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Objective Knowledge

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0 Introduction Karl Popper's *Objective Knowledge* stands at the threshold of his last major philosophical phase, the period from his retirement from the London School of Economics in 1969 until his death in 1994. The two great books that he wrote before he came to London, *Logik der Forschung* (1934) and *The Open Society and Its Enemies* (1945), contain much more than the innovations in the theory of scientific method and the theory of democracy for which they are famous. *Logik der Forschung*, translated into English as *The Logic of Scientific Discovery* (1959), is by no means just a tract, even a revolutionary one, on the methods of science, since about one third of the text is devoted to searchingly original treatments of the frequency theory of probability and of the interpretation of quantum mechanics, whilst the unpublished manuscript on which the book was based, *Die beiden Grundprobleme der Erkenntnistheorie* (1979), at whose content it often hints, presents a wealth of significant material on the psychology of human learning and its biological context. Popper had, after all, been a student of Karl Bühler, and had absorbed the theories of problem solving developed within the Würzburg School (Popper 1974a, § 15). *The Open Society and Its Enemies*, for its part, is not so much a book defending democratic liberalism as a defence of democratic liberalism nestling within a book that discusses every other topic under the sun. It offers profound and provocative studies of the thought of Plato and Marx (see Hacoen 2000, Chapter 9, for a critical appreciation), an abundance of scholarly, if often controversial, historical interpretations of the work of Heraclitus, Aristotle, Hegel, J. S. Mill, Wittgenstein, Mannheim, and others, the elements of a new theory of rationality, and many elucidations of the workings of science that go beyond what is explicit in *Logik der Forschung*. One insight worth mentioning, since it is little known, is the clear recognition in section II of Chapter 11 that most important advances in science are revolutionary, not cumulative; a commonplace today

The foreword ('Euskarazko Ediziorako Hitzaurrea') to Karl R. Popper, *Ezagutza objektiboa. Ikusmolde ebolutibo bat*, the Euskarán edition of *Objective Knowledge*, translated by Alberto Gabikagojeaskoa, edited by Andoni Ibarra, and published in 2003 by Klasikoak S. A., Donostia / San Sebastián. It is a revised and enlarged version of the afterword ('Posleslovie k russkomu izdaniyu *Ob'ektivnogo Znaniya*') to the Russian edition, *Ob'ektivnoe Znanie. Evolutsionny podkhod*, translated by Delir G. Lakhuti, edited by Vadim N. Sadovsky, and published in 2002 by Editorial URSS, Moscow. The present version (2011) contains some minor typographical and stylistic changes, and (in blue print) some new footnotes and bibliographic items.

perhaps, but one that many philosophers attribute to Kuhn (1962). Another insight, also often forgotten, is the emphasis in Chapter 23 on the social character of science, which acts as the main custodian of its claim to objectivity (Jarvie 2001).

The richness and variety of his intellectual background, and of his published works, notwithstanding, it is not inaccurate to think of Popper during most of his professional career in Britain (1945–1969) as something of a specialist: primarily a philosopher and methodologist of science, especially physical science, and secondarily, though more by reputation than in practice, a social and political philosopher. His particular interest in the philosophy of physics was further cultivated in Volumes II and III of *The Postscript to the Logic of Scientific Discovery* (written in 1950–1957, published in three volumes in 1982–1983); while Volume I, and the other principal record of his work in the period 1950–1965, *Conjectures and Refutations* (1963), for the most part tackle logical and methodological problems arising in the study of physical science. In his last 25 years, however, Popper moved substantially into new areas: the philosophy of biology, the philosophy of mind, and the general theory of knowledge. To be sure, he continued to be fascinated by the old problems of scientific method, of probability, of quantum mechanics, of democratic theory, and of Greek philosophy, and substantial contributions to all these fields were published near the end of his life or shortly after his death. But the emphasis did change, as he began to develop his philosophy of problem solving into a general theory of life, of nature, and of man’s extraordinary place in nature. *Objective Knowledge* testifies strikingly to this transition.

