The Critical Rationalist

ISSN 1393-3809

Volume 02 **Number 02**

05-Jun-1997

Editor-in-Chief: Dr. Ray Scott Percival
E-mail: tcr-editors@www.eeng.dcu.ie
Web: http://www.eeng.dcu.ie/~tkpw/tcr/tcr-home

The Critical Rationalist **Vol. 02 No. 02**
ISSN: 1393-3809 **05-Jun-1997**

Popperian Ideas on Progress and Rationality in Science

John Watkins

The Critical Rationalist **Vol. 02 No. 02**
ISSN: 1393-3809 **05-Jun-1997**

Popperian Ideas on Progress and Rationality in Science

(First presented at the American Philosophical Association Eastern Division Meeting 1996, at the Symposium on Scientific Rationality and Progress in Memory of Paul Feyerabend and Karl Popper.)

(1) I got to know Paul Feyerabend well in 1961, when I was in Berkeley. (I also met Tom Kuhn then.) I loved his racy, carefree, and often surprisingly erudite talk and letters. But after *Against Method* we got rather stuck into confrontational postures (see for instance our argy-bargy in *Progress and Rationality in Science*, Radnitzky & Andersson 1978). Karl Popper I first encountered back in 1947. He had a decisive formative influence on my intellectual life. I remain a tremendous admirer of the ideas about science, especially about the growth and progress of science, which he published during his heyday, from 1934 down to about 1960. But I am critical of certain tendencies in his later philosophy, one of which I turn to now.

(2) For Popper, science is like music or poetry in that it requires inventiveness, there being no recipe or method for creating it, and also unlike music and poetry in that there are criteria of progress for it but not for them. What are these criteria? Popperians are generally agreed that corroborations have a dominant role in determining whether one scientific theory is better than another; but there have been large disagreements as to why, or in what sense, the better corroborated of two theories is the better theory. This is the main question I shall address.

(3) Through the 1960s and early 1970s Popper believed that he had got hold of an ordinal scale for truth-likeness or verisimilitude. Its lower bound was given by the contradiction, and its upper bound by the set T of all true statements. In place of this latter concept he sometimes operated with the idea of the target theory T^* towards which a sequence of scientific theories T_1, T_2, $T_3 \ldots$, each of which may be false, is progressing (see for instance his 1976, p. 155).

(4) When is a theory T_2 more truth-like than theory T_1? Popper's original answer was deceptively simple and persuasive. A theory's content is the totality of its logical consequences; its true consequences constitute its truth-content and its false consequences, if any, its falsity-content. And Popper proposed the following three requirements for theory T_2 to have greater verisimilitude than theory T_1: first, T_2's truth-content includes T_1's; second, T_2's falsity-content, which may be zero, is included in T_1's; third, at least

one of these inclusions is strict. These requirements are trivially met when T_2 is true and strictly entails T_1. But his idea of increasing verisimilitude should apply to false theories. For it was a main plank of his anti-inductivist view of science that, at least since Anaximander's explanation of the earth's supposed stability in terms of there being no sufficient reason for it to move in any particular direction because it is equidistant from the heavenly bodies around it, science has been advancing explanatory theories that typically turned out to be false, and were often aimed at erroneous explananda.

(5) He broached these ideas in 1960, at the first LMPS meeting, in Stanford. They were published in Chapter 10 of *Conjectures and Refutations* (1963). As is well known, a decade later a fatal defect in them was discovered independently by David Miller (1974*b*) and the late Pavel Tichy (1974). Soon afterwards Herbert Keuth (1976) and Hermann Vetter (1977) independently discovered another discouraging result, and I will begin with this.

(6) I used to believe that I had made one small contribution to the theory of verisimilitude. In my *Science and Scepticism* (1984) I made much use of the idea of two statements being incongruent counterparts of one another; that is, their consequences, though different, are in one-to-one correspondence. And I took it for granted that for one incongruent counterpart to be closer to the truth than the other, the ratio of true to false among its consequences must be higher than it is among those of the other. I was unaware that Keuth and Vetter had shown that for any false statement, the number of its true consequences equals the number of its false consequences. I tried to put matters right in Watkins (1987).

