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TRUTHLIKENESS, RATIONALITY AND SCIENTIFIC METHOD*

ABSTRACT. I. A. Kieseppä's criticism of the methodological use of the theory of verisimilitude, and D. B. Resnik's arguments against the explanation of scientific method by appeal to scientific aims are critically considered. Since the notion of verisimilitude was introduced as an attempt to show that science can be seen as a rational enterprise in the pursuit of truth, defenders of the verisimilitude programme need to show that scientific norms can be interpreted (at least in principle) as rules that try to increase the degree of truthlikeness of scientific theories. This possibility is explored for several approaches to the problem of verisimilitude.

1. INTRODUCTION

The modern concept of verisimilitude or truthlikeness was introduced in the philosophy of science by Karl Popper (1963) as an attempt to illuminate the rationality of scientific research. Honouring this origin, the concept has played since then an important role in the discussion about the rationality of science, especially when the realist interpretation of scientific knowledge was being defended or criticised.[1] Scientific realism is a theory about the *aims* of science; in short, it asserts that science pursues true knowledge about the world, though further sophistications concerning the meaning of 'true knowledge' are a matter of bitter dispute. But truth (the full truth, and nothing but the truth) is an elusive object of desire, as the provisionality of all the greatest theories in the history of science patently shows. Hence, if a realist philosopher intends to preserve the rationality of science (as most of them do), it seems mandatory to understand the progress of scientific knowledge simply as a process of *approximation* to the truth. A plausible definition of this concept is 'a growing degree of similarity to the full truth about some aspect of the world', which was Popper's inspiring idea when he presented his definitions of verisimilitude. Unfortunately, those definitions were shown to be logically defective, and this fact encouraged many other authors to elaborate alternative, logically coherent concepts of truthlikeness. This philosophical research programme reached a summit during the second half of the eighties with the publication of Oddie (1986), Kuipers (1987) and especially Niiniluoto

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(1987), a monumental book which contains the most detailed elaboration of the verisimilitude programme. Niiniluoto (1998) surveys the last developments in the verisimilitude programme.

The main result of those works was to establish beyond any doubt the internal coherence of the concept (or concepts) of approximation to the truth, but it can be reasonably asked whether or not this logical achievement is enough to defend the rationality of scientific research from the realist point of view. In some previous papers,[2] I have defended the idea that, besides conceptual clarification, verisimilitude theory should also be complemented with a methodological perspective (this had been insistently pointed out by other authors, especially Theo Kuipers), and, what is perhaps a more problematic assertion, that the methodological approach to verisimilitude is particularly apt to offer a philosophical explanation of the nature of scientific knowledge and scientific method. These ideas have been recently criticised by Kieseppä (1996). In that article, Kieseppä rightly argues against the definitions of verisimilitude offered in the first of my papers, but, in spite of recognising this, I still think that his criticisms to the full methodological approach to verisimilitude cannot be accepted. In the next sections I will try to show why the methodological perspective is an essential element of the verisimilitude programme (Section 2) and whether methodological ideas can be derived from some definitions of truthlikeness (Section 4); in Section 3, I will examine another recent criticism to the idea that methodological rules can be justified by appeal to the aims of scientific research, a criticism presented by Resnik (1993).

2. THE UNAVOIDABILITY OF METHODOLOGY

According to Kieseppä, the theory of truthlikeness

> can be seen as *a theory providing us with an explication of an ordinary-language expression*, of 'closer to the truth' (. . .). The modest aim of giving a more precise meaning to this unclear way of speaking is quite sufficient for what has been (. . .) the most important philosophical background motivation for developing the theory of truthlikeness, or for the defence of a realist account of scientific progress.[3]

It can be argued that, if we are trying to make a solid defence of the realist interpretation of scientific knowledge, then we cannot be satisfied with just having an explication of what people *mean* when they say that 'modern scientific theories are closer to the truth than the older'. We want also to know *whether this proposition is true or not*, or at least, whether it can be *rationally defended* or not. After all, our main task as scientific realists should be to justify the idea that the rationality of scientific knowledge

amounts to its possibility of getting closer and closer to the truth. Hence, a brief inspection to the concept of 'rationality' seems unavoidable.