Like Monteverdi’s *Vespers*, the book starts retrospectively, looking back to previous achievements before breaking out in new and unanticipated directions. The first chapter, aptly entitled ‘Conjectural Knowledge’, reconsiders Hume’s problem of induction, which Popper thinks (rightly, in my opinion) that he solved in essentials in *Logik der Forschung*, and proceeds to extend his solution from the arena of theoretical science to that of practice. About this extension, which has been widely discussed and almost as widely dismissed, I say more below. The simple solution to Hume’s problem was to scrap the traditional assumption that the theories and hypotheses of science are obtained by a procedure, in sore need of justification, of inductive inference from instances, or from experience, and to replace it with the assumption that they are guesses or conjectures that need no justification. The ingredient of rationality in scientific research is not repudiated in this new perspective, as it was in traditional scepticism, but transferred from the production of hypotheses to their criticism or refutation, which, Popper proposed, is conducted in an impeccably deductive manner. That is to say, although conjectures are admittedly the outcomes of moves in the ‘inductive direction’ — the most important of them are universal or statistical generalizations — they are not inferences; and although refutations are undeniably the outcomes of logical inferences, in particular the deduction of testable predictions, they are not inductive. The philosophy of conjectures and refutations, as it came to be called, in this way renders redundant the entire theory of inductive logic, together with all its obscurities and contradictions.

‘Two Faces of Common Sense’, the second chapter of the book, also glances backwards occasionally, but for the most part it moves confidently forwards. Popper’s intention in this long and somewhat uneven chapter, he tells us, was to provide ‘a fairly full answer to the critics of . . . (his) views on science’ (p. 32). Although he plainly had in mind the critics at the LSE itself, especially Lakatos, who in an unfortunate series of papers (1968a), (1970), (1974), around this time was promoting the idea that in *Logik der Forschung* neither the problem of induction nor the problem of demarcation had been solved, and

also the criticisms advanced by Kuhn (1962), Popper does not here directly answer these criticisms. (For some answers the reader must look at the reply to Lakatos on pp. 999–1013 of Popper 1974b, at Popper 1970, and at the 1982 Introduction to Popper 1983.) What he does is to mix together some familiar themes with some that are new, or anyway not given very much attention in his philosophy up to this time (though they recur more frequently later). The old themes include, for example, truth and verisimilitude (§§ 6–11), topics first discussed at length in Chapter 10 of *Conjectures and Refutations*, and returned to in Chapter 9 of the present volume; and the commonsense or ‘bucket theory of the mind’ (§§ 12–15, 19–22), which Popper first described in this way in Chapters 23 and 25 of *The Open Society*, and a little later in 1948 in a remarkable lecture here reprinted as Appendix 1. Themes treated in Chapter 2 in greater depth than in earlier writings include his defence in §§ 4–5 of commonsense realism (see also Popper & Eccles 1977, Chapter 1, and, from another perspective, Chapter 8 of this volume), and his explicit endorsement of a version of evolutionary epistemology, about which I say more below. The chapter ends with an ‘Afterthought on Induction’ in which Popper tries to disentangle the logical problem of induction from Hume’s ‘bucket-theoretical search for the origin or the basis of the idea of (causal) necessity’ (p. 88).

The central theme of *Objective Knowledge*, announced in its title, is that scientific hypotheses are not only not justified, and not accessible to justification, they are not beliefs either, and in a clear sense are not accessible to belief. A subsidiary theme, already alluded to, is that scientific hypotheses are usually not even true, and that although true hypotheses may sometimes be achieved, and are to be prized, we do well to see science as striving to approach the truth rather than to attain it. In short, though Popper does not put it this way, the greater part of scientific knowledge is unjustified, untrue, unbelief (Miller 1994, p. 54*). Scientific knowledge, as Popper understands it, is an extension of commonsense knowledge, and has much in common with it, but it has little affinity with what is conventionally regarded by professional philosophers as the subject matter of the theory of knowledge. This no doubt explains why Popper is still hardly known as an epistemologist, and his name does not appear in many anthologies on epistemology.