(7) Miller (1994, pp. 209f) has recently come up with a neat new way of obtaining this result as well as his and Tichy's previous result. Let T be a false theory and let f be a proposition in its falsity-content. Now consider the bi-conditional f-iff-x, where x is some proposition; f-iff-x is true when x is false and false when x is true. To every x in T's truth-content there will correspond an f-iff-x in T's falsity-content, and to every x in T's falsity-content there will correspond an x-iff-f in T's truth-content. This reproduces the Keuth-Vetter result.

(8) I may mention that my idea concerning the comparative verisimilitude of incongruent counterparts would have avoided this result if I had restricted the comparison to their empirical content, as understood in *Science and Scepticism* where it is equated with the class of the theory's singular predictive implications, or SPIs, as I call them. These are simply the negations of what Popper called its potential falsifiers. Thus if, to take the simplest case, the theory were (x)(Fx $\rightarrow$ Gx) where F and G are observational predicates, then Fa & $\sim$ Ga is a potential falsifier and Fa $\rightarrow$ Ga is the corresponding SPI. As

I characterized them, SPIs generally have a pebbly character: if you have two separate pebbles you don't create a new pebble by sticking them together, not even if you use super-glue; and nor do you create a new SPI by sticking together two SPIs, at least if their predicates do not overlap. Thus if f is one SPI and x is another, f-iff-x is not a SPI. And if T and T' are incongruent counterparts with their SPIs in one-to-one correspondence, it is entirely possible that the ratio of true to false among the SPIs of T is higher than it is among those of T'. Miller once used the analogy of invited guests arriving trailing gatecrashers behind them to depict the defect in Popper's definition of verisimilitude (Miller 1974*a*). Restricting the consequences that are taken into account to SPIs should exclude 'gatecrashers'.

(9) Now to Miller's new way of obtaining his and Tichy's old result. The question is whether, given a sequence T_1, T_2 ...of theories which, though false, are progressing towards the true target theory T^*, T_2 could have more verisimilitude than T_1 in Popper's (1963) sense. Assume that T_1 and T_2 satisfy Popper's first and second requirements: T_2's truth-content includes T_1's and T_1's falsity-content includes T_2's. Can they satisfy his third requirement, that at least one of the inclusions is strict? Let f now be a statement in T_2's falsity-content; by the second requirement f will also be in T_1. Let x be any consequence of T_2. Suppose first that x is false. Then by the second requirement, again, this false x must be in T_1. Suppose next that x is true; then f-iff-x is in T_2's falsity-content and again must be in T_1. But since T_1 already contains both f and f-iff-x, it will also contain this true x. Thus every x, whether true or false, in T_2 is also in T_1; T_2 has no excess content, and hence no excess truth-content, over T_1. Popper's third requirement cannot be met.

(10) Popper's critics did not attach much importance to his concept of verisimilitude in its heyday; but after its debacle in 1974, news of which spread rapidly, some of them came to see it as the central plank of his philosophy of science, the whole system being brought down by its collapse. Popper's estimate of its importance changed in the opposite way. When he first introduced this idea he had indeed made it a, if not *the*, central plank of his philosophy of science, saying that we simply cannot do without something like it (1963 p. 232) and equating scientific progress with increasing verisimilitude. After the debacle his estimate flipped over. This shows up clearly in *Realism and the Aim of Science* (Popper 1982). In the main text, work on which had come to a stop in 1962, he had said that to describe one theory as better than, or superior to, another *is* to claim that it comes nearer to the truth (p. 25); and he even suggested that the problem of induction can be solved by replacing *truth* with 'better *approximation to the truth*' (p. 67). But in his new Introduction, dated 1982, he remarked parenthetically that the idea of verisimilitude was not an essential part of his theory (p. xxxvii).

(11) Because attention has mainly focused on defects in Popper's definition of verisimilitude, not much attention has been paid to the role he was giving to verisimilitude in his later philosophy of science. That is what I now turn to. Various attempts have been made to replace his definition by a viable one, for instance Kuipers (1982, 1987*a*), Niiniluoto (1987), Oddie (1986), and Schurz & Weingartner (1987). I have not attempted to assess their comparative merits, partly because of the escalating technicality of the discussion. But I now assume for argument's sake that an adequate definition exists; and I now ask: how satisfactory would Popper's equation of the aim of science with ever-increasing verisimilitude have been if the latter had been adequately defined.

