Эпистемология и философия науки
2022. Т. 59. № 4. С. 181–188
УДК 167.7

Epistemology & Philosophy of Science
2022, vol. 59, no. 4, pp. 181–188
DOI: https://doi.org/10.5840/eps202259465

POPPER AND HIS POPULAR CRITICS: THOMAS KUHN, PAUL FEYERABEND AND IMRE LAKATOS: APPENDIX

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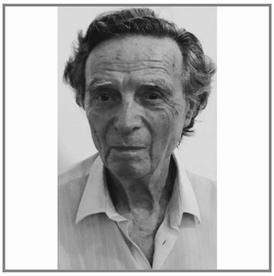

Popper's popular critics – Kuhn, Feyerabend, and Lakatos – replace his older, Wittgenstein-style critics, now defunct. His new critics played with the idea of criticism as beneficial, in vain search of variants of these that could better appeal to the public. Some of their criticism of Popper is valid but marginal for the dispute about rationality. He was Fallibilist; they hedged about it. He viewed learning from experience as learning from error; they were unclear about it. His view resembles Freud's reality principle; they hedged about this too, as they defended the stupid idea of constructive criticism (namely, hold on to your faith in a refuted theory until you can replace it). He stressed his criticism of the view of science as inductive; they endorsed it. They differed from him significantly regarding their intended readers: he had addressed those who readily admit criticism and his popular critics addressed those who find it hard to admit openly that criticism upsets them somewhat. Current popular criticism of Popper's ideas shows yet again the logical relation between the critical attitude and liberalism: liberalism without critically mindedness is permissible, scarcely the other way around. Hence, we better read the objection that Popper's popular critics have launched against him not as criticism proper, but as somewhat reasonable protest against his use of the highest standards in his relentless advocacy of liberalism and of criticism in his valuation of science and of democracy as joint.

Keywords: Popper, Kuhn, Lakatos, Feyerabend, criticism, liberalism

ПОППЕР И ЕГО ПОПУЛЯРНЫЕ КРИТИКИ: Т. КУН, П. ФЕЙЕРАБЕНД И И. ЛАКАТОС. ПРИЛОЖЕНИЕ

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Популярные критики Поппера – Кун, Фейерабенд и Лакатос – заменили его более старых, ныне несуществующих критиков витгенштейнианского стиля. Его новые оппоненты играли с идеей критики, стремясь извлечь из этого пользу, находясь в поисках вариантов, которые могли бы понравиться публике. Некоторая часть их критики Поппера обоснована, но все же не имеет большого значения для спора о рациональности. Поппер был фаллибилистом; они сторонились этого. Он понимал обучение на опыте как обучение на ошибках; их отношение к этому осталось непроясненным. Его точка зрения напоминает принцип реальности Фрейда; от этого они тоже себя оградили, так как отстаивали дурацкую идею конструктивной критики (а именно, держитесь за свою веру в опровергнутую теорию, пока не сможете ее заменить). Он подчеркивал, что его критика относится к индуктивистскому взгляду на науку – это им понравилось. Они значительно отличались от него в том, что касается их предполагаемых читателей: он обращался к тем, кто с готовностью принимает критику, тогда как они обращались к тем, кому трудно открыто признать, что критика их несколько огорчает. Нынешняя популярная критика идей Поппера еще раз демонстрирует логическую связь между критической установкой и либерализмом: либерализм без критического мышления допустим, а не наоборот. Поэтому возражение, выдвинутое популярными критиками Поппера против него, лучше рассматривать не как собственно критику, а как разумный протест против использования им самых высоких стандартов его неустанной защиты либерализма и критической установки в отношении к науке и демократии как единому целому.

Ключевые слова: Поппер, Кун, Лакатос, Фейербенд, критицизм, либерализм

This is an addendum to my *Popper and His Popular Critics: Thomas Kuhn, Paul Feyerabend and Imre Lakatos* of 2014. It is an update of its discussion of the background information to the study. This is needed mainly due to the circumstances surrounding the reception of Popper's ideas: at the time the whole philosophical establishment ignored him in a seeming conspiracy of silence. Seemingly, since the source of the silence was not intended but an unintended consequence of the cowardice normal among academics. Indeed, under extreme pressure they reluctantly gave him a back-handed compliment – saying, he appreciated science; how insulting! These days appreciation of Popper is commonplace: his supporters and his critics alike take the value of his output for granted. This has raised his reputation considerably, since now one may praise him without going on a limb. During the beginning of my academic career, well over half-a-century, it was hard to have published in the respected academic press any paper that praises him. This is how his output was suppressed with no conspiracy to speak of.

