung» («research») became «discovery» in the English title seems a curiosity because Popper (following Reichenbach's distinction between «the context of discovery» and «the context of justification») repudiates unequivocally the logical analysis of the process of discovery (as something that happens in the heads of scientists and belongs to «world 2», the domain of psychology) and suggests that epistemology should focus on the appraisal of the theories themselves (as objects of «world 3» of objective knowledge), no matter how they are arrived at.

It seems that Lakatos's view on this matter was very different. His pupils Elie Zahar and John Worrall recollect that he held with them «long, frequent and richly-detailed three-cornered discussions on the logic of scientific discovery» [25, p. 88]. They see his notion of «positive heuristic» as envisaging the program of the elaboration of this logic and work on the development of this program. However, there are serious problems with this.

Firstly, as Worrall remarks, the account of «positive heuristic» in Lakatos's published works is very sketchy and one-sided. Worrall adduces two small passages (six sentences together) as «the sum total of Imre's general remarks about positive heuristic» in Lakatos's main work on the methodology of scientific research programs [25, p. 88]. Moreover, in these fragments «positive heuristic» is described not as tools for *scientific discovery* but as tools for *immunization* of the «hard core» of a scientific research program from possible refutations.

Secondly, the sketchiness of Lakatos's account of positive heuristic is not accidental and remediable – it is rather what should be expected given the place of this concept in the structure of Lakatos's conception of the methodology of scientific research programs. According to Lakatos, positive heuristic is a part of a research program. Hence, every research program has its own positive heuristic. Moreover, these heuristics *are not formulated anywhere explicitly*; rather, they are implicit in the whole of scientific education and work within a research program.

However, because Lakatos's «positive heuristic» is not explicitly singled out from scientific knowledge, there is no reason to assume that it exists at all, as a distinct part of a research program that guides production of discoveries. Rather, such a production is guided by the sum total of the available relevant scientific knowledge, explicit and implicit. Admittedly, *for a particular discovery* it is possible by the hindsight to specify those pieces of scientific knowledge predating the discovery that probably were involved in the discoverer's reasoning, but there is no telling beforehand which pieces will be needed for a new discovery.

John Worrall contrasts Popper's account of scientific discoveries as originating in conjectures with the Lakatosian account, especially as developed by Elie Zahar: «scientists can be seen as *arguing to*, rather than simply 'conjecturing', theories» [25, p. 88]. I think that this contraposition is misleading. It is unlikely that Popper would disagree with Zahar's and Worrall's claim that many (probably most) important scientific discoveries, especially such complex theories as Einstein's special and general relativities, are not conjectured at a single moment in all their fullness and complexity. He would probably agree that such theories are *developed* in a process that involves *a series of conjectures and much of reasoning*, and that these conjectures are guided by the knowledge the scientist already has (and often are conjectures about a new fruitful application of some already known ideas), and the reasoning involves deductive logic and mathematics. Also, I suppose that Popper would agree that this reasoning is reconstuctible, and that such rational reconstructions can be of interest for the history of science. (In fact, there is quite a few such plausible reconstructions, in works by Zahar and Worrall as well as several other authors not directly involved with Lakatos's project.) These reconstructions can even be described as logics of particular scientific discoveries, but there is no general logic of scientific discovery that can be used either for the generation of new valuable theories or for the appraisal of the available theories. (See also [11] for an illuminative account of what was traditionally expected from the logic of scientific discovery, and how recent attempts at revival of this project fail to meet any of these expectations.)

It seems appropriate to note also that rational reconstructions of the kind proposed by Zahar and Worrall are possible not only for the development within a scientific research program but for scientific revolutions just as well. This can be seen from Zahar's book *Einstein's Revolution: A Study In Heuristic*, or from a recent book by the Polish philosopher Wojciech Sady, *The Structure of the Relativistic and Quantum Revolutions in Physics* [21][4]. The «positive heuristic» that gave rise to these revolutions

[4]Sady seems to arrive at his conception independently of Lakatos and Zahar and be unaware of their predecessorship (he does not even mention Lakatos's conception of positive heuristic and Zahar's conception of the logic of scientific discovery). For a critical review of Sady's book, see [22]; another informative review in English is [12].

was part of the Newtonian research program at that time, which refutes Lakatos's claim that within a scientific research program, its «hard core» is held sacrosanct and «positive heuristics» serves a tool for immunising this «core».