The doctrine of the objectivity of scientific knowledge is foreshadowed in various sections of *Logik der Forschung*, especially § 8 and § 27, and also (as already mentioned) in Chapter 23 of *The Open Society and Its Enemies*. Precursors whom Popper acknowledges include Plato, Kant, Hegel, Bolzano, Frege, and Heinrich Gomperz (pp. 122–127, 152). Where *Objective Knowledge* goes far beyond these ideas is in its detailed attention to the biological setting of human knowledge. Strange to say, it is biological considerations that bring to light the decisively non-psychological character of scientific knowledge, and of much of our commonsense knowledge too, and enable Popper to contrast it with subjective or dispositional knowledge, both human and animal. Knowledge is presented here as an abstract human construction that improves by exosomatic (non-genetic) means man’s adaptation to the world. Apart from language itself, science is, from the rationalist perspective, the most important part of Popper’s world 3, the world of abstract ideas, of linguistic and artistic creations, of ‘culture’ in some dignified sense of that easily abused term. It must be emphasized that the inhabitants of world 3 are human creations, rather than eternal (as in Plato and Frege), they are theories rather than concepts (as in Plato); and although they develop, they do so under the pressure of problems tackled by human thinking, and have no internal dynamic (as in Hegel).

* At the University of Warwick, in the summer term of 1971, I gave a series of 12 lectures entitled UNJUSTIFIED UNTRUE UNBELIEF as part of the third-year undergraduate course *Epistemology & Metaphysics*.

Chapter 6 (‘Of Clouds and Clocks’) is historically the first of the three chapters that expound the theory of world 3, and display its versatility in the analysis of issues in the philosophy of mind, the philosophy of mathematics, and the practice of historical interpretation. The title derives from section 6 (‘Clocks and Clouds’) of volume II of Popper (1982a). (It is of some interest that ‘Clocks and Clouds’ is the title of a choral work written in 1972–1973 by the Hungarian composer György Ligeti. There can be little doubt that he learnt the phrase from Imre Lakatos,[†] a fellow exile and exact contemporary who made uncontrolled use for his own purposes of a set of *Postscript* proofs in his possession.) But Chapter 6 of the present book goes beyond the indeterminism of *The Open Universe* (as originally written) by holding that for there to be freedom and inventiveness, ‘indeterminism is not enough’ (p. 226). For there to be either art or science, the physical world must contain not merely causal gaps but causal gaps that are open to exploitation by purposeful thinkers. Indeed, if we could not manipulate parts of the physical world at will, scientific hypotheses could not be conjectured and they could not be tested either. This is the start of the idea of world 3, a largely autonomous domain that can interact with the physical world through the mediation of human mental activity. The importance of world 3 to our understanding of the human self forms the core of Popper’s contribution to *The Self and Its Brain*, written jointly with Sir John Eccles and published in 1977.

Popper distinguishes at least three worlds or subworlds of the universe in which we live. World 1 is the world of physical objects and processes. World 2 is the world of mental activity — often held to be merely a part of world 1, but still distinguishable from it. World 3 is the world of the abstract products of human thought — most importantly language itself, but in addition, specific items such as scientific theories, works of art, and perhaps also social institutions. Popper likens these products, created by us but in some sense independent of us, to the material productions of some animals: birds’ nests, spiders’ webs, beavers’ dams (p. 112). Like these artefacts, they require some degree of embodiment in one of the other two worlds — principally in world 1, where they appear as printed, painted, performed, or electronically stored objects, but to some extent also in world 2, where they are entertained temporarily in live thought processes (but not necessarily as beliefs). Anti-psychologism is of course an old methodological thesis of Popper’s (1934, §§ 2, 8), but in the theory of the three worlds it takes on a metaphysical dimension. The existence of subjective knowledge is not denied, but it is identified with a network of biological dispositions (Chapter 2, § 20).