A necessary condition of the rationality of an activity (in this case, of scientific research) is its having *a consistent set of goals*: you can't both have your cake and eat it. In this sense, finding out an internally consistent definition of closeness to the truth was, undoubtedly, an obvious demand in the defence of scientific realism (or, as it was called by Laudan (1981), of 'convergent realism'). But the internal coherence of a goal is only *one* necessary condition of rationality; the other two essential requisites are the *workability* of one's aims and the consistency of one's *behaviour* with the *satisficing* of those aims.[4]

Suppose we have proposed a certain explication of 'truthlikeness', and that we have already shown that our definition is internally consistent and also that it satisfies certain intuitions we had *a priori* about the meaning of that concept. Could this be taken as a strong defence of scientific realism? Obviously, it cannot, since, although the proposition 'scientific knowledge is growing towards the truth' may have been shown to be logically coherent, this only means that (being neither a tautology nor a contradiction) it can be either true or false . . . but perhaps it is false! It is conceivable that critics of scientific realism accept that this philosophical doctrine is internally consistent, though they may still find reasons to reject it, not the least being the one according to which humans may fail to have the ability to recognise progress towards the truth. Of course, to have a consistent definition of a term is something very valuable, especially if that term plays an essential role in your own conception of science; but stopping here the study of that concept does not help you to show that your worldview is better than the rival ones.

Hence, besides proposing a plausible definition of verisimilitude, we have also to give an answer to some additional questions:

(a) Is it possible to recognise (even tentatively) whether one scientific theory is closer to the truth than another, in the sense given by our definition? (This would illuminate the former 'workability' question).

(b) Do scientists evaluate their work in such a way that those theories with a higher degree of verisimilitude – as defined by us – tend to be finally accepted by them (perhaps with some *ceteris paribus* proviso)? (This would answer the 'behavioural' question).

These are important questions to be formulated if our aim is to defend the idea that scientific knowledge progresses (or can be made to progress) towards the truth. In fact, Kieseppä seems to be thinking of something like this when he asserts that

the expression 'closer to the truth' can be explicated in a variety of ways, just like many other ordinary language expressions, but the theory of truthlikeness might be used for *showing that science has progressed towards the truth on some particular cognitive problems* in the sense of various reasonable explications of the concept.[5]

Of course, in order to *show* that 'science has progressed towards the truth', a little more than a mere definition of truthlikeness is needed. It is also necessary to develop from such a definition some analytical instruments that could be applied to particular cases. These instruments can be called '*methodological theorems*', i.e., general statements logically derived from our definitions of verisimilitude and expressing a connection between (i) some empirically recognisable relations between several theories (or perhaps the same theory in some different circumstances), and (ii) a certain relation (perhaps a statistical one) between the degrees of verisimilitude of those theories. If these methodological theorems cannot be obtained in any way, then our definition of truthlikeness will be absolutely vacuous, and it will be impossible to test it against the actual evolution of scientific knowledge.

It could be answered that the concept of verisimilitude should only be taken as a 'regulative ideal', and I plainly accept that it is one. But something is a *regulative* ideal only if it offers *some criteria to regulate* somebody's behaviour. For example, to assert that 'you must be good', while giving *absolutely no indication* about how can one tell good from evil, is simply not offering any ideal at all. In the same way, to assert that we must look for theories 'closer and closer to the truth', without offering any *criterion* to distinguish (tentatively, at least) more truthlike theories from less truthlike ones, is simply an invitation to radical methodological anarchism.

After having derived a set of methodological theorems from our preferred definition of verisimilitude, it is reasonable to ask *who* are the subjects to which these theorems are addressed. One possibility is to interpret those theorems as methodological rules which scientists have to use as 'hypothetical imperatives'; this is a reasonable strategy if one defends a *normative* interpretation of scientific realism, i.e., if one assumes that truth approximation is, or should be, one of the essential goals of scientific research. Another possibility is to understand those methodological theorems simply as instruments to test the hypothesis that science is actually approaching the truth; this strategy is appropriate if what is intended is to make a *rational reconstruction* of the evolution of scientific knowledge. I think that both strategies are legitimate and mutually compatible, but both require the formulation of methodological theorems to be of some applicability to real science.

Kieseppä, notwithstanding, does not accept even the 'softer' strategy of using the theory of verisimilitude to make a rational reconstruction of the scientific method. He asserts that, even if we could calculate the estimated degree of verisimilitude of a family of hypothesis $g_1, \ldots, g_n$, we would not be *forced to accept* the most verisimilar of them (suppose it is g_1).[6] If we were able to make that calculation, Kieseppä argues, then we would also be able to find a disjunction h of maximally informative statements such that the probability of h according to the empirical evidence is higher than the probability of g_1, and this seems to entail that we should 'accept' h instead of g_1.