### 3.2. Other apparent divergences

To vindicate the claim that other apparent divergences between Popper and Lakatos «reduce in the last analysis to differences in emphasis» [26, p. 21], Zahar analyses Popper's early work *Die bieden Grundprobleme*, written in 1930/31 but published only in 1979, as well as other Popper's works, and shows that Popper's approach to evaluating methodologies (the method of epistemological criticism he describes as «transcendental») was very much like that advocated and explained by Lakatos. Viz., both Popper and Lakatos countenanced and practiced the evaluation of methodologies by singular value judgements in regard to certain scientific achievements, pretty much like experience is used to test scientific theories. I think that Zahar succeeds to show that in this respect, Popper's and Lakatos's approaches have much in common, and the difference can be described as that in emphasis. However, even this difference in emphasis is not insignificant, insofar as it indicates a tendency in Lakatos's philosophy that Popper would disparage on principled grounds. I mean Lakatos's claim that the singular value judgements involved in this evaluation should be those of scientific *élite*. I guess that Popper would not endorse this but rather use his own judgements and appeal directly to the judgements of his readers, elite or noelite: firstly, because his philosophy is alien to elitism; secondly, because the deference to scientific elite would pose the hopeless problem of demarcation between elite and noelite; thirdly, because taking the judgement of elite as the criterion of scientific value slights the dichotomy between facts (about the value judgements of scientific elite) and values. (On the latter point, see the discussion in [14]. Although the dichotomy is not as undisputable in the contemporary philosophy as Musgrave's characterisation of it as one of philosophy's «very few solid discoveries» might suggest, it was unequivocally and vigorously upheld by Popper [15].) This, of course, does not preclude that in his or their judgements about scientific values Popper and his readers are influenced very much by judgements of competent, and especially outstanding, scientists in the relevant field.

However it may be with Popper's and Lakatos's approaches to evaluating methodologies, their demarcation debate indicates another difference, which seems to be more than a mere matter of emphasis. To begin with, if there are no substantial divergences between Popper and Lakatos, apart from the question about the possibility of a genuine logic of discovery, then Lakatos's criticism of Popper's demarcational proposition and falsificationism generally is entirely a matter of misunderstanding. (Surely, Lakatos was not criticising his own positions!) In fact, Zahar suggests that as far as the demarcation and falsificationism are concerned, there are three distinct lines of Lakatos's attack, two of which are based on misunderstandings, whereas the third one rests on a Popperian thesis that Zahar contests. However, Lakatos's «misunderstandings», as Zahar describes them, are not misunderstandings about Popper's views but rather substantial mistakes, whereas Popper's thesis Zahar contests was really held by Popper.[5] So eventually, the conclusion of Zahar's argument should be not that there was no substantial divergences between Popper and Lakatos, but rather that there would not be such divergences if both Popper's and Lakatos's positions were corrected according to Zahar's suggestions.