Popper marks out as particularly significant inhabitants of world 3 what he calls objective problems and objective problem situations. His well known general schema

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$$

of problem solving (initial problem \rightarrow tentative theory \rightarrow error elimination \rightarrow subsequent problem), mentioned on a number of occasions in the book (on pp. 126 and 164, and, in a generalized form, on pp. 243 and 287), ‘may be regarded’, he says, ‘as resulting from the critical interpretation of the (non-Hegelian) dialectic schema’ discussed in the early paper ‘What Is Dialectic?’ (1963, Chapter 15). The growth of science is claimed largely to be an activity taking place in world 3 in accordance with this schema, since it is the objective logical relationships among theories and the problems that the theories address that are important for their development — and certainly for understanding their development — and not any subjective or psychological processes in which they are involved. It is

[†] Since the Lakatos Archive at the LSE contains, I am informed, no correspondence with Ligeti, this assertion must be treated with caution.

of course admitted that the objects in world 3 need to be apprehended or grasped in subjective thought processes in order, as it were, to come to life. On the other hand, a book that is unread remains dormant rather than extinct for as long as the possibility of its being understood persists (even if the language in which it is written goes out of use). Popper holds that the grasping of a world 3 object involves a kind of re-creation of it; understanding a problem, in particular, consists in trying to solve the problem and failing (Chapter 4, § 10). Hence arises the non-justificationist conception of an expert not as someone who has authority by virtue of his commanding knowledge, but as someone who has made most of the obvious mistakes, and many of the unobvious ones, and has a pretty shrewd idea what will not work (see also Popper 1994, p. 99, and compare Bohr's remark, reported by Infeld 1941, p. 208, that 'an expert is a man who by bitter experience has learned of all the possible mistakes in his restricted field').

It is an important part of Popper's thesis that world 3, though the product of the workings of innumerable minds, is to a considerable extent autonomous; problems emerge in world 3 that were never put there by anyone. A standard example, often repeated, is that although the natural numbers were created by human mental activity (pp. 118, 138, 160), there are many arithmetical problems and theorems, such as the question of whether Goldbach's conjecture is true or false, that were not foreseen by anyone, and had to be discovered. This view may be contrasted with that of Gödel (1951), who held that the creator of an abstract object has complete control over its properties (as a novelist has complete control, one might think, over his characters). Gödel's moral, of course, is that mathematical objects are not humanly created but independently existing. In any event, Popper's view demonstrates that some compromise is possible between constructivism and platonism with regard to mathematical objects.

Of the few critical discussions in the literature of Popper's theory of objective knowledge, the most valuable are Carr (1977), Currie (1978), Cohen (1980), and Mackie (1985).[‡] One insistent problem is the extent to which we can accurately speak, as Popper often does speak, of the creation of genuinely new ideas. If there is an internal logic within world 3, as there has to be if it is to realize the autonomy that Popper requires, then this must consist, at least in part, of recursive rules. Indeed, if the natural numbers are the result of human thought, all of them, then this must be because we have consciously created some of them and consciously created as well a rule by which new numbers may be constructed from numbers already created. But given that most developed natural languages possess something fairly close to a recursive syntax, we should be able to conclude in the same way that all possible English sentences are already within world 3. The problem here is provoked not by logic (as suggested by Cohen (1980), § 4), but simply by grammar. Now if all linguistic expressions are already available in world 3, in what sense can we talk of the invention of new ideas? It might be claimed that although Newton's theory existed before Newton, it took Newton's genius to see its relevance as a theory of the heavens. No doubt genius was involved. But the statement that Newton's theory provides a solution to the objective problems posed by Kepler, Descartes, and Galileo, was also an inhabitant of world 3 before Newton. So again it seems as if Newton merely discovered something that was already there, and in no sense did he create something new. This consequence might just about be tolerable with regard to science, but it is surely less acceptable with regard to artistic creation. The same problem arises, after all, with regard to music, and even more sharply. There is still plenty of room for genius, to be sure, since no problem solver

[‡] To this list should now be added Niiniluoto (2006).

can feasibly search systematically through all possible strings of symbols for a solution of his problem. But real creativity remains elusive.

As I have said, the papers in *Objective Knowledge* are rather original in the way that they present human knowledge as a truly biological phenomenon. Still, the structural similarity between the process of conjectures and refutations on the one hand, and evolution by Darwinian natural selection on the other, had been recognised by Popper much earlier, and contrasted with the similarity between Humean induction by repetition and Lamarckian instruction. But to compare the competition between hypotheses with the competition between organisms, or between species, is not to rise very far above the level of illuminating metaphor. It is only when the inhabitants of world 3 are seen to be genuine (though exosomatic) organs, rather than organisms, that their development can be understood as a special kind of biological development. This way of articulating evolutionary epistemology is not dissimilar to the theory of memes of Dawkins (1976) (see Mackie 1985).

