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Let a Thousand Flowers Bloom:

Popper's Popular Critics *

By Joseph Agassi

Tel-Aviv University

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1. Let a Thousand Flowers Bloom

Two suggestions are at the back of the present talk. First, toleration is obligatory, not criticism. So do not try to make people critically-minded: do not force them in any way to try to offer or accept criticism, to learn to participate effectively in the game of critical discussion. If they refuse, then they are within their right. Also, they will easily advance excuses for their refusal; admittedly some of these are unreasonable, but not all. Instead of trying to make people critically-minded, try to help them become critically-minded if and when they request help on this matter, but not otherwise. My second suggestion is that a simple, inconclusive, criterion should be used to distinguish with ease between proper and improper criticism — not by reference to validity, since proper criticism may turn out to be invalid, and some of the worst diatribes may inadvertently include valid criticism. This should hardly be expected of diatribes, and these are recognizable by their display of poor appreciation their target. (This was noted in a recent review by Stefano Gattei of the Italian edition of the Lakatos-Feyerabend correspondence: it largely concerns Popper, yet it displays boorish disrespect to him.) So much for my messages here. I also wish to present here the following ideas.

With the demise of classical theory of rationality as proof or proof-surrogate, rationalism can survive without a theory of rationality, but it is better off with (at least) one. And there are two competing candidates: critical rationalism that identifies rationality with the critical attitude and relativism that says, any recognized intellectual system includes its own criteria of rationality that are binding within it. Relativism is now popular. The chief exponent of critical rationalism is Karl Popper.

Critical rationalists should invite criticism of their views, of course. In my view, they should not try to convert adherents of classical rationalism or of relativism. Popper's leading critics were relativists: Kuhn, Feyerabend and Lakatos. Their criticism was off target in one and the same way, but they were all very successful nonetheless. Popper had addressed people who love criticism; his critics, however, addressed a different kinds of people, ones who hate-criticism-but-refuse-to-say-so-out-loud — in brief, the furtively anti-critical. The strictures of Popper's popular critics appeal to such people.

Thus, Popper's popular critics are popular only because of this dislike of criticism: their criticism has no merit. There is no more to it than the appeal to the furtively anti-critical. This is not true of Popper’s other critics, those who support some version of classical rationalism. Now what classical and critical rationalism share is the recognition of the value of criticism, even though classical rationalists see it as only secondary, since they view proof as important and disproof as a preliminary to proof. Relativists, on the other hand, deem criticism of the intellectual framework within which one operates a priori invalid. What critical rationalism shares with relativism is the readiness to entertain diversity: classical rationalism takes it for granted that one option out of any set of competitors is compulsory, since the truth is one and is compulsory for rationalists. Critical rationalists agree that the truth is one, but they allow for diversity out of ignorance, out of learned ignorance, that is. Relativists deny that there is one truth.

In this essay let me center only on relativism, and ignore classical rationalism and its later substitutes. Moreover, I will center on the criticism that the leading adherents to relativism have launched against critical rationalism, as this is the most popular philosophical material today. Let me first observe, however briefly, why the hostility to criticism that relativists show is not really serious.

The hatred of criticism is as childish as the hatred of medicine: though feelings go against it, interest goes for it: in the sense that we love to be well, then, when the only way to be well is to take our medicine, we all love medicine. Hence, the furtively anti-critical have a good reason to be furtive. It is often said that it is natural to hate criticism. This is beside the point, as the matter is not psychological: psychology explains the prevalence of the dislike, but the acceptance of criticism, whether gladly or reluctantly, is due to its value, intellectual, practical or both. This is one important reason for Popper's exclusion of psychology from the philosophy of science, for his view of knowledge as objective, of epistemology as in no need for a knowing subject. Kuhn was equally explicit in reintroducing psychology into the field, in his assertion that psychology may offer considerations that might prove essential for the theory of scientific discovery. Though he did not offer a theory that would do that, he stressed that one is needed; he did not even say why. Criticism is not a matter of psychology: the value of criticism explains the change of opinion that it effects; the hatred of it explains only why this change is delayed and introduced furtively: people do accept criticism and consequently they do change their views, but they prefer to deny this: after they get well they pretend that they had never taken medicine, that they never were in need of it, that they never were sick. Refutations of scientific theories are very popular, but not as refutations: they are known as factual discoveries that stand on their own, with no reference to theory. Unable to say that they are refutations, historians of science must fail to explain their importance and even their origins. At times they declare that they have no theoretical origins: they are allegedly accidental, that is, unrelated to any known theory.

Furtive change is a form of deception; more often than not, it is self-deception or delusion. And it always greatly confuses and wastes time. It is no news that many people love to be deluded, though they pay dearly for it. The demand for delusion is huge and manufacturing it is cheap, so that it is always in stock; suppliers advertise this kind of commodity by hints, of course, as what they offer are reasons to justify the folly of the consumers' delusions. These reasons come in different price-ranges. The suppliers of cheap reasons for delusions are traditionally called mountebanks; the expensive, respected ones are traditionally called false prophets; in modern parlance, they are called highfalutin demagogues. Popper's popular critics are highfalutin demagogues. And so we better avoid paying too much attention to them, except when invited to do so for a good cause.