This argument is grounded, from my own point of view, on a certain misconception of the process of theory assessment. If the verisimilitude programme is minimally correct, scientific researchers do not want simply to find out statements with a high probability of being true: they mainly want to find out meaningful and strong answers to scientific problems, *even if these answers are known to be only approximate*. In the previous example, $g_1, \ldots, g_n$ should be taken as *all* the tentative *and rival* answers that researchers have taken seriously in a given moment of time, and this set will usually be a small one. Not every logically possible disjunction of 'maximally informative statements' will count for *scientists* as an acceptable answer to their epistemic or technological problems, since additional, non-logical criteria of acceptability will be assumed by them. In particular, the disjunction of two theories will be usually considered as no 'theory' at all. For example, it is obvious that, if neither 'g_1 or g_2' will be at least as probable as both g_1 and g_2, but surely no researcher will consider it as a reasonable answer to his cognitive problem. Instead, what scientists try to do is to select, among the set $g_1, \ldots, g_n$, that theory which *seems* to be closer to the truth than the rest, or that which has a higher probability of being closer to the truth. This is especially clear when we think that scientists can be sure that *none* of $g_1, \ldots, g_n$ is, strictly speaking, true, not only because the available evidence may have disconfirmed already all of them, or because they are grounded on counterfactual idealisations, but also because of the 'pessimistic induction' which tells, on the ground of the history of science, that every interesting theory will be falsified sooner or later.[7]

Even when the aim of scientists is to reach the *true* answer to a given problem (as, for example, when they try to find out the composition of a chemical substance), there will usually be a moment after which considerations of *closeness to the full truth* will be more important for researchers than considerations of probability: once they have discarded all plausible answers except one (which is taken as true with probability close to 1),

then they will look for further *refinements* of that answer For example, if they have already found out the qualitative composition of a substance (let this be represented by proposition g), they will next try to find out the stereochemical configuration of its molecules (proposition h). Passing from g to h will increase the verisimilitude of scientific knowledge, because from two true statements, the strongest one will always be the closest to the truth. But, since h entails g, $p(h, e)$ cannot be larger than $p(g, e)$ under any corpus of empirical evidence e, and it will be usually lower. The classical Bayesian function, $p(t, e) - p(e)$ is more reasonable in this case than $p(t, e)$ as the quantity scientists would try to maximise, though it leads to uncomfortable consequences where $p(t, e)$ is known to be zero for any reasonable theory at hand.

3. ON THE CONNECTION BETWEEN SCIENTIFIC GOALS AND SCIENTIFIC RULES

The underlying idea of the preceding section has been that the only rational way to defend verisimilitude as one essential aim of science is by showing that scientists' behaviour is (or, at least, can be) consistent with the search for highly verisimilar theories; this can only be shown if we derive from our preferred definitions of verisimilitude some reasonable criteria for comparing (tentatively, at least) the degree of truthlikeness of different theories. Such a methodological strategy would be an example of what Resnik (1993) has called 'the teleological view' of scientific method, i.e., the notion according to which methodological rules can and have to be justified on the basis of scientific aims. In his brief paper, Resnik criticises the teleological view by trying to show that the very notion of 'scientific aims' does not apply to real science.

Resnik' s arguments seem to be directed against the view that there is a *single and coherent set of scientific rules* (what could be called 'the' scientific method) derivable from *a single scientific aim* (or, at most, from a limited and consistent set of them). Accordingly, his criticisms are based on the pervasive axiological plurality existing both among scientists (some pursue truth, some pursue prestige, some pursue wealth ...) and among philosophers of science (some endorse truthlikeness as the aim of research, some empirical adequacy, some explanation ...). This factual plurality would show, according to Resnik, that nothing like 'the aims of science' exists, in the sense of a *commonly shared* set of values accepted by all or most researchers, and hence, no single methodology would be homogeneously applicable to every case of scientific research.