There are several controversial claims in *Objective Knowledge* that are anything but settled after 30 years, and deserve more discussion than they have yet received. One is the solution to the pragmatic problem of induction that is outlined in §9 of Chapter 1. At the time that *Objective Knowledge* was being assembled, in the summer of 1971, Popper was labouring over his extensive ‘Replies to My Critics’ (1974b) in *The Philosophy of Karl Popper*, a volume in THE LIBRARY OF LIVING PHILOSOPHERS edited by Paul Arthur Schilpp. In introducing Part III of these Replies, which was devoted to a handful of essays about the problem of induction, Popper took the opportunity to reproduce some parts of the present Chapters 1 and 2, including the section in question. What is not obvious to the reader of *Objective Knowledge* is that some additional remarks about the pragmatic problem of induction were inserted into the text of the Replies. These remarks, made at my suggestion, seem to me to be worth sketching here, since they revive the idea (clearly stated in §20 of *The Poverty of Historicism* 1944) that scientific theories tell us what cannot be achieved rather than what can be achieved. It follows that we should not talk, as §9 unfortunately does, about ‘preferring’ one theory to another as a ‘basis for practical action’, for theories do not make practical recommendations. (This point seems to be clearly appreciated in §VIII of Appendix 1. It is elaborated in §3 of Miller 2002.) What is in dispute when we are pondering how to act, what may loosely be said to be the ‘basis for practical action’, is not any theory but a practical proposal (often an invention). Once this logical point is seen, it becomes almost trivial that the best proposal is the proposal that best withstands criticism. This is where theories enter; they provide criticism, but they do not provide more. In particular, they do not provide any positive suggestions about how to proceed.[§] When Popper finally says that ‘it is perfectly possible that the world as we know it, with all its pragmatically relevant regularities, may completely disintegrate in the next second’ (p. 22), he does not mean that the regularities may disintegrate (though of course they may). What he means is that it is not the regularities or the theories that we adopt that bear the responsibility for the practical successes that we enjoy.

Another neglected thesis of *Objective Knowledge* that I should like to draw attention to is the suggestion that ‘the method of trial and of the elimination of errors . . . can be said not to be an empirical method but to belong to the ‘logic of the situation’ (p. 70). Having on the previous page raised against the theory of variation plus natural selection the familiar accusation of unfalsifiability, Popper goes on to suggest that in characterizing Darwinism

[§]For a more elaborate discussion of the pragmatic problem of induction, see Miller (2009), §3.

as a version of situational logic he has identified its ‘logical or a priori components’.[¶] By the time of his Darwin Lecture (1978) he had abandoned the view that the doctrine of the survival of the fittest is empirically empty. But as far as I am aware, he never disavowed the idea that it is by the method of trial and error alone, and by no other method, that organisms, including humans, can come to know what they do not already know.