2. The Evasion of Criticism

Having said this, perhaps I should stop here; but I was invited to discuss Popper's popular critics' strictures, since they still interest a number of critically-minded people, who deserve to be shown that there are better things to do, for example, to try one's hand in serious criticism of Popper. For, it is not my aim in the least to defend Popper against his popular critics or against any other critics, and I consider myself one of his severest critics. But I do wish to help those who are ready to stop listening to highfalutin demagogues, who may be interested in serious criticism, of Popper or of other worthy targets. I suggest that the most popular criticism of Popper is obviously invalid; it is then better to concentrate on criticism that may be correct. The claim that the criticism of Popper's popular critics is obviously false rests on the claim that Popper has effectively criticized his predecessors and offered an alternative to them that does not suffer from that criticism. Popper's popular critics, by contrast, do not present his criticism of his predecessors and they dodge the question, is it valid? Having refused to learn from history, they relive it.

The most popular, most devastating, all-out criticism launched against Popper is miserable. It is the truism that no criticism is ever final. This truism is true: no criticism is unanswerable. Anyone knows this who has some mediaeval education, and in a Catholic country like Italy even today it is hard to find scholars untouched by scholasticism. The question is, of course, how good is the answer to a given criticism? For, wise critics tend to ignore answers that are too tiresome. This is how a critical debate that continues indefinitely may finally stop: it continues indefinitely because so many parties take it for granted that they have to go on as long as they can: they think that they owe it to themselves or to their pride or to loyalty or to something else. This, in principle, need never end, and some people do indeed engage in single lifelong debates transmitted to their followers. But more often in our society debates stop because they bore their participants to death. And indeed it is a good advice to stop a debate as soon as it begins to bore.

A debate can also be suspended: one answer to any criticisms is always good and always available: I do not have a good answer right now, but this may very well be because I have forgotten it or that I do not remember where I can find it or I need time to look for it. This, too, is always true: good answers are often easily overlooked or forgotten. Take, for example, not just good old scholasticism but a modern, up-to-date book on the problem of knowledge, written by a severe critic of tradition, especially of the religious tradition of the scholastics: Sir Alfred Ayer. In his book on the problem of knowledge he says exactly this: the skeptic is in error: I can justify many views that I hold; my views are indeed justified, but I do not always remember the justification just now. Do check and see of my report is right, if the intelligent and famous philosopher that Ayer was said this in his The Problem Of Knowledge of 1956.

Yet the answer that Ayer offers, poor as it is, is at times correct: at times one finds oneself losing a debate simply because one was too tired or distracted. There is a sillier answer than what Ayer presents, and it is, I have to consult others, and the people to consult may be a priest or a party organizer or some other leader in the position of an intellectual authority. This is regrettable, because in such cases I am not as knowledgeable as the leader; yet it is my decision to accept as the intellectual authority of the leader of this organization and not of another. Still, even this escape at times may make sense.

This happens when some reasonable social factors intervene. Some people are rightly considered authorities and they deserve special attention: they are fallible, but recognized as more learned and judicious than you or I. When a person like the Astronomer Royal speaks, one listens; one does not offer as an immediate response to what the Astronomer Royal says that it is silly -- even if one thinks so: one has to go home and first re-examine it. And when someone says something strange and unconventional in physics, then one does well to find the response of the Astronomer Royal to it before rushing to voice it in public. Even if one thinks the strange idea beautiful and perhaps even correct, one still waits. It is often the right response, as long as it is not final. Where the proper rule of conduct is to wait indefinitely for the authority to approve of a view before voicing it in public, opinions may be frozen for good merely because authorities hesitate.

An example for this is discussed in my Faraday as a Natural Philosopher: fields replace action-at-a-distance; they met with understandable hesitance and a reluctance to rush to voice an opinion, favorable or not: they could not even be criticized, since to engage in criticism of an idea is to examine whether it is true or false; to deliberate this way is to admit that Newton's mechanics is possibly false; this could not be admitted, for, current philosophy said, Newton's theory had been proven, and a proven theory is necessarily true; being its contrary, field theory is then necessarily false. If anyone could speak on so grave a matter, it was the Astronomer Royal of the time. As it happened, that person was somewhat of a friend of Faraday, and he, too, did not mention field theory though he knew how much Faraday wanted him to. He simply did not comprehend the matter.

Consequently Faraday's theory was almost forgotten. After Faraday died John Tyndall, his only disciple, wrote a book called, Faraday as a Discoverer, in which he declared field theory silly and barely comprehensible. Earlier, however, field theory had a lucky break: a cousin of Faraday was a substitute physics teacher in Edinburgh, and one of his students there was William Thomson, later Lord Kelvin, who became professor in Cambridge at the age of 26. There is published evidence that Kelvin told Maxwell to read Faraday; before that it did not occur to Maxwell that Faraday had a theory and his writings should be read. In my book I cite complaints of Faraday against the wall of silence against him. Since my book on Faraday broke this wall of silence, the same wall has engulfed it too. The idea that field theory contradicts the Newtonian action-at-a-distance theory is still not mentioned in physics textbooks. I hope some of you will look at my book and judge for yourselves whether criticism in science takes place or not, surreptitiously or not.