Resnik is also right in inferring that nothing like *the* scientific method exists, in the sense of a detailed set of instrumental norms valid for every process of research and universally accepted by every researcher and every philosopher. This is true because of two different reasons, at least. In the first place, the axiological pluralism pointed out by Resnik leads to a corresponding methodological pluralism.[8] In the second place, each scientific discipline develops a different and peculiar method, since it has also peculiar goals and because actual methods are strongly based on the already accepted empirical khowledge (for example, methods to measure time are not identical in particle physics, in geology and in history). But, in spite of accepting the existence of this methodological pluralism, it seems obvious to me that *each* proposed methodological norm can only be justified on the basis of the scientific aims it tends to promote, *even if alternative norms can be defended on the basis of different aims*, and even if the individual scientist has to weight different and frequently conflicting aims in order to *decide*, among a set of conflicting norms, which one he is going to follow in each concrete case. This situation is analogous to that of legal systems: each law has to be justified, both from an ethical and from a technical point of view, with reference to the social or moral values it intends to defend or to foster, although different laws are proposed by different parties and applied in different countries and times. The idea of 'the only correct scientific method' is no more defensible than the idea of 'the only correct system of laws', but this does not make it absurd to defend *particular* methodological norms on the basis of *particular* scientific aims; it only serves to remind us that rational discussion on methodology cannot be separated from rational discussion on scientific goals.

In order to be more constructive, I will indicate three possible ways in which norms can be 'derived' from aims in the context of scientific methodology. In the first place, there would be an *instrumental derivation*: in this case, one simply tries to establish those conditions which allow for the satisfaction of a given set of scientific goals. Such a work would produce a series of 'hypothetical imperatives' of the form 'if your goal is *X* and you are in situation *Y*, then do *Z*', as well as an analysis of the mutual connections and trade-offs between different aims.[9] In the second place, there would be a *normative derivation*: one may use the results just stated, as well as other philosophical and epistemological arguments, to defend a particular goal or set of goals (and correspondingly, a set of methodological rules) as the most appropriate to scientific research. In the third and last place, there would be also a *positive derivation*: one would assume a certain set of aims or preferences (both cognitive and non-cognitive) of individual scientists, as well as the social, economic, psychological and

institutional constraints faced by them, and would try to infer what patterns of methodological decisions one could expect to observe in actual scientific practice. I think that this positive strategy cannot be automatically understood as a *justification* of particular scientific methods, but merely as an *explanation* of why certain patterns of decision-making are used by scientists in certain circumstances. In fact, this combination of epistemic and social goals, power relations and scarcity of resources will surely make it implausible to find out, in the real practice of science, the same 'pure' rules obtained through the normative and instrumental ways of derivation, for whatever combination of scientific aims one takes as preferable. But comparing the results of the positive derivation with those obtained in the instrumental and normative strategies could certainly help us to *criticise* the existing institutions of science and to propose others which could promote in a better way our preferred combination of scientific aims. Stated in other words, with the help of a positive strategy one can try to answer the normative question 'what institutional designs of scientific activity would best drive the progress of scientific knowledge in the direction preferred by us?'. This is, in any case, a work which I shall leave for further study.[10]

4. THE METHODOLOGICAL FORCE OF THE CONCEPT OF TRUTHLIKENESS

Once I have argued that a defence of the verisimilitude programme *needs* (and not only 'can be reasonably complemented by') a methodological approach, and that methodological rules should be justified or explained by appeal to scientific goals, we have to see whether the existing definitions of verisimilitude offer an appropriate explanation of the scientific method, such as is followed by actual scientists, or, at least, whether we can derive from those definitions some plausible methodological rules in order to give them a normative role. Fortunately, a big part of this work has already been done; both Kuipers' and Niiniluoto's theories of verisimilitude (the most important ones developed until now) include a relevant methodological or epistemic aspect. The first one is more consistent with the original Popperian idea of using the concept of truthlikeness as 'a logical basis of the method of science',[11] though his definition of the predicate 'is closer to the truth than' is radically different that Popper's, and does not suffer from the logical shortcomings that forced the abandonment of the latter.[12] Popper-Kuipers' idea is that theory testing has to be seen, not as a confrontation of individual theories (say, A and B) with the 'empirical evidence', but rather as a test of the meta-hypothesis 'A is closer to the truth than B'; the fact that all the successes of A are successes of B, and that all the failures of

B are also failures of *A*, can be seen as a *corroboration*, in the Popperian sense, of that meta-hypothesis, since this fact is simply a prediction that can be derived from the assumption that *A* is more truthlike than *B* (both according to Popper's and to Kuipers' definitions of truthlikeness). In Popper's words, if our empirical tests corroborate one theory and falsify the other,