Objective Knowledge is dedicated to Alfred Tarski, and it is therefore appropriate to conclude this foreword by looking at Popper’s treatment of a number of issues relating to Tarski’s theory of truth. The first is the question of what the real philosophical significance of Tarski’s theory of truth was. It is well known that, in Popper’s judgement (Chapter 2, § 6, Chapter 8, § 4, Chapter 9, § I; see also 1963, Chapter 10, § 3), Tarski rescued from dishonour the ancient and much maligned classical or correspondence theory of truth. The judgement is underwritten by the idea that the instances of the Tarski’s T-schema ‘*P* is true if & only if *p*’, where ‘*P*’ is replaced by a structural-descriptive name (or quotation-name) of an object-linguistic sentence and ‘*p*’ is replaced by the translation into the metalanguage of the sentence itself, indicate, as Popper puts it, ‘just what fact a statement *P* will correspond to if it corresponds to any fact: namely the fact that *p*’ (p. 45); ‘Snow is white’ is true if and only if the fact that snow is white is there for it to correspond to. It must be admitted that these days, when minimalism and deflationism predominate in the theory of truth, this realistic understanding of Tarski’s T-schema is not much in favour; especially when combined with the realistic attitude towards facts that Popper displays, apparently going beyond the more cautious statement in *Conjectures and Refutations* that ‘(f)acts are something like a common product of language and reality; they are reality pinned down by descriptive statements’ (1963, Chapter 9, § IX). Popper goes so far as to say here that ‘it is not his successful description of a method for defining “true” which makes Tarski’s work philosophically so important, but his rehabilitation of the correspondence theory of truth, and the proof that there is no further difficulty lurking here once we have understood the essential need for a semantical metalanguage which is richer than the object language and its syntax’ (p. 328). Now it is not hard to agree with him (1945, note 39 to Chapter 11) that Tarski’s motivation in defining truth was not a desire to answer the question ‘What is truth?’; he was no essentialist, and he cannot be understood (though sometimes he is) as providing a conceptual analysis, or explication, of the word ‘truth’ or of the expression ‘true sentence’ (Miller 1999, p. 58). But the final remark just quoted underlines, rather than diminishes, the virtue of Tarski’s achievement in providing an explicit definition of truth avoiding semantical primitives, since it was in this way that he showed that ‘there is no further difficulty’ (that is, that with care the semantical paradoxes can be avoided).

Tarski’s definition shows too, I venture to suggest, that there is indeed something that the true sentences of a language have in common, though it is a more subtle question whether what they have in common is that they correspond to the facts. Those who accept Tarski’s theory should therefore reject the current enthusiasm for minimalist theories of truth (Horwich 1990). But deflationism, the doctrine that truth is not a substantive property, is a different matter. To me it looks remarkably like an application of old-fashioned nominalism (that is, anti-essentialism). If this is right, it is easy to agree with Engel (2002), § 2.3, that Tarski’s theory of truth is deflationist. Popper too would surely have agreed that neither Tarski nor anyone else has revealed the essence or nature of truth. But this is not to say that the class of true propositions is not an interesting class (in comparison, for example, with the class of all propositions ever entertained by

[¶]For a more elaborate discussion of the a priori elements in Darwinism, see Miller (2006b).

people whose birthday is on January 1st). As the physical sciences show, the rejection of essentialist modes of thought need not be accompanied by a disavowal of the search for universal laws (1963, Chapter 3).

The investigation of verisimilitude (truthlikeness) is a field of activity for which Popper, through the failure of the suggestions of his (1963), Chapter 10, § 3, bears a considerable responsibility. I should like here to mention two aspects of this complex subject. The first is Popper's endorsement in Chapter 2, § 33, of something similar to the so-called miracle argument for scientific realism (an argument that goes back at least to Whewell and Poincaré). Popper writes: 'There is something like verisimilitude, and an accidentally very improbable agreement between a theory and a fact can be interpreted as an indicator that the theory has a comparatively high verisimilitude.' Since the section is entitled 'Analysis of an Argument from the Improbability of Accidents', it is clear that the word 'accidentally' is out of place here; a precise agreement between a theory and fact could be an accident, but the point is that this is supposed to be improbable. Popper continues: 'I do not think that much can be said against this argument, even though I should dislike its being developed into yet another theory of induction.' Since the argument has indeed been 'developed into yet another theory of induction' by other writers, and Popper himself has been accused of introducing here 'a whiff of inductivism' (O'Hear 1980, p. 67), it is perhaps for the best that, after all, a good deal can legitimately be said against the argument. Much of it is said from a Bayesian standpoint by Howson (2000), Chapter 3 (see also Miller 1994, p. 49). The crux of the objection is that the argument is valid for some distributions of probabilities, not for all, and it is profoundly unclear whether the distributions needed to make it valid hold in practice. For Bayesians, of course, this is yet another proof that all our decisions depend on our prior probabilities. Critical rationalists should conclude instead that there is never going to be a positive argument in favour of realism (or in favour of any other hypothesis). Only criticism counts.