(12) What was Popper taking as the aim for science before he hit on the idea of verisimilitude? Well, from *Logik der Forschung* (1934) to 'The Aim of Science' (1957) he was saying that science aims at ever better *explanations*, and that choices between competing explanatory theories are controlled by *corroborations*. He said some very interesting things about how, in the best case, a theory should stand to its predecessor(s) to constitute a better explanation. The bottom line was that it should be more corrobor*able*, and go on to become better corroborat*ed*.

(13) Such a theory will not be certainly true or probably true, but it will be possibly true in the sense that it is well tested and, as yet, unfalsified. So I proposed in *Science and Scepticism* that so far as truth is concerned science aims at theories that are possibly true. This brought Alan Musgrave's wrath upon me: science aims at *truth*, not at possible truth (see e.g. his 1989, pp. 302–3). Miller agrees with Musgrave here. Now I of course accept that the idea of truth is a regulative ideal for science: truth is what science aspires after. But to aspire after X is not equal to aiming at X. A schoolboy who dreams of being a military hero does not yet have military aims. If one is to aim at X, and pursue one's aim rationally, one needs to be able to monitor the success or failure of one's attempts to achieve X. Are Popperians entitled to claim that one could do so if X were simply *truth*? Here is a simplified version of what, for us, would be a paradigm of scientific progress. Within some problem-situation a powerful new theory T_1 is advanced. It is tested and for a time it only wins corroborations. But then a more corroborable theory T_2 is advanced. Crucial experiments between it and T_1 are performed, and they go in favour of T_2. The splendid T_1 has fallen in battle. Later, the pattern is repeated, with T_2 being refuted and superseded by the more corroborable T_3. Was science fulfilling the aim of truth in this admirable progression? Not with T_1, which turned out to be false, nor with T_2 which suffered the same fate. Perhaps this aim was fulfilled with T_3? Well, we may learn that it was not but we'll never learn that it was.

(14) But if X were ever better corroborated theories, science could monitor very well the success or failure of its attempts to achieve X. In our idealized progression the latest theory's pattern of corroborations dominates its predecessors', with every test-result that corroborates them also corroborating it, while some test-results which corroborate it are either neutral to earlier theories, being in an area where they are silent, or discorroborate them. Of course, corroboration situations may be messy; but in this clearcut situation Popper's pre-1960s vintage methodology said that it is rational provisionally to accept T_3 as the currently best corroborated theory in its field.

(15) This was the cue, back in those pre-verisimilitude days, for critics of this purportedly non-inductivist philosophy to ask *why* the best corroborated theory is the best theory. As Feyerabend might have put it, what's so great about corroboration? If Popperian corroboration is free of any tincture of inductivism, if a corroboration-appraisal merely compares the past performances of the theories under test, of what interest is it, except to historians, to know that T_2 is better corroborated than T_1? Perhaps T_1 will perform better in the future.

(16) In *Science and Scepticism* I attempted to answer this question without bringing in verisimilitude and in a way that would preserve the non-inductive character of this philosophy of science. I will come back to that later. In the meanwhile let us ask what use Popper made, in the 1960s and 70s, of his new theory of verisimilitude in his account of progress and rationality in science. The short answer is that he used it to turn what many saw as a pessimistic philosophy, in which the truth is permanently hidden, into an optimistic philosophy in which we can know, or at least have reason to believe, that we are making progress with respect to truth. Corroboration, he said, though not a measure, is an *indicator* of verisimilitude (1972 p. 103, his emphasis). An old inductivist view was that if, say, twenty pairs of experimental values are found to fit a simple linear function $y = f(x)$, then this function very probably represents a law of nature since it would otherwise be extremely improbable that just these values would have obtained. Hermann Weyl criticised this on the ground that all sorts of other mathematical functions could be defined which these values will equally satisfy; and in *The Logic of Scientific Discovery* Popper went along with that (1959 pp. 139–140, and *n1). But after the introduction of verisimilitude his position changed. He now declared that a claim like, 'It is highly improbable that Einstein's theory would make predictions that are precise and correct unless it were true' becomes valid when 'true' is replaced by 'high degree of verisimilitude' (1974 p. 1192). He also wrote: 'If two competing theories have been criticized and tested as thoroughly as we could manage, with the result that the degree of corroboration of one of them is greater than that of the other, we will, in general, have *reason to believe* that the first is a

better approximation to the truth than the second' (Popper 1982, p. 58, his emphasis). In short, corroboration-appraisals provide some justification for the corresponding verisimilitude-appraisals.