> we can take this as one of the objective reasons in favour of *the conjecture that the new theory is a better approximation to the truth than the older one*.[13]

So, if scientists employ the method of 'conjectures and refutations', and select among their competing theories those which have been less falsified, we can perhaps take this as a corroboration of the philosophical hypothesis that they are pursuing truthlikeness as their main epistemic goal (or, at least, as one of their main ones). Kuipers has also shown that his 'refined' definition of verisimilitude (which is based on an underlying notion of approximation among single structures; see his (1992)) allows us to interpret the substitution of a more idealised theory by a less idealised one as a conjectural approximation to the truth. So, the use of the well known method of idealisation-and-concretisation by scientific researchers (see esp. Nowak (1980)) can also be seen as an indication that these may be trying to find out truthlike theories.

Niiniluoto's epistemic approach is very different. After presenting some quantitative definitions of the verisimilitude of a theory, as its 'distance' to the full truth about a given scientific problem, he introduces the concept of 'estimated verisimilitude', as the expected value of that 'distance' given some empirical data. One interesting consequence of this concept is that a refuted theory can still have a high degree of estimated verisimilitude (if the states of the world allowed by the data are different, but very close in general, to those states allowed by the theory); so, we can talk about a series of refuted theories which nevertheless 'seem to approach' the full truth. Unfortunately, Niiniluoto has failed to derive from that concept, as far as I know, a set of methodological theorems as strong as those derivable from Kuipers' and commented on in the previous paragraphs, but I think that this derivation can be a promising line of research in the future, allowing perhaps some idealisations and simplifications to be applicable to reasonable methodological contexts.

A much different approach has been followed by me in a series of papers, among them the one criticised by Kieseppä. What I have proposed is to substitute the duality between the 'objective' and the 'apparent' closeness to the truth of a theory, for the idea that verisimilitude is essentially an epistemic concept. In other words, instead of defining the 'real' truthlikeness as 'the distance of a proposition to the full truth', I

prefer to say that a proposition's verisimilitude is *its perceived degree of similarity to the known portion of the truth, weighted by the 'relative size' of that portion of the truth*. If we want to preserve in some sense the duality between 'apparent' and 'objective' truthlikeness, it is simply possible to define the last concept as the verisimilitude that a theory would have if the full truth were known; if the term 'truthlikeness' is nevertheless preferred for that ideal notion, we could give the other concept the name '*provisional*' or '*tentative truthlikeness*'. Some possible formalisations of this epistemic concept of verisimilitude are offered in Zamora Bonilla (1996a) and (1997), where a number of plausible methodological rules are also derived. In particular, my simplest definition of verisimilitude is $Vs_1(T, E) = [p(T\&E)/p(T \vee E)][1/p(E)] = p(T, E)/p(T \vee E)$, where '$p(T\&E)/p(T \vee E)$' represents the *similarity* between the theory T and the conjunction E of all known empirical regularities, and '$1/p(E)$' represents the '*rigour*' of E (this is a measure of content, chosen only by analytical convenience). An interesting and perhaps shocking consequence of this definition is that, though for *confirmed* theories (those entailed by E) verisimilitude covaries with logical strength, for *corroborated* theories (those which entail E) verisimilitude covaries *inversely* with logical strength;[14] this is not consistent with Popper's preference for 'bold' theories, but I think it reflects better the actual methodological practice of scientists, since, if T can explain adequately E, to prefer a theory T' stronger than T would go against the principle of parsimony. Of course, the situation may change when new empirical regularities are discovered, if these corroborate the stronger theory but not the weaker one.

Under one more complex definition ($Vs_2(T, E) = \max(F \subset E)Vs_1(T, F)$, where '$F \subset E$' abbreviates 'some conjunction of empirical regularities F included in E'), I show that to be 'at least as successful' as a rival theory (in the sense of having passed at least all the tests the other has passed, and having being refuted at most by the empirical regularities that have falsified the other) *only* becomes a sufficient condition for being more verisimilar if the most successful theory has also a higher *prior* probability.[15] This can explain the fact (noted mainly by Kuhn) that defenders of an older theory reject many times a new one even if this has a lesser number of 'anomalies', since the new paradigm's hypotheses may have for them a very low prior probability according to the old paradigm's background presuppositions.