The social dimension of criticism, in science and more so elsewhere, then, refers to the responsible leadership, and today the irresponsible conduct of the leadership in the history and philosophy of science demands responsible response to it.

3. Popper for Criticism in Science

This brings us back to Popper; that is, to the attitude of the leadership towards him, then and now. Already in his very first book, of 1935, he emphasized that every criticism is answerable, and he discussed there this matter at great length. He used it against the naturalism that the leadership of the time was cultivating: the theory of scientific method cannot be a theory of a natural process, he said, since naturally there is a choice to accept criticism or to reject it, with or without explaining why.

It is hard to reconstruct the philosophical atmosphere in Vienna after World War I and some time before Austria turned Nazi officially. The dominant school of philosophy there was the famous Vienna Circle, which took as central and unquestionable the thesis of Ludwig Wittgenstein that logic proves that metaphysics is dead. Logic was rightly supposed to be the necessary condition to all discourse, and if logic tells us that all that can be said is fully demonstrable or fully refutable, then there is no leeway for decisions, no room for debate. There is something pathetic about debates about the thesis that there can be no debate. This is what G. E. Moore has called a pragmatic paradox, namely a statement or a debate whose very existence determines its outcome: participants in any debate know that they can have a debate, and so the conclusion is reached before the debate begins on the question, can debate ever take place?

It is well-known that Popper's early work was opposed to the theory that in principle it can always be found whether a statement is true or false, that the truth value of every statement can be decided. Yet it is seldom noted that Popper's chief argument against this thesis was that it is naturalistic. When this is noted, Popper is branded a conventionalist. This is reasonable on the basis of the ancient classical theory that there are two and only two kinds of truth, truth by nature and truth by convention. But though it is true that the truth by convention is in principle knowable, this is not true of the truth by nature. Indeed, the status of the truth by nature was unclear at the time. Popper's first book, was written in an effort to evade all metaphysical disputes. As it evades altogether the question of truth, on the ground that (a) falsity is clear and uncontroversial, and (b) that the removal of falsehoods from science should suffice to advance science. At lest in part Popper was right: the ancient theory confuses the truth with its demonstration; but he was also partly in error: he soon had to take a stand on controversial matters, including the controversial matter of the truth. And he did -- magnificently.

Popper was neither a naturalist nor a conventionalist. In his first book already he argued against both naturalism and conventionalism. Whereas the naturalist philosophy of science is the view that scientific truths are unassailable, so that all criticism of them must fail, the conventionalist philosophy of science is the view that in response to valid criticism valuable theories should be altered minimally — as little as required by the criticism. Against both naturalism and conventionalism Popper suggested to devise rules to encourage criticism and against making light of it. Against naturalism he asserted the view that everyone can always make mistakes, and he therefore suggested to go for high degree of openness to criticism; try to invent theories that invite easily devised efforts to criticize them, and take the criticism as seriously as possible. The conventionalists recommended damage control: to accommodate criticism as cheaply as possible; Popper, to the contrary, suggested allowing criticism to do as much damage as possible. True, criticism is not always valid; likewise, the validity of valid criticism in itself need not impose it, as it may rest on false assertions. Thus, when Newton's theory was taken to be true, all criticism of it was checked for validity, and when it turned out to be valid, its premises were declared false. That is, some observations were made that were found to conflict with the theory; it was then declared that the facts are not as observed. This was explained, once as the result of optical effects that had been ignored (aberration), and once as a result of the oversight of a planet that interfered with the deviant planet: it was caused by an unknown planet. In both cases it turned out that once the damning evidence was corrected it was then no longer damning, no longer in conflict with the theory. Popper's popular critics take this as evidence that science is dogmatic, that science resists empirical criticism. This is plainly silly, since it only shows that the criticism itself is not immune to criticism. And so any criticism may be rejected in a very special way, namely by refuting it, by showing that it is invalid or that it rests on false assertions; otherwise response to it should always be maximal, says Popper, that is, in manners that raise, not lower, the challenge. (In Lakatos' classical Proofs and Refutations Popper's idea was advocated as the promotion of progressive problem-shifts and the avoidance of regressive ones.)

4. Lakatos Against Criticism in Science

Popper's popular critics did not like this suggestion of his. Let me begin with Lakatos, as his critique of Popper is so unintelligent, I hope you will not take my word that I describe it correctly but go and check it for yourselves. For, there is no doubt that Lakatos was one of the greatest and most critically-minded thinkers -- as long as he preferred being critically-minded to being popular. But now I am discussing the criticism which he offered of Popper's critical philosophy that he offered in order to be popular, when for the sake of fame he was willing to display hostility to the critical attitude to which privately remained faithful. (To use Lakatos' own terminology, there are here two different philosophies of Lakatos. Lakatos-one and Lakatos-two. And the difference between them is easy to spot, as Lakatos-one promoted criticism and furnished lovely mathematical examples of its fruitfulness, whereas Lakatos-two played down criticism and dogmatically declared it ineffective in science.) According to Lakatos, since any criticism of any thesis can be answered, no thesis is really criticizable in the first place. He spent much time discussing the view that every criticism is answerable, and some of those who tried to defend Popper against him, for example Noretta Koertge, took up the challenge. They all referred with justifiable respect to Pierre Duhem, the justly great authority, the leading conventionalist philosopher and historian of science of the early twentieth century. This is a confusion: Duhem never said what Lakatos said: on the contrary, he said, every time a thesis is criticized, it should either be given up or rescued by the slightest modification possible, preferably by mere reinterpretation. He never said there is no valid criticism; only Lakatos said that, perhaps also Kuhn (who was less sharp in his articulation than Lakatos).