(17) It seems clear that an inductive element has been let in here. Consider the following scenario. T_1 and T_2 respectively entail conflicting singular predictive implications SPI_1 and SPI_2; these are relevant to some urgent technological problem, say to prevent a nuclear catastrophe. There has as yet been no test on the two theories with respect to predictions of these kinds, but the theories have been severely tested in other places, and T_2 has emerged as unambiguously better corroborated than T_1. If that gives us reason to believe that T_2 is nearer the truth than T_1, then we surely have some reason to prefer SPI_2 to SPI_1. Let us use 'reliable', in analogy with 'desirable' and 'preferable', to mean 'worthy of being relied upon'. Popper once asked rhetorically: 'But do I not really draw inductive conclusions from past performance to future performance?' (1982 p. 66). The answer seems clear. To proceed from evidence about past instances to a categorical conclusion about the next instance is of course an inductive inference; and Popper insisted that it would still be inductive if it proceeded only to a probabilistic conclusion. To which we may add that it would still be inductive if it proceeded to such a conclusion in several steps. In the present case, we proceed from evidence as to how T_1 and T_2 have performed under test in the past via a corroboration-appraisal to a verisimilitude-appraisal and thence to a conclusion about their relative reliability in the future. Inductivists may exclaim, 'So he's one of us after all! Good for him.' But I find it sad when a philosophy of science whose proud claim was to have dispensed with induction covertly reneges on that claim.

(18) Is there any way in which Popperian ideas of rational choice between competing scientific theories, and of theoretical progress in science, can be upheld without a resort to some form of inductivism? I think there is. Popper had the valuable idea, which I exploited in *Science and Scepticism*, of separating justification of *preferences* for statements (hypotheses, theories) from justifications of the statements themselves. I will suggest that he misused this distinction but that it can be used to solve our problem.

(19) Justificationists usually see justification as allowing of degrees, with 'verification', 'proof', etc. reserved for limiting cases; they would say that to justify a statement you need arguments which go some way towards establishing it as true, or at least as close to the truth; actually to establish its truth, if that were possible, would justify it conclusively. And with respect to competing hypotheses they would say that to justify hypothesis a against hypothesis b you need arguments for a being more probable, or perhaps more truthlike, than b. It seems obvious that the justification of a preference for a

over b, whether these are statements or entities of some other kind, must be with respect to some property (or complex of properties), call it ϕ. You are free to prefer lemonade to champagne; but if you seek to *justify* your unusual preference you must bring in some property, say keeping a clear head, with respect to which lemonade tends to perform better than champagne. If the foregoing elucidations are accepted, it follows that to justify statement a is the very same thing as to justify a preference for a with respect to truth. One may justify a preference for a with respect to ϕ without justifying a only if ϕ is not truth, or some relative of truth such as verisimilitude.

(20) In *Realism and the Aim of Science* (pp. 19-20) Popper distinguished between three problems: that of adjudicating between competing scientific theories, that of justifying scientific theories, and that of showing one scientific theory to be *preferable* to another. Whereas justificationist philosophers subordinate adjudication and preference to justification, Popper claimed to take an attitude to the problem of justification that was 'as *unambiguously negative* as that of any irrationalist or sceptic'. His idea was to leave out justification and solve the problem of adjudication via the problem of preference. But now comes a big letdown: he added that by a theory being preferable to another he meant that we have *reasons* to think it *a closer approximation to the truth* (p. 20, his emphasis). At another place, after saying that there is a world of difference between justifying a *preference* and justifying a *theory*, he immediately added: 'To justify a theory is to show that it is true. But we may justify a preference, even for a false theory, if we can show that of all the competing theories it appears to come nearer to the truth than any of the others' (1994 p. 138). His equation of justification with conclusive justification is a bit rich, coming from someone who spent much time attacking probabilistic versions of justificationism, like those of Keynes, Reichenbach and Carnap, and who insisted that the problem of justifying inductive conclusions remains the same if one puts 'probable' in front of 'conclusions' (1972 p. 4). Once it is accepted that justification allows of degrees, it seems clear that to justify a preference for a theory with respect to verisimilitude would tend to justify the theory itself. His later philosophy was tainted by justificationism as well as by inductivism.