The consequence just noted also allows us to establish a difference (though I admit that it is a slight one) between my complex definition of verisimilitude and the notion of empirical success; the latter would lead us to prefer those theories which make more correct predictions and

less mistaken ones, independently on the prior plausibility of each theory's axioms; on the contrary, the former notion would give also some value to that plausibility: a theory may be known to be false, but can be regarded as truthlike because, in spite of having some failures, it makes many correct predictions as well, *and* because its laws are consistent with our background presuppositions about the world. In fact, this is more or less the meaning that the term 'verisimilitude' has in ordinary speaking, at least in roman languages: something is 'verisimilar' when, as the Italian adagio says, *si non è vero, è ben trovato*. Furthermore, those 'background ideas' will usually contain, as one of their most important ingredients, the *ontology* assumed by each scientist or by each scientific school, and so, the prior plausibility of a theory will strongly depend on its coherence with the scientists' ideas about the underlying constitution of reality. The difference between a realist and an instrumentalist researcher, in connection with their methodological preferences, would lie especially, then, in the former's preoccupation about the 'realisticness' of each hypothesis. I think that the requirement of having a high prior probability (i.e., of being highly probable when only background ideas, and not empirical results, are taken into account) can be seen as a reasonable reconstruction of this preoccupation for 'realisticness'.[16]

Nevertheless, it can also be shown that, substituting the occurrence of Vs_1 in the definition of Vs_2 for its expected value (what can be a rational strategy when a high flow of new empirical discoveries is expected to occur within a 'short' period of time; for example, during the first stages of development of a paradigm or research programme), scientists who compare rival theories according to that new complex definition of verisimilitude will behave as if they only valued empirical success, both in the sense of counting only each theory's correct predictions, and in the sense of ignoring the prior probability of those theories.[17] This can be interpreted (with some benevolence) as an explanation of the fact, noted by Lakatos, that disconfirmations are not taken into account very seriously during the first stages of a research programme. *Scientists are, hence, more 'instrumentalist' when they are developing new ideas and more 'realist' when a theory has been 'firmly established'*, and the notion of 'tentative verisimilitude', if it is assumed as a relevant goal of scientific research, allows us to understand this peculiar methodological behaviour.[18]

Of course, I do not affirm that my definitions will be able to offer something like a complete explanation of the scientific method, in the sense of the general methodological practices actually followed by scientific researchers. I am sure that other epistemic values, besides verisimilitude, are present in the scientists' preferences, and other, nonepistemic values, also

have a strong influence on their decisions, though I guess that something more or less similar to what I have defined as 'provisional verisimilitude' can be seen as one of the main factors which constitute those preferences. In any case, the achievements of the verisimilitude programme allow us to be more optimistic about the possibility of understanding the scientific method from the point of view of scientific realism than what Kieseppä's and Resnik's arguments might have made us to think.

NOTES

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1 See especially Newton-Smith (1981).

2 See, especially, Zamora Bonilla (1992), (1996a) and (1997). The Spanish reader can also see Zamora Bonilla (1996b), where a detailed history of the verisimilitude programme is offered.

3 Kieseppä (1996, 432). Italics in the original.

4 I adopt here a 'soft' definition of the economic concept of rationality, in which 'maximisation' is substituted by 'mere satisfaction' (i.e., to reach a level of 'utility' that the economic agent considers 'sufficient'). See, e.g., Simon (1982).

5 Kieseppä (1996, 435). Italics mine.

6 Kieseppä (1996, 425).

7 The expression is taken from Newton-Smith (1981).

8 A referee of a previous version of this paper indicated that it is also possible that different aims lead to the same methods: for example, the goals of 'empirical success' and 'social utility' may both lead to the use of an instrumentalist methodology.

9 This would roughly be the vision of scientific methodology defended by Laudan (1984).

10 See, for example, Kitcher (1993, Chap. 8), for such a critical strategy in social epistemology.

11 Popper (1972, Chap. 2, Sec. 8).

12 Popper's definition was that A is more verisimilar than B if and only if (1) all true consequences of B are consequences of A, (2) all false consequences of A are consequences of B and (3) at least one of these relations of inclusion is strict. Kuipers 'naive' definition is that A is closer to the truth than B if and only if (1) all physically possible structures which are models of B are also models of A, (2) all physically impossible structures which are models of A are also models of B and (3) at least one of these relations of inclusion is strict. See Popper (1963, Chap. 12), and Kuipers (1992) and (1996).