The argument of Lakatos, applied to archeology, reads thus: since every piece of pottery that is broken is repairable, no piece of pottery is really breakable. Those who meet this argument may ask, can a piece of pottery be so thoroughly crushed that it cannot be repaired? This is an error on their part; the answer to this folly of Lakatos is different. It is, the need to repair a piece of pottery is proof enough that it is broken, and if it is broken, then, surely, it is breakable. The question is, are there any refutable theories? And the answer is, yes; those who say theories should be rescued from refutations thereby admit that they are refuted, so that there are refuted theories, so that there are refutable theories, and these surely are scientific -- by Popper's demarcation of science as refutability.

Lakatos would not agree, and he would ask, boldly, what is refutation? Let me offer here some comments on the theory of refutation which belongs to the great philosopher and historian of science of the early nineteenth century, Dr. William Whewell.

Refutation is a disappointment from an expectation, more briefly, it is a counter-expectation. There may be different reasons for an expectation, it should be noted. An expectation may rest on a dream or on a whim; it may rest on any kind of misunderstanding of any passing hint, and it may rest on some vision, perhaps a religious vision, perhaps a vision of an image of the universe. In rare cases, the expectation is deduced from a clearly stated theory. In the case of this kind of expectation, of an expectation based on a clearly stated theory, if it is repeatedly disappointed (about the importance of repeatability see below), then that disappointment is a refutation, and a it is thus a refutation of the theory on which it is based. Such events do occur, but almost only in science: seldom is a theory clearly articulated and its consequences formally discussed, so as to derive from them a refutable expectation, so as to test them repeatedly. Indeed, the very concept of a refuted theory is one to be found only in the literature that discusses science one way or another, that is, in the scientific literature or else in the literature in the philosophy of science (at times also in stories about scientific activities: biographies, fiction or science fiction). Those who doubt or deny that refutations occur in science do not know what they are talking about, or they pretend not to know, or they do not want to know. This is their liberty.

Lakatos would have claimed that this is all not to the point: he would change his tune, and offer another variant of his views. For he had a few variants, and he switched from one to another as means of escaping criticism. He would then not deny that disappointed expectations happen, that some of them are based on theories, and that therefore the theories should be altered. He declared these alterations irrelevant, since what is called a scientific theory is not a fixed set of statements as envisaged by Whewell, Duhem and Popper, but a series of such theories, all of which share a small set of statements, which he called their hard core, claiming that in historical fact the hard core is taken as given and protected from any modification due to refutation -- until it is replaced by an alternative to it. This version of the view that Lakatos has offered does allow for refutations but it makes light of them. It is a variant of what the image of science as presented by Kuhn, the variant that identifies Kuhn-style paradigms with Lakatos-style hard core, except that the paradigm Kuhn-style is not explicitly stated and the paradigm Lakatos-style is. They agree that no prediction follows from the paradigm or hard core and so it cannot be empirically disappointed, yet it may be replaced. It is not clear then, why Lakatos says it has to be protected and why Kuhn says it has to be imposed on the profession by its leadership. It is intriguing that at least Kuhn was serious enough to present the paradigms as obvious in cases from the history of science: he offered as an example only the history of astronomy and one case from the history of chemistry -- the Lavoisierian revolution). Lakatos admitted that his idea of the hard core hardly a description of the history of science; it is a rational reconstruction, he explained, a way of looking at it. All in all, it is not clear why Kuhn endorsed Lakatos' identification of Kuhn-style paradigms with Lakatos-style hard cores, despite the fact that Lakatos' view contradicts Kuhn's claim that the paradigm is not given to clear articulation. In any case, this idea, that the paradigm not given to clear articulation, is not serious. It flatly conflicts with his explicit assertion that as a paradigm is repeatedly modified in the light of evidence to the contrary -- in other words, because some evidence is valid criticism – it becomes more and more ad hoc and a point arrives when the leadership thinks the situation is too unpleasant, so they spend some sleepless nights and dream up a new paradigm. Kuhn insisted that what is needed is not a logic of discovery but a psychology of discovery, so as to dodge the fact that the endorsement of criticism invites a search for an alternative. Even according to his own theory criticism induce scientific revolutions, except that he says only series of criticisms is revolutionary, not a single one, and the leadership decides how long or short the series should be before a change is enacted.