[5]The thesis at issue, let us designate it as T, is «that every scientific proposition is theory-laden and can consequently never be established» [26, p. 26], and that this is true in particular of statements used to test scientific theories («basic statements», in Popper's terms). From this, on Zahar's account, Lakatos draws a conclusion that «hypotheses cannot be falsified in the sense of being definitely known to be false» [26, p. 26], whereas Popper takes a conventionalist view of basic statements, which makes his theory highly vulnerable. Zahar proposes to discard this conventionalist view together with the thesis T, and replace it with the position he designates as «the phenomenological view of observation», which «asserts that every basic statement can in the last analysis be reduced to a proposition which is exclusively about our perceptions and can be decided by them» [26, p. 28]. I agree with Zahar that to make his theory defensible, Popper would need to discard the conventionalist view of basic statements, but he needs neither accept the phenomenological view nor discard the thesis T. I think he needs rather to correct his description of the agreement between scientists as to which basic statements are to be accepted by making the distinction between agreement as truth-indifferent convention and agreement as truth-aspiring consensus. The agreement needed for falsificationism is of the latter kind – it is a *consensus* between observational judgments of scientists as to what is the case. Admittedly, this would involve the recognition (made by Popper in his reply to A.J. Ayer [18, p. 1114]) that experiences can serve as inconclusive reasons for the acceptance of basic statements. The resultant consensus about the acceptance of basic statements is fallible, and so falsifications are inconclusive, provisional, at least in principle, which is perfectly OK for Popper. (The reservation «at least in principle» leaves it open whether it really happens that the recognition of the falsification of a scientific theory by the scientific community gets later recalled.) See also Andersson [1; 2; 3] for a case for a nonconventionalist fallibilist reading of Popper, according to which «in Popper's methodology test statements are not dogmatically accepted on the basis of some infallible kind of experience, nor are they conventionally accepted by arbitrary decisions or pure acts of will» but «*critically* accepted» [1, p. 61-62].

The present analysis identifies and evaluates the Popper-Lakatos divergences differently. To begin with, there is Lakatos's statement that it is OK for scientists to ignore apparent falsifications, which diverges very much from Popper's position. Perhaps, this statement on its own could be discounted as a rhetorical exaggeration. However, we can hardly take so lightly a related Lakatos's claim, that for a research program, the «hard core» «is "irrefutable" by the methodological decision of its proponents». This claim seems integral to his theory of the methodology of scientific research programs, and it seems to be the crux of the Popper-Lakatos controversy.

On the other hand, the present analysis suggests corrections and interpretations of Popper's and Lakatos's accounts that would lead to their convergence.

## 4. John Watkins on some quasi-conflicts between Popper's and Lakatos's views

John Watkins [24] points out that at least two apparent conflicts between Popper's and Lakatos's epistemological views are pseudo-disputes generated by Lakatos as a result of his «Research-Programme imperialism». By this, Watkins mean the following:

> Let $p$ be a proposition held by Popper and $q$ be a proposition held by Lakatos, where $p$ and $q$ are, from a logical point of view, mutually compatible. A pseudo-dispute arises if Lakatos nevertheless asserts that $p$ must be supplanted by $q$, although he makes no case for this and his reason for it is only that it is good propaganda for his Research-Programme methodology. [24, p. 7]

So, Lakatos opposed his claim that in science we appraise competing *research-programmes* to Popper's proposition that in science we appraise competing *theories*. However in fact, there is no contradiction between these two propositions: it may be, and seems likely, that in science we appraise both competing *theories* and competing *research-programmes*:

> It might likewise be held that in architecture we appraise individual buildings and that in architecture we appraise streets. Neither kind of appraisal need exclude the other. But now imagine that within the architectural profession a militant Street Party starts up whose main plank is that street-appraisals must supplant building-appraisals since *streets* are the basic unit of appraisal in architecture. The main plank of Lakatos' Research Programme Party was, in his words, «The basic unit of appraisal must not be an isolated theory or conjunction of theories but rather a

> *research programme!*» I accept that scientific research programmes, about which he said interesting and important things, do constitute genuine units of appraisal. But what argument did he offer for his conclusion that theories, which he usually spoke of as «isolated», do not also constitute genuine units of appraisal? After saying, rightly, that a scientific theory is judged in relation to its predecessors, he immediately added: «Then, of course, what we appraise is a *series of theories*» (1978, p. 33). That is rather as if, after saying that a building has to be appraised in relation to neighbouring buildings, he had added: «Then, of course, what we appraise is *streets.*» And in any case a series of theories is not yet a research programme: the series terminates with a last theory, whereas a research programme looks to the future. It is as if he had claimed that the basic unit of appraisal is not buildings, or streets, but plans for urban renewal [24, p. 7-8].