This is normal with any new revolutionary idea that meets the learned press. It is not always the case, however, since immediate endorsement of the intellectual leadership of a daring idea does happen from time to time. This was the case with Einstein's ideas: Max Planck published then in the leading physics periodical and even sent his assistant to meet Einstein in person. The reason for Planck's conduct was complex, though a conservative in any sense, he had an innovative vision when quite young. He scribed it to his teachers but to his dismay they dismissed it. His vision was, the second law of thermodynamics should have the same status as the first. The fate of the first law was not easy either. Newton declared it false. He suggested that the solar system's total energy falls continuously and is increased back by brief but repeated divine intervention. It took a great mathematician, Joseph-Louis Lagrange, to prove (mid-eighteenth century) that in a Newtonian system energy is conserved (the law of conservation of energy, namely, the first law of thermodynamics, is a first integral of the system so-called). Histories of physics often ascribe the discovery of this law to Julius Robert Mayer in 1842, which is absurd. He did make a significant contribution, though: he suggested the use of this famous law to systems that are known but not understood: their mechanisms may be unknown (as yet?). Planck understood Mayer this way and wished to emulate him regarding the second law. He admitted defeat when (in 1905) Einstein explained the second law statistically. For, (unlike causal explanations) statistical explanations allow for deviations.

A new revolutionary idea may cause public debates that often lead to their dismissal but on felicitous occasions also for their endorsements, as is the case of Machiavelli's liberalism or Maxwell's equations. Liberalism looked too good to be true yet it is now the default option in political discussions in the West. Maxwell's equations had the opposite fate. They were successful but highly problematic: efforts to make them fit Newton's mechanics (to render them Galileo invariant) were stuck until Einstein reversed the project and proposed to render Newton's mechanics fit Maxwell's equations (to render it Lorenz invariant) – as a first approximation, thus extending the (by now famous) mass-energy equation (E = mc2) from electromagnetic fields, where it first appeared (to explain their allegedly observed momentum), to mechanics in general (allowing for the mass-energy conversion that occur in nuclear processes). This was in 1905; in the 1911 first Solvay Conference Einstein won the agreement of Henri Poincaré, who was a great scientific authority and who had declared in his terrific 1902 *Science and Hypothesis* that if and when Newton's mechanics were empirically refuted, he would rescue it by what Karl Popper termed the conventionalist twist. He did not do that; he bravely yielded to young unknown Einstein. For this he deserves all the praise in the world.

The valuable idea behind this discussion of Poincaré is barely appreciated today when Einstein's having dared to replace Newton's mechanics with a new one is commonplace information, as is the fact that he thus killed the idea of science as certitude (under the influence of eighteenth-century critic David Hume, he reported). He showed that Newton's mechanics approximates his own: the Newtonian value for kinetic energy is the first term of the Taylor expansion of Einstein's value of it in his mass-energy equation. Once his relativist mechanics replaced Newton's, the possibility that his mechanics should follow the fate of its predecessor remained a viable option: the view of science as certitude had gone for good. (For a model to imitate, Einstein engaged in a long debate with Niels Bohr, the most famous physicist next to him, and quite amicably.)

Positivist philosophers of science were then engaged in efforts to replace certitude with high probability. Popper opposed them valiantly:

scientific ideas are bold, he observed, and thus improbable. He showed that (in a dice-game model) probability differs from empirical support: an initially highly improbable conjecture may be empirically supported (evidence may support it) yet remain less probable than an alternative initially highly probable conjecture that was empirically undermined while remaining the more probable of the two. This presents empirical support as (not probability but) probability-increase. Received opinion had to follow suit. The positivist philosophers who tried to replace certitude with high probability, and they were the majority of the philosophers of science, considered high probability certainty surrogate that shares characteristics with it as much as possible. This will not do. A valid inference transmits the truth of its premises to its conclusions; it does not transmit probability this way, since when a and b imply c, a and probably b do not imply probably c – since a and probably b and non-c is obviously consistent. For, however improbable non-c is, it is still possible.

There is more to it: even a very high probability differs from certitude, and inherently so. The sooner one gives up certitude, the quicker one hits the problem of the demarcation of science: what theory is scientific? This problem Popper has raised under the label of Kant's problem. This very attribution allowed for some confusion, since Kant answered this question with no hesitation: science equals certitude. He even proposed (preface to the second edition of his *Critique of Pure Reason*) suppression of the publication of conjectures! Why care about certitude? Because, Plato said, certitude eliminates controversy and thus strife.