13 Popper (1972, Chap. 2, Sec. 23). Popper's italics.

14 If E entails T then $Vs_1(T, E) = 1/p(T)$; if T entails E, then $Vs_1(T, E) = p(T)/p(E)^2$.

[15] This result is based on the simplifying assumptions that each empirical regularity is statistically independent of the rest, and that its only possible relations with each one of the theories under comparison are: (1) the theory entails the regularity, or (2) the regularity falsifies the theory, or (3) for any conjunction of regularities which do not stand in the former two cases, the theory is statistically independent of that conjunction. Under these assumptions, and if 'ECT' and 'EIT' stand respectively for the conjunction of regularities in E which are correctly predicted by T and for the conjunction of regularities in E which are statistically independent of T it can be proven that $Vs_2(T, E) = Vs_1(T, ECT\&EIT) = p(T, ECT\&EIT/p(T\vee ECT\&EIT)) = p(T)/[p(ECT)p(T\vee(ECT\&EIT))]]$ (for the proof, note that, for any statement G, $Vs_1(T, G) \leqslant Vs_1(T, G\&H)$, both when H is a logical consequence of T and when it is statistically independent of T though verisimilitude increases more in the first case than in the second; so, the subset of empirical regularities in E which maximises $Vs_1(T, E)$ is $ECT\&EIT$).

Hence, if ECT includes ECT' (that is, all known regularities explained by T' are explained by T), if $ECT'\&EIT'$ is included in $ECT\&EIT$ (what amounts to saying that all known regularities which falsify T also falsify T') and if $P(T') \leqslant P(T)$, then $Vs_2(T', E) \leqslant Vs_2(T, E)$.

[16] I have reached this conclusion stimulated in part by conversations with Uskali Mäki and by the reading of some of his papers on economic methodology (for example, Mäki (1994) and (1998)), from where I have taken the term 'realisticness'. In fact, the discussion about 'realism vs. instrumentalism' in economics tends to be more directed to this issue about 'realisticness' than to the problem of non-observable entities, which is typical of the philosophy of physics. I admit that the difference between 'background ideas' and 'empirical results' is, *in the abstract* more one of degree than an absolute one, but I also think that we can take it as given in each *concrete* case of research practice.

[17] If the logical space is considered to be divided into points (let x be one of them), in such a way that any proposition is logically equivalent to a disjunction of those points (for example, logical constituents), then the expected value of $Vs_1(T, E)$ is

$$\begin{aligned}
&\sum(x \vdash E)p(x, E)Vs_1(T, x)\\
&= \sum(x \vdash E\&\neg T)p(x/E)Vs_1(T, s) + \sum(x \vdash E\&T)p(x, E)Vs_1(T, x)\\
&= 0 + \sum(x \vdash E\&T)p(x, E)p(T, x)/p(T \vee x)\\
&= \sum(x \vdash E\&T)[p(x)/(p(E)][1/p(T)]\\
&= p(E\&T)/[p(E)p(T)] = p(E, T)/p(E) = p(T, E)/p(T).
\end{aligned}$$

Substituting for this value the occurrence of Vs_1 in the definition of Vs_2, we obtain that the subset of regularities which maximises this new function of verisimilitude is ECT (because now if T entails H, the expected value of $Vs_1(T, H)$ is $l/p(H)$, and, if H is statistically independent of T the expected value of $Vs_1(T, G)$ is equal to the expected value of $Vs_1(T, G\&H)$ for any G). So, if ECT' is included in ECT, then T will be more verisimilar than T' according to this definition of verisimilitude, disregarding the prior probability of both theories and the cases which falsify them.

Note that the notion of 'expectation value of Vs_2' cannot be adequately defined (except in some special circumstances), since each point of the logical space can be equivalent in principle to more than one conjunction of regularities.

[18] Another interesting consequence of my approach, less directly related to the problem of scientific method, is that it offers also a partial solution to the problem of translation (the fact that a theory A can be more verisimilar than another theory B when they are expressed in a certain language, but the reverse can be true when their translations to a different language are compared; see Miller (1975)). My definitions do not generate fully invariant orderings of theories, mainly because they are based on a *subjective* probability function, which can change from subject to subject. But it is trivial to show that the *methodological theorems* derivable from those definitions provide *some* language- and probability-function-invariant comparisons of some pairs of theories. For example, if E entails T and T entails T' then T will be more verisimilar than T' for any subject who makes the comparison and for any language into which the three propositions are expressed.

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