(21) A quick word now about the view of scientific progress and rationality taken in my *Science and Scepticism*. Call a property ψ of a theory contingent or inherent according to whether or not possession of ψ depends on factors external to the theory. Thus the consistency of a consistent and falsified theory is an inherent property, while its falsity is a contingent property. And a theory is inherently ψ-er than another if degrees of ψ-ness depend only on features of the theories and are independent of factors external to them. Thus T_2 may be inherently simpler, say in a "paucity of parameters" sense, than T_1. But if T_1 and T_2 are neither tautological nor self-contradictory but

regular scientific theories, then T_2's being closer to the truth than T_1 would be a contingent property.

(22) At one extreme, a proposed aim for science might erect one contingent property, such as being verified, into the sole desideratum. The aim proposed in *Science and Scepticism* goes as far as it can in the other direction. All but one of the properties of theories which it holds up as desiderata are inherent properties: its B-pole, as I call it, says that science should aim at theories that are ever deeper, more unified, more predictively powerful and exact. The great advantage of such inherent properties in the present context is, of course, that they are outside the reach of inductive scepticism. Presented with two theories suitably formulated in a language for whose predicates there is an agreed partition into observational and theoretical, logical examination should enable one to tell which is ψ-er when ψ is one of these properties. It turned out that the above four properties can be collapsed into two: deeper, and wider. Suppose that theory T_2 has been found to be deeper and wider than T_1, its only serious rival. In that case it will typically happen both that some of T_2's testable content diverges, if only slightly, from T_1's and also that T_2 has excess testable content over T_1. This brings me to this aim's A-pole. For T_2 to be accepted as an advance over T_1 there needs to have been at least one test on it in areas of divergent or excess content, and its pattern of corroborations should dominate T_1's in the way mentioned earlier, with every test-result that corroborates T_1 also corroborating it, and some test-results which corroborate it either neutral to or discorroborating T_1.

(23) I claimed this aim to be the optimum aim for science; it contains what can be retained of the Bacon-Descartes dream for science when this is de-utopianized and rendered feasible. Like Popper's philosophy of science, this revised version gives corroborations a decisive role. But its way of justifying a preference for the best corroborated theory in its field is not like the way he started talking in the 1960s and early 1970s; it does not treat corroborations as indicators of verisimilitude, thereby surreptitiously turning a justification for a preference for a theory into a justification of the theory. In suitable cases it justifies a preference for the better corroborated T_2 over T_1 on the ground that T_2 is better than T_1 with respect to the optimum aim of science, where the latter does not require the thus preferred theories to be certainly true, or in some sense probably true, but only possibly true in the old Popperian sense of having been severely tested and surviving so far.

References

Keuth, H. (1976), 'Verisimilitude or the Approach to the Whole Truth', *Philosophy of Science* pp. 311–336.

Kuipers, T. A. F. (1982), 'Approaching Descriptive and Theoretical Truth', *Erkenntnis* **18**, 343–378.

Kuipers, T. A. F. (1987*a*), Contributions, in *What is Closer-to-the-Truth?* (Kuipers 1987*b*), pp. 1–7, 79–99, 177–186.

Kuipers, T. A. F., ed. (1987*b*), *What is Closer-to-the-Truth?*, Rodopi, Amsterdam.

Miller, D. (1974*a*), 'On the Comparison of False Theories by their Bases', *The British Journal for the Philosophy of Science* **25**, 178–188.

Miller, D. (1974*b*), 'Popper's Qualitative Theory of Verisimilitude', *The British Journal for the Philosophy of Science* **25**, 166–177.

Miller, D. (1994), *Critical Rationalism: a restatement and defence*, Open Court, Chicago.