This seems to me the lowest point of western thinking. These days, when pluralism is in fashion, there is no need to argue that the demand for certitude is an obstacle and that disagreement as such is no cause of strife. And, indeed, since we consider friendly many sportive competitions – including public debates – we may deem scientific debates this way too. Admittedly, certitude eliminates disagreement and thus it also may eliminate controversy. But not necessarily: two individuals competing for the same object agree, yet they still compete – possibly with hostility and possibly in the chivalry that spells respect for opponents. Obviously, whereas certitude may dispel dissent, dissenters may expose received claims for certitude as erroneous. Indeed, the very readiness to express doubt about, not to say dissent from, any publicly expressed claim for certitude, is inherently a challenge to its originator as unjustly pretentious. This is why the refutation of a theory was traditionally viewed a condemnation, and even decidedly so: the author of a conjecture, Francis Bacon declared, displays more love for reputation than love for the truth. It is dangerous to do that, since self-love prevents one from admitting error, and error not open to correction is prejudice. Criticism is supposed to dissuade, Bacon added, yet history disproves this: centuries of scholasticism have not closed mediaeval debates. Hence, he said, we should give up all preconceived opinion as a sacrifice for the public benefit. Moreover, this sacrifice (giving up one's opinions) is beneficial: empty minds can see things as they are and thus make discoveries "in buckets and streams".

In Bacon's time a great discovery about observation appeared on the scene: there are no pure observations, since our opinions influence our vision (we see what we know). Were this not so, said Galileo, we would see the moon as if it was a cat jumping from rooftop to rooftop like a cat as we stroll in a moonlit night. We see the moon as distant because we know it is distant. This discovery is known as the Duhem-Quine thesis. Bacon used it. He said, a conjecture is not likely to be true, and then, when we apply it we see facts distorted, so that refuting observations are seldom recognized as refuting. Only with empty minds, he said, do we see facts as they are.

Bacon's idea was simple: with prejudice we see things wrongly, and we deduce from wrong observations the wrong theories that we insert in them; without prejudice we see things as they are and deduce from true observations true theories. This means that we always see first and think later (which was the received opinion that followed Aristotle as he refused to admit Plato's Heaven). This is induction: all thinking is inductive, said Aristotle and Bacon repeated – right or wrong as it may be. Even today, many researchers take it for granted that science is inductive: deductive logic leads from truth to truth with certitude; not so inductive logic: logic is deductive, science is inductive. Hence, tradition taught, inductive reasoning requires caution. When Popper first came up with the assertion that there is no induction, what he was heard saying is, there is no science, whereas he said science has no use for induction – since it comprises bold thinking, contrary to the inductivist view that demands caution and staying as close to facts as possible. All his life Popper argued against inductivism, inventing more and more new arguments against the view that science is inductive. His arguments were increasingly brilliant, but as means of dissuasion they were quite useless: philosophers of science know that inductivism is flawed, and his marshalling more and more evidence for its defectiveness may raise their discomfort but to no avail, since they will not give up science and the idea that science is not inductive sounds to them the claim that science is a priori true or that it is absurd. Let me stress that I am saying this as a witness. As evidence, many philosophers of science cited Karl G. Hempel. He found induction puzzling, but he never considered giving up science as a viable option. I do not understand this; with my Talmudic background, I sucked the hypothetico-deductivist technique with my mother's milk. Moreover, Hempel was puzzled about generalizations in blunt oversight of the scientific tradition that demands (since the scientific revolution) to consider as (scientifically) factual all and only information reported more than once and declared repeatable (correctly or not). Hence, science does not allow for what mainstream philosophers of science label

Hempel's paradox and take with the seriousness that the scientific tradition systematically withholds.

Behind the silly error of taking Hempel seriously stands something worse: subjectivism. This is the theory that science is certainty and certainty is a feeling. Subjectivism comes to save certainty: whether a proof is valid or not may be questioned, but that it makes me feel comfortable is not, since, allegedly, my feelings are the most certain thing that I possess. This is irrelevant (since science is intersubjective), erroneous, and even refuted: some proofs of some theorems are far from leading people to feel comfortable about them. The paradigm case here is Gödel's proof: even experts may feel that it is unsatisfactory, and indeed Ludwig Wittgenstein has (foolishly) rejected it on this very ground. Moreover, after twentyfive centuries of demand for proof the question finally came up, what is proof? Instead of proof, Euclid deduced his theorems from axioms. By what authority? By the authority of Reason: a reasonable individual cannot possibly deny it. This is why he introduced the parallel axiom (given a line and a point outside it, there is no more than one parallel to that line going through that point) later in the book and not together with the rest of them. Only when Gödel offered his famous proof in which he took this procedure for granted, did he reach (almost) unanimity about it. (Intuitionists still disagree: they refuse to admit that in all cases the negation of negation of a statement is equivalent to it as well as that the complement of the complement of a class is equivalent to it; they allow for this always and only in the presence of a valid proof of that statement or the method of construction of that class.)