All this, however, is not too significant, at least not by comparison to the central question regarding criticism. Kuhn said his view hardly differs from that of Lakatos. Presented in the best possible light, the view in question reads as follows. Science progresses by the invention of series of irrefutable metaphysical ideas (called paradigms by Kuhn and hard cores by Lakatos). These are supplemented by series of sets of clothing; each set of clothing is refutable, but this does not matter, as the refuted set is replaced by another set which shares with it the metaphysical idea. The metaphysical idea itself is replaceable too, but only after a better one is found, not before. Query: is the added clothing deemed false after it is refuted and before it is replaced? Is it the prerogative of the metaphysical idea alone to be deemed true until it is replaced? Why? On the whole, what causes the change -- of the clothing and/or of the metaphysics -- if not refutation? Is science empirical? Does science learn from experience? If not, how does it progress? Where is the novelty of scientific discovery? As Lakatos died young, some of his followers stepped in and offered a theory of novelty for him. The idea is that novel facts are unexpected. This idea belongs to Sir Francis Bacon. Since Bacon demanded that before the start of a research all expectations (anticipations, he called them) are given up, he had no room for counter-expectations, so that he was left with the expected and the unexpected. He rightly said the expected is not new, and concluded, validly but in error, that the new is the same as the unexpected. Whewell criticized Bacon, saying, the genuinely unexpected, that which is neither expected nor counter-expected, is not noticed. Popper's theory suggests that the novelty of facts is their being counter-expected. Modern perception theory, says John Wettersten, rests on ideas of Whewell and of Popper.

5. Surprise as Refutation

A disappointed expectation, though seldom a refutation of a theory, it is always a surprise. It is an interesting fact that in ordinary language, the description of a surprising event follows Bacon: the common expression is, the event took us by surprise; we had no idea that it would occur. This is a misleading articulation. In a cocktail party we are introduced to Sr. Bianchi; a few second earlier we had no idea that this would happen; we did not even know that Sr. Bianchi exists; was that a surprise? No, definitely not. It is expecting Sr. Rossi and meeting Sr. Bianchi in his stead that is surprising. Similarly, meeting the prime-minister or a prima donna in a cocktail party is surprising for those who had no idea that they would be there, meaning, those who had expected them to be not there but somewhere else. Consider our deceased friend. He was young and strong as an ox, and one day alas! he dropped dead; no one expected that. In other words, he was positively expected to outlive us all. Suppose, on the contrary, that a friend reported deceased was approaching us as we were walking down the Galleria. What a surprise! How unexpected! we thought you were dead! In other words, the expectation was to never meet you again, and, what a joy! here you are nonetheless, alive and well. Thank God!

The encounter with our friend must be taken as a refutation; a refutation of either the assertion that he is dead or of the acclaimed theory that death is final. Does Popper recommend that the theory be deemed refuted? Should it be considered refuted? No, twice no. Not at all. Why? Allow me to dwell on this stupid question, in order to show how futile is the learned discussion of this matter that engaged Popper's popular critics and that still engages cohorts of their followers and critics. Obviously, we were mistaken, as a disappointed expectation was experienced. Of course logic says that there is a choice in the way it is possible to correct the mistake; Popper has stressed that as far as logic is concerned the choice is between two options: to reject the observation that our friend is dead, and to reject the theory that death is final. Yet commonsense says, the theory will stand and the observation that our friend is dead has to be rejected and replaced with the one that we have just met our friend down the Galleria. Does this not refute Popper's view that the theory should be considered false?

Perhaps so. It is not that our friend was reported dead that is the matter; this, after all, is not scientific information. But such information does exist. Surprises about death do occur: people survive clinical death repeatedly. Yet this did not lead science to reject the hypothesis that declares death final; rather science changed the idea about clinical death. Why?

The answer seems obvious: the observation that decayed flesh cannot return to life is far from having been refuted, and the signs of clinical death are supposed to be technical means for the precise determination of the moment after which revival is impossible. New means of revival have raised the question, what part of the body should be beyond revival for the certification of death. This occurs with many other technical means for determination of some transition or another. For there are many practical means for determination, and they are called touchstones, after the initial touchstone which was the means for the determination of the degree of purity of pieces of gold offered for sale on the open market. Touchstones could be fooled, as they often are; but the characteristic that the touchstone tests can be examined otherwise and then a genuine refutation may take place, a refutation of a theory proper. In other words, touchstones are convenient means, not reliable observations.

Lakatos, and more so Feyerabend, will say, there are no reliable observations anyway; there is no assurance that we have really met our deceased friend walking down the street. What we saw may have been another person, or a hoax, or a hologram, or a ghost. None of these options can be denied with assurance. This fact was stressed decisively by Popper in his first book. When it was first published, it was met with hostility, since at the time the reigning dogma was the famous verification principle, which says that the only utterly certain information is of reports of observations of facts and statements that follow from them. The popularity of the criticism launched by Popper's popular critics against him proves that his observation is now fully admitted, but only when stated as a criticism of his views, not when honestly admitted as a refutation of the verification principle. Thus, very regrettably, some patent dishonesty is behind the popularity of Popper's popular critics.