Musgrave, A. (1989), Saving Science from Scepticism, *in* F. D'Agostino & I. C. Jarvie, eds, 'Freedom and Rationality: essays in honor of John Watkins', Kluwer, Dordrecht, pp. 297–323.

Niiniluoto, I. (1987), *Truthlikeness*, Reidel, Dordrecht.

Oddie, G. (1986), *Likeness to Truth*, Reidel, Dordrecht.

Popper, K. R. (1934), *Logik der Forschung*, Springer, Vienna.

Popper, K. R. (1957), 'The Aim of Science', *Ratio* **1**, 24–35. Reprinted in (Popper 1972), pp. 191–205.

Popper, K. R. (1959), *The Logic of Scientific Discovery*, Hutchinson, London. English translation, with new preface, footnotes and appendices of (Popper 1934).

Popper, K. R. (1963), *Conjectures and Refutations*, Routledge, London.

Popper, K. R. (1972), *Objective Knowledge*, Clarendon Press, Oxford.

Popper, K. R. (1974), Replies to my Critics, *in* P. A. Schilpp, ed., 'The Philosophy of Karl Popper', Vol. XIV of *The Library of Living Philosophers*, Open Court, Illinois, pp. 961–1197 (Book II).

Popper, K. R. (1976), 'A Note on Verisimilitude', *The British Journal for the Philosophy of Science* **27**, 147–159.

Popper, K. R. (1982), *Realism and the Aim of Science*, Hutchinson, London.

Popper, K. R. (1994), *Knowledge and the Body-Mind Problem: In defence of interaction*, Routledge, London and New York. Edited by M. A. Notturno.

Radnitzky, G. & Andersson, G., eds (1978), *Progress and Rationality in Science*, Reidel.

Schurz, G. & Weingartner, P. (1987), Verisimilitude Defined by Relevant Consequence-Elements, *in* Kuipers (1987*b*), pp. 47–77.

Tichy, P. (1974), 'On Popper's Definitions of Verisimilitude', *The British Journal for the Philosophy of Science* **25**, 155–160.

Vetter, H. (1977), 'A New Concept of Verisimilitude', *Theory and Decision* **8**, 369–375.

Watkins, J. (1984), *Science and Scepticism*, Princeton University Press and Hutchinson, Princeton and London.

Watkins, J. (1987), 'A Note on Incongruent Counterparts and Verisimilitude', *Erkenntnis* **26**, 295–300.

Author Contact Information

John Watkins

London School of Economics
Houghton Street,
London, WC2A 2AE,
United Kingdom

Copyright

Retrieval

This article is electronically retrievable, in various formats, via the World Wide Web.

The root URL for the HTML format article is:

`http://www.eeng.dcu.ie/~tkpw/tcr/volume-02/number-02`

For information on availability of alternative formats please see the volume index at URL:

`http://www.eeng.dcu.ie/~tkpw/tcr/volume-02`

About The Critical Rationalist

The Critical Rationalist (**TCR**) is a scholarly academic journal, with peer review, founded to facilitate the application, elaboration, and criticism of Popperian philosophy in general, and of Critical Rationalism in particular. **TCR** strives to examine Popper's own philosophy in a critical light, to explore new applications of his philosophy, and to make this accessible to academics and the general public alike. In addition to independent contributions, **TCR** carries articles presented at the **Annual Conference on the Philosophy of Sir Karl Popper**.

TCR supports and solicits the ongoing submission of *Critical Commentary* relating to all target articles.

TCR is disseminated *electronically* in a variety of *presentation formats*, including both Postscript (for local printing) and HTML (for interactive, hypertext viewing, via the World Wide Web or otherwise). Electronic dissemination permits a flexible publication schedule: articles are published as soon as possible after final acceptance. Accordingly, each **TCR** issue normally contains a single article.

Authors of accepted manuscripts assign to **TCR** the right to publish and distribute their text and to archive and make it permanently retrievable, in any media, commercially or otherwise; but they retain the copyright. After it has appeared in **TCR** authors may republish their text in any way they wish—electronic or print—as long as they clearly acknowledge **TCR** as its original locus of publication.

For further information please consult the **TCR** Web site at:

`http://www.eeng.dcu.ie/~tkpw/tcr/tcr-home`

or contact the editors at:

`tcr-editors@www.eeng.dcu.ie`