Finally, I should like to look at the ups and downs of the thesis that 'the search for verisimilitude is a clearer and a more realistic aim than the search for truth', and especially the thesis that 'while we cannot ever have sufficiently good arguments in the empirical sciences for claiming that we have actually reached the truth, we can have strong and reasonably good arguments for claiming that we may have made progress towards the truth' (pp. 57f.). As already indicated, Popper never wavered from his judgement that Tarski had rehabilitated the correspondence conception of truth, and had shown that truth is a defensible ideal even though there is no general criterion of truth. When the idea of verisimilitude (or truthlikeness, or approximation to truth) was first introduced and defined as (very roughly) the difference between a theory's truth content and its falsity content, Popper emphasized that the assertion that one hypothesis is closer to the truth than another can likewise be only a guess (*Conjectures and Refutations*, Chapter 10, § XII). He echoes this thought on p. 58 here, and also when he says that there can be 'no criterion for the applicability' of the notion of verisimilitude (p. 335). But here he tells us that sometimes 'a preference, with respect to truth or falsity, for some competing universal theories over others' can be 'justified by . . . "empirical reasons" '(p. 8), and that this is even more markedly the case if our aim is not simple truth but increased verisimilitude. Many have been puzzled by such remarks, which look like an attempt to have a cake and at the same time to eat it.

A hypothesis T_1 that is refuted is definitely false (given the truth of the test statements involved), while an unrefuted hypothesis T_2 may be true. This is what leads Popper to say that, since we prefer truth to falsehood, we prefer the unrefuted T_2 to the refuted T_1 .

In the same way, if T_2 has greater truth content than T_1 (which is something that can be determined a priori, since it is equivalent to saying that T_2 has greater content than T_1), and apparently less falsity content (since it withstands all the tests that T_1 withstands), then a preference for high verisimilitude will lead us to prefer T_2 to its rival T_1 . For if verisimilitude grows with increases in truth content and with decreases in falsity content, then T_2 may actually be closer to the truth than T_1 is, while T_1 is definitely not closer to the truth than T_2 is. But these preferences are deductive consequences of the empirical record (and so empirically ‘justified’) only if they are nothing more than summaries of the state of the discussion. For it is evident that all that we may derive from the fact that T_1 is refuted and T_2 is not refuted (together with a statement of our preference for truth over falsehood) is that T_1 should not be preferred to T_2 . It does not follow that T_2 should be preferred to T_1 (Miller 2002, § 4). The same holds if our abstract preference is for hypotheses that possess greater verisimilitude than their rivals: if T_2 has greater truth content than T_1 , and apparently less falsity content, then all that we can deduce is that T_1 should not be preferred to T_2 . Again it does not follow that T_2 should be preferred to T_1 . Once more it is manifest that experience alone can give us only negative advice.

By the time of the second edition of *Objective Knowledge* (1979), it had become known that Popper’s own account of verisimilitude, and most others, are seriously defective. It is a theorem that if T_2 possesses more truth content than T_1 , and is false, then it possesses also more falsity content. It follows that if we prefer theories with greater verisimilitude in Popper’s sense, then preferences among false theories are never possible. Popper acknowledges all this, and other logical difficulties, in (3)–(5) of the newly added Appendix 2, but looks forward hopefully to the time when an adequate theory of verisimilitude will be at our disposal. (This time, in my judgement, has not yet arrived.) Assuming that happy state, he goes on to make a determined effort to explain more clearly in what sense the empirical record sometimes gives us ‘reasons’ for preferring one theory to another. At my prompting he added an explanatory remark at the end of item (4): ‘Whenever I say . . . that we have reasons for believing that we have made progress . . . I do (not) claim . . . that T_2 is, in fact, nearer to the truth than T_1 . Rather, I give an appraisal of the state of the discussion of these theories, in the light of which T_2 appears to be preferable to T_1 , from the point of view of aiming at the truth’ (p. 372). Whether this statement contains another of those ‘whiffs of induction’ that Popper’s critics so often manage to detect in his writings, I leave for the reader to judge. But the truth is that our theoretical preferences are quite as conjectural as our theories are; and there is nothing wrong with this, provided that we can, as suggested in the previous paragraph, test these preferences against the facts, and sometimes overthrow them.

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