6. Feyerabend Against Method

Here Feyerabend parted company with Kuhn and Lakatos: he freely admitted that he was dishonest; he said, anything goes: if you want, you may say that you have met a dead friend in the street. Why not? Most readers found Feyerabend's position very stimulating; of the rest, some found it funny, others found it dangerous, and only few took it to mean what it says. What did he really mean? No one knows, and Feyerabend himself insisted that he did not mean to say anything in particular, that he said whatever he said only as therapy. As mere therapy. For my part, I do not know what this therapy is, as I do not know what his diagnosis is, not even what disease he was diagnosing. The only diagnostic point he made, and he made it repeatedly, is that apart from being imperialist, science is a culture like any other, neither better nor worse. And he argued at times that he meant this very seriously. In a published letter to me he said that as a patient he came to prefer consultation with traditional folk healers over modern medicine. This, he admitted to me personally, is rather misleading: he never underwent any treatment as prescribed by folk medicine of any kind. As he said whatever he said as a mere part of his therapy, what he said as his diagnosis is also a part of the therapy, so I do not know where I can find his diagnosis proper, or even the complaint he supposedly was diagnosing. So I will die ignorant of this. Yet I do know what he said in his early phase, when he was still seriously engaged in efforts to criticize Popper's ideas. He said then, since factual information is conjectural, the choice between theory and contrary evidence is a matter of free choice, contrary to Popper's suggestion that there is, or should be, a rule governing this choice (between a theory and its refutation).

What is there, then, free choice or a rule? Feyerabend said, there is an utterly free choice, as there is no method, no valid rule. Take then the case of our allegedly dead friend whom we met walking down the Galleria. May we suppose that he has come back from the dead? Yes, said Feyerabend unabashed, if we want to. There is a story of a certain Rabbi Akiba, he said, having gone to heaven and come back to life. He allowed anyone who wants assert that this story is true -- literally true. It is easy to say, he exaggerated. The question is, what is wrong with his idea that any fairy tale may be declared literally true?

This seems to be the central issue when Popper's philosophy of science is discussed, and it rests on the assertion that no criticism is unanswerable. This fact, that no criticism is unanswerable, is explained by a dual claim: first, that no description of fact is free of a theoretical element, and second, that all theory is fallible. This argument is faulty, as it is both unnecessary and invalid. It is unnecessary, because all assertions are possibly false, including factual observations, and were they free of all theoretical element, they would still be fallible. It is impossible, because of a simple confusion. The support which observations provide theories is unreliable as they are theory-laden. This is very obvious: it is because the theory is fed into the observation that the observation supports that theory. The proof of this, said Bacon, is that conflicting theories are supported by the same observations, presented differently by their different adherents. Bacon said, hence, as scientific observations should serve as a firm basis for theories, they must be free of all theoretical bias. (This is way he insisted that a good researcher must have no opinions to begin with.) This explains the search for theory-free evidence: it is conducted by those concerned with validating the support of theories by evidence. For people like Popper, however, who are concerned rather with criticism than with support, this is neither here nor there: the criticism that contrary evidence provided is not weakened by its being tainted by the theory it is should undermine. On the contrary, if evidence contradicts a theory even though it is tainted with it, then this is hardly ground for suspicion that the conflict between theory and observation is rooted in the influence of theory on observation. Demanding a rule for the admission of criticism, Popper endorsed the traditional rule that (though fallible) observation is preferable to conflicting theory: it is better to follow the verdict of observation than of theory, he said, in order to prevent dogmatism. Feyerabend, however, saw no fault with dogmatism, so he naturally was not swayed By Popper's argument.

Still, the choice is not limited to the positions of Popper and of Feyerabend. There is, for example, the position of Bunge, which may be correct or not, but which is certainly very sane. He said, it is advisable to do what one can to rectify the situation by making a small concession to the criticism, but in the hope that this will not do, that in the effort to rectify the situation the concession will have to grow, so that the end product will be a scientific revolution proper.

My own view is neither Popper's nor Feyerabend's, nor even Bunge's, though, like his, it is a middle position: I support the freedom to violate any rule at one's own risk. All this is interesting, but it does not in the least pertain to the case of our encounter of a presumably dead friend. When we meet a presumably dead person, we assume that the factual information about his demise should go, not the theory of the finality of death. Why? Is it proper to join Feyerabend and say, anything goes, any choice is as good as any other?

It should be clear that if sticking to theory in the face of facts dispenses with the need to test the theory, and no point. So clearly in case of conflict between theory and information, preference goes sometime this way, sometime the opposite way. Why? Is this an arbitrary choice? It does not look like it: the information about a dead friend come to life is not admitted as a refutation. Why? When is theory jettisoned? What criticism of a theory is deemed deadly? Why?

7. Kuhn Against Destructive Criticism

What criterion do/should be employed to decide between theory and evidence to the contrary? This question was raised by Duhem and Poincaré. They said, theories are tools; decisions about them should be made in the most agreeable manner. This sounds like Feyerabend's view, but it is not. His view is, anything goes; there is no criterion; one may decide arbitrarily; every decision is as good as any other. They offered a criterion, and criteria may be misapplied. The criterion, as it happens, is indeed not easy to apply. Yet they both insisted that, above all, a useful theory must be reconciled with evidence to the contrary, since a contradiction makes it useless. So not all theories need be rescued from criticism, only the useful ones. This was particularly stressed by Poincaré who was less friendly to Mediaeval science than Duhem. He said, to be scientific one must be honest, and so admit openly the validity of the criticism one tries to accommodate. Conduct that is not open makes the resultant theory pseudo-scientific.