From the very start Popper tried to circumvent all psychological issues when discussing methodology – so as to avoid circularity. This is rooted in a metaphysics, and importantly so. Popper was not averse to metaphysics. He rightly viewed Wittgenstein's linguistic argument against it a mere name-calling [Popper, 2006, §4]. But he was unfriendly to it all the same as it is irrefutable – on the basis of his breath-taking maxim, irrefutability is not a virtue [Popper, 1945, Ch. 25, 26] but a vice [Popper, 1957, p. 159]. He changed his mind when he studied indeterminism. He first thought commonsense indeterminism suffices even while assuming determinism to be the true metaphysical doctrine [Popper, 1945, towards the end of Chapter 22). He later changed his view and declared that it does matter. He said, as metaphysical determinism is irrefutable, it is worthwhile to render it scientific and then refute it. Except that there are many ways to do that. Now that Wittgenstein's taboo on metaphysics is all but forgotten, this appears with no opposition to it: much of Popper's initially militant, brave assertions sound sheer commonsense. Yet many consider it insufficient, as it does not offer an alternative to what it opposes. Now the question, what opinion should one endorse is, indeed, a question he did not answer. One might say, he did not believe in rational belief, particularly since (as Spinoza was the first to observe) it is not under our control (although we might control disbelief by encouraging criticism). One might consider this a mere matter of windows of opportunity. Today this does not raise the objection that tradition encouraged by its inclusion of the idea that (in principle) every (well worded) question has one and only one proper answer. This idea made Popper's view questionable and its abandonment renders it admissible. We do take it for granted these days that some questions do not have one proper answer. This removes many of the objections that Popper's ideas met initially and makes them a part of our background knowledge.

This being so, in retrospect, what are we to make of the objections to Popper from the pens of his popular critics Thomas Kuhn, Paul Feyerabend and Imre Lakatos?

Kuhn's criticism of Popper is evasive: he said he objected to Popper's sharp language more than to his message. But then he said that of Carnap and of myself too: he wanted the history of science to be scientific, namely, to have a paradigm, namely unanimity among its experts. To that end he declared agreement with all and sundry, hoping to cover reference to every expert around, past, present, and future – which is absurd. He tried to overcome this absurdity by a (historically) relativist theory of truth. This renders his view either worthless or instrumentalist. Indeed, he advocated the instrumentalist view of science, as an echo of the view of Pierre Duhem who took theories as languages and so as not comparable to each other – a view that Kuhn christened incommensurability. This became his battle slogan. Did Kuhn differ from Duhem? The literature does not answer this question unequivocally. And so, at a distance of but a few decades, his message seems to have fizzled out.

The story of Feyerabend is somewhat different. He was even more ambitious than Kuhn. He asked me if I did not mind failing to be the most popular philosopher of science. He followed Kuhn in talking about incommensurability, but this talk paled in comparison with his final formula: anything goes. This is abandoning all sense of discrimination. Feyerabend recommended not only tolerating folly, which is imperative, of course, but even encouraging it. He said, most academics oppose magic out of sheer dogmatism, since they know almost nothing about it: how many academics are familiar, he asked rhetorically, with the classics of magic, such as *Malleus Maleficarum*? He encouraged his African-American students to show interest in magic. This is outrageous. He told me he was provocative in order to challenge people to think. But he went further. He said he consulted witch-doctors. In truth he barely did that and he never tried out any quack medical advice.

The story of Lakatos was still more outlandish. Of the three he was the most original: his *Proofs and Refutations* (1963) is a monumental contribution to both the history and the philosophy of mathematics. Popper said, of all his students only Lakatos taught him something new. Unfortunately, it did not bring him the reputation that he deserves.

In an attempt to gain such reputation he published some silly ideas in the philosophy of science, and these gained him the great fame that he still has, decades after his regrettably early demise.

In a letter to Lakatos [Feyerabend, Lakatos, Motterlini, 1999, p. 239] Feyerabend mentions Popper as "our lapis irae" (our stumbling block). Why was Popper an obstacle? To which activity? The answer is not flattering to Feyerabend. He, Lakatos and Kuhn were all former students of Popper's (who gave a seminar in 1950 in which Kuhn participated) who took his greatness for granted contrary to received opinion that still overlooked him. What they wanted is fame, and so they looked askance at Popper's rising fame in the hope to reach recognition before him! This race for recognition may be not new; among Popper's aficionados it was. These days, when his fame is too big to allow for a club of his aficionados, things look different.

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