The second pseudo-dispute is concerned with acceptance and rejection in science. Here, as Watkins explains, Lakatos interprets Popper's claim that a scientific theory can be accepted so long as it withstands empirical tests (attempts at falsification) and must be rejected if it gets falsified as if Popper meant by «acceptance» of a theory that a scientist works on it, and by «rejection» that he ceases working on it. In fact, Popper meant quite a different things: to accept a theory is to judge that it satisfies certain acceptability requirements and is the best among the available alternatives; to reject a theory means that it fails in these respects. There is no contradiction between rejecting a theory in Popper's sense and continuing working on it [24, p. 8-9].

Yet one point that Lakatos presents as a locus of disagreement between Popper and himself is concerned with «an inductive principle» that connects corroboration (the success of a theory in withstanding attempted falsifications so far) with verisimilitude (the degree to which a theory nears truth). Lakatos suggests that Popper needs a principle that corroboration is a (fallible) indicator of verisimilitude, and that Popper was unwilling to admit this. (Lakatos describes the principle as «inductive» because it involves a move from the past performance of a theory to its correspondence with reality, which applies to the present and the future as well as to the past, or from cases that were already observed to those that were not as yet.) However, John Watkins argues that Lakatos is mistaken here: Popper did introduce the principle at issue, even before Lakatos proposed it. So, in his reply to Lakatos, Popper wrote: «I *did* suggest in *Conjectures and Refutations,* Chapter 10, that the degree of corroboration may be taken as an indication of verisimilitude» [18, p. 1011]. This means that «the propositional content of Popper's earlier publications already contained the inductive postulate called for by Lakatos. ...the whiff of induction which Lakatos invited Popper to introduce into his philosophy was already there» [24, p. 7].

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# Філософія науки Поппера та Лакатоса: головні розбіжності

**Дмитро Сепетий**

**Анотація**. У статті здійснюється порівняння концепцій розвитку наукового знання Карла Поппера та Імре Лакатоса. Обґрунтовується думка, що центральне розходження між цими концепціями стосується тези Лакатоса про структуру науково-дослідних програм як таку, що утворюється «твердою серцевиною», яка є «неспростовною» за методологічним рішенням прибічників програми, та «захисним поясом» допоміжних гіпотез. З позиції критичного раціоналізму Поппера, це теза є неприйнятною, оскільки відтісняє критичну дискусію на периферію наукової діяльності; крім того, як вказували Алан Масгрейв та Вільям Берксон, ця теза спростовується історією науки. Друга важлива відмінність між концепціями Попера та Лакатоса стосується визначальних характеристик емпіричної науки («критерію демаркації»). Аналіз дискусії стосовно цього питання дає підстави віддати перевагу дещо пом'якшеній версії демаркаційної пропозиції Поппера, яка враховує слушну тезу Лакатоса, що фальсифікація залежить від виникнення кращих теорій, але відкидає іншу його тезу – схвалення «імунізації» серцевинної теорії дослідницької програми стосовно можливої фальсифікації. Цікава відмінність між поглядами Поппера та Лакатоса стосується питання про можливість справжньої логіки наукового відкриття. Проте, з одного боку, ідеї Лакатоса в цьому питанні не були достатньою мірою розроблені й чітко сформульовані в його опублікованих працях, а з іншого, спроби розвитку цих ідей Елі Захаром та Джоном Воролом демонструють лише можливості раціональної історичної реконструкції вже здійснених наукових відкриттів, а не утворюють загальної «логіки наукового відкриття», якою могли б керуватися науковці для здійснення нових відкриттів. Ряд інших позірних розходжень між Поппером та Лакатосом є скоріше відмінністю в наголосах та фокусі зацікавленості, а також (як показує Джон Воткінс) штучного й часом некоректного контрастування Лакатосом своїх ідей стосовно поглядів Поппера.

**Ключові слова**: раціональна критика, фальсифікація, науково-дослідна програма, демаркація, емпірична наука, логіка наукового відкриття.

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