Poincaré wrote a memorable, classical passage, saying, as long as it is not decided what is meant by "lines", Euclidean geometry cannot be tested. The decision on the meaning of "line", say as a beam or ray of light, renders the theory testable and refutable. Suppose, then, that at times light rays refuse to follow Euclid. Euclid may then be blamed for it; alternatively, the rays may be blamed. What is the more intelligent move? The theory is so well entrenched, said Poincaré and it is used so often, that it would be hard to do without it. It is not intelligent, then, to jettison Euclid. So it is better to declare that light rays are not always straight lines. Einstein disagreed; he retained the idea that light rays are straight lines and concluded that straight lines do not obey Euclid.

Enter Kuhn. He took Einstein for granted. He said, Einstein has offered an alternative to Euclidean geometry. Poincaré knew the alternative. He had disregarded it because of his criterion of choice of a theory. The criterion was simplicity, and whatever this is, he was convinced that Euclidean geometry is simplest. Kuhn learned from Polanyi that the choice of a theory need not and cannot obey any criterion. This sounds dangerously like Feyerabend's view. It is not: he says choice is arbitrary; Polanyi says, the scientific leadership should choose responsibly, though they neither can nor need explain their choice. Kuhn says, leaders always choose a theory, usually extant theory, but at times an alternative of their design.

A theory is retained until an alternative to it is found, says Kuhn. This is the theory of constructive criticism, that often goes under the name of Lenin. He usually could dismiss criticism lightly; also, he asked for alternatives. A criticism with no alternative appended he declared a barren exercise; otherwise he moved to attack the alternative: the best defense is the attack, he used to say. The result was successful, and so he hardly ever had to change his mind, no matter how harmful his mistakes were. Back to Kuhn. Perhaps his demand for constructive criticism does not help with our instance of a friend come back to life, since there is no theory of death as yet and no theory of immortality. Not so; theory that makes it practically impossible to bring dead flesh back to its original structure, and no alternative to it exists. There is no trouble allowing the clinically dead to return to life prior to the decay of vital tissues. And so the theory in question cannot be given up, whatever it is, until an alternative to it is found. Now, whatever the theory is, it involves basic ideas of physics, thermodynamics in particular, of biochemistry and of molecular biology. So we cannot give up the theory that decayed flesh will not return to life, Is this good? Let us dwell on this question.

Popper's popular critics say, better stick to theory, at least until an alternative to it is found; hence, they add, the alternative is the real cause of change, not the evidence that Popper deems refuting. Now it is logic that says, contrary evidence is refuting, so that admitting it is also admitting the falsity of the theory it contradicts. In the same way, admitting the evidence that a person returning to life amounts to admitting it as a refutation of the hypothesis that there is no return to life. So why is the theory taken for granted and the contrary evidence rejected? Because the evidence is not scientific. To be scientific evidence has to be repeated, deemed repeatable, and corroborated. We still do not believe that there is return from death, but we do witness many miracles that but a generation ago were deemed impossible, mere science fiction. So just as the possibility of reviving the clinically dead is admitted, were it possible to repeatedly revive the dead even past a certain degree of putrefaction, it would be admitted too. Any evidence that has repeatedly stood to the test would be admitted. The friend whom we have just met was deemed dead not for following a careful test but for following an unreliable piece of gossip. It is an important and interesting fact that we do trust a corroborated theory in preference to uncorroborated conflicting evidence. Popper's popular critics deny this fact: despite evidence but consistently they cling to their view that one better cling to one's view despite evidence.

All this places our presumably dead friend besides the debate, as the debate concerns a corroborated theory and a piece of corroborated evidence to the contrary. Which should be considered false? Is it a free choice as Feyerabend says? Is it an obligation to endorse the evidence to the contrary as Popper says? Or the theory be endorsed until an alternative to it is found as Kuhn and Lakatos say? Or is Popper followed by all except by the adventurous, as Bunge and I say?

8. When Theory and Evidence Clash

How is it decided what to blame for the conflict, the theory or the counter-evidence? This question was decided for the fist time by Robert Boyle, who made it the rule of the Royal Society of London, and thus it became the rule accepted by the commonwealth of learning. It is, indeed, the only rule endorsed by all members of the scientific community with no exception. The rule says, judgment should be suspended as to unrepeatable evidence, but when repeated and deemed repeatable, evidence is binding; hence a theory contradicted by it has to go. This, to repeat, is the only item agreed upon by all scientific researchers. It is not accepted by philosophers of science, strangely enough, as most of them examine the question, how is singular evidence support its generalization? Perceiving repeatedly some members of the set A belonging to the set B, how does one properly conclude that all A are B? This question does not belong to science, since in science only the generalization is endorsed, and has to be endorsed -- until it is refuted, of course. Of all recent philosophers of science, those who take this for granted are Popper and Bunge, and their reasoning is very similar: to be scientific an item has to be testable. Ironically, the only significant item in Popper's philosophy of science that is in full accord with the scientific establishment is the one most passionately attacked by the philosophical establishment. If they cared more about finding reasons for rejecting him than finding the truth about science, they would have been doing the right thing. For, the rule that he has endorsed and that they have attacked is known to be the rule most rigorously followed in scientific practice.

The move repeated by Popper's popular critics was first taken by others in order to expose him as a conventionalist. Of course, he rejected naturalism and recommended a few conventions, including the one at hand, to prefer scientific evidence to scientific theory in case of a conflict between them. The attack was meant to do more, to expose him as a follower of Duhem. A follower of Duhem he was in the sense that he saw science as involving conventions, but not as to what conventions science respects: Duhem said science recommends damage control and Popper said the opposite. Duhem could not make sense of Boyle's rule, and Popper could and did. Why then are there so many essays devoted to proving that Popper was a Duhem-style conventionalist? Because, this will show that his view is not obligatory. Were he able to show that though positive evidence is not final, evidence to the contrary is, then his view would be obligatory; but he could not show that evidence to the contrary is obligatory either. Fine; it really good to emphasize that Popper's views are not obligatory — even according to Popper. It is also not obligatory to express appreciation of achievement, but it is only decent to do so. Popper did score, at least in that his peers had failed and he succeeded to take account of the standard scientific rule that repeatable evidence is obligatory. But there is something obligatory here too: the rule of the game requires that critics who base their critique on the assertion that no evidence is final, evidence to the contrary included, should observe that this is what Popper himself asserts, and even emphatically so. The critics will then have to say what damage this admission causes to the author whose views are under scrutiny. This was never done. In brief, the discussion of Popper's work in the standard literature still is downright dishonest. Why this dishonesty? I do not know; I do not know why Popper's critics were not honest from the start, and I do not know why his later popular critics were welcomed by the philosophical establishment while the charade still continues. It is time to clear the air and review with some measure of honesty the literature on the reception of Popper.

Still, the question stands: when scientific theory and scientific evidence conflict, since neither is final, which should be preferred? This question need not be decided right now.

It is more important to understand the meaning of the extant options. According to Kuhn and Lakatos the theory is needed, so it is not declared false until it is replaced. This is refuted by the frank use of false theories: they are often needed and then they are used and with no fear of admitting that they are false. Classical theories are used in physics, at times as they stand and at times with modifications, slight or substantial as the case may be, at times but only at times in agreement with the observation of Duhem that theories vary only gradually, not ever drastically. Some times classical ideas are used because there is no known way to avoid them. As Schrödinger worked on an equation for the electron, he naturally preferred to work within the relativistic framework. He found a relativistic equation that he found unsuitable (though later it was rediscovered and found use in another context), so he tried an equation within the classical framework. He did not like it; he found it a defect; but it was the best he could do then, and it was deemed magnificent anyway and it won him a Nobel Price. This refutes the theories of Kuhn and of Lakatos.

Schrödinger’s equation is very much in use. But one does not say, since we use it we deem it true. Hence, we take truth and usefulness ad different matters. Hence, both naturalism and conventionalism are refuted. The use of different alternatives, such as Schrödinger's equation and Dirac's equation for the electron, is in conflict with and in accord with conventionalism; the refusal to declare both true is in accord with naturalism and in conflict with conventionalism. It is not that the universe adjusts to our equations, and it is not that our equations adjust to the universe; our research doe try to adjust to the universe, but its outcome is only partially adjusted. Newtonian mechanics holds within limits set to it by relativity and by quantum mechanics, for example. All this is ignored by Kuhn, contrary to received views, yet he is the champion of the received views as received views. So he is simply inconsistent, and the reason he is so popular is that he flatters scientists by telling them that they are always right when they conform, and to philosophers in his opposition to Popper. All this is not serious, unlike the question, when is the evidence to the contrary preferred and when scientific theory is, and why?

The problem seems to me not too pressing. As Bunge observes in other contexts, there are many options here, and science may examine more than one. Even the same researcher may and often does try more than one option. It has amazed me that this obvious commonsense fact, corroborated in ever so many case histories, of course, is not as popular as it should be. It is really not hard to see cases in which the same researchers, facing a scientific theory and the scientific evidence to the contrary, try options based on the hypothesis that the theory is true and other options based on the contrary hypothesis. There is nothing wrong with that. It is after all very much the same as the case of the detective who explores all avenues and considers every individual around a suspect. Of course, the courts will have to consider all suspects innocent until proven otherwise, but the detective explores a few suspicions.

What then is proof in court? What is its equivalent in science? Can the process of proof in science be emulated in courts? Can the process of proof in courts be emulated in science? It should be noted that science is not above the law, and that scientific evidence is often used in courts. Can the rules of the courts be exported to science? No. For one thing, the rules of proof in courts vary; they differ from time to time and from place to place; their truth is decidedly truth by convention. So which of the sets of rules should science opt for? The best, of course. By what rule can decision be made as to which set of court-rules is the best? The one that is perfect, of course, the one that is infallible. Except that this does not exist. It is the desire for perfection that makes science so very different from courts. Once perfection is given up, the rules can be exported from the courts to science, except that the rules of science are better than the rules used in any courts! And this is so for a very simple reason: science can afford more skepticism than courts, it can explore more avenues, it can overturn the most established ideas. Courts are not allowed to question the law of the land. Science is allowed to question everything, or rather research is allowed to do so, and if and when something is found interesting it is shared with peers and with the whole commonwealth of learning.

This should do for now. I leave my discussion with this serious question, as this question leaves Popper's popular critics behind: they claimed to have solved it better than Popper, and with trite arguments. They did not. The question is still open.

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