Verisimilitude

By Joseph Agassi
Tel-Aviv University and York University, Toronto

1. Introduction

The idea of verisimilitude is implicit in the writings of Albert Einstein ever since 1905, when he declared the distribution of field energy according to Maxwell's theory an approximation to that according to quantum-radiation theory, and Newtonian kinetic energy an approximation to his relativistic mass-energy. All his life Einstein presented new ideas as yielding older established ones as special cases and first approximations. The news has reached the philosophical community via the writings of Sir Karl Popper half-a-century after Einstein's trailblazing conception — first in his epoch-making "Note on Berkeley as a Precursor to Mach" and then in his classic "Three Views Concerning Human Knowledge" (both reissued in his Conjectures and Refutations, 1963).

Why the delay? Perhaps it was due to the fact that verisimilitude carries is a version of realism — usually known as scientific or critical realism, which, being metaphysical, clashes with the anti-metaphysical fashion of the day, a fashion to which even Einstein submitted a little for about a decade. (He later called this his sins of youth.) The same anti-metaphysical fashion also swayed Popper, whose classical Logik der Forschung of 1935 and The Open Society and Its Enemies of 1945 are sharply anti-metaphysical (though never in the then popular linguistic variant inaugurated by Ludwig Wittgenstein and propagated by Moritz Schlick and his the Vienna Circle). Popper became a forceful advocate of scientific or critical realism some time between 1945 and 1950. (Scientific or critical realism is the view that scientific theories should be taken literally; theories are descriptive putative truths; they are intended to reflect reality. It is regrettable that Popper was never clear about his change of mind, leaving his position vis-à-vis metaphysics generally unclear. The fact remains: taking physics realistically, scientific or critical realism allows for different specific metaphysical views within specific fields of inquiry, such as the atomistic metaphysics expressed in nineteenth-century chemistry and such as the view of matter as continuous, as expressed in various theories of elasticity. Only as a theory is taken as a mere approximation, it's realistic metaphysical import is relinquished, to be replaced by that expressed in its successor.

Popper noted in 1935 the significant role of atomism in the history of physics — a role usually called heuristic. Possibly he later admitted as a general rule what in 1935 he admitted as an exception, namely that metaphysics is a standard heuristic device, a device for the generation of scientific hypotheses. If so, then he later only slightly deviated from his

earlier anti-metaphysical position, as he thus only recognized the prevalence of a technique which he had earlier recognized as occasional, and which he anyhow was not particularly concerned with then, as he was then not concerned with heuristic at all. (He stressed this fact at the time, and those who criticize him for his oversight of heuristic are thus very unfair to him.) Possibly, however, he later admits metaphysics as a quest for a unified picture of the world, as an activity whose significance is not derivative and whose products should both help assess the value of scientific theories and be open to critical examination with the aid of these theories. If so, then he seriously altered his view about the place of metaphysics.

As the theory of verisimilitude is inherently pro-metaphysics, there are two possible readings of Popper's view of verisimilitude. Let me call these two versions the halfway and the fully-fledged. The difference between them is crucial in the understanding of Popper's realism, since realism is the general metaphysics that permits either of these attitudes to specific metaphysics. And the difference between these two versions of realism may be reflected in Popper's view of what his theory of verisimilitude should achieve: his theory of verisimilitude is realistic, yet in detail it can reflect pro-metaphysics either half-way or fully, and its half-way version may serve as an attempt to avoid specific metaphysics. I think the criticisms that it has met are due to his vagueness on this matter and to the obvious shortcomings of the halfway admission of metaphysics into science.

Though verisimilitude is a relative newcomer to the modern physics, it already has a rich literature, including two books by Ilkka Niiniluoto, the most energetic advocate of the topic as well developing it. His book, Truthlikeness (Kluwer, Dordrecht, 1987, pp. 518), with its 18 thick pages of bibliography, mostly on the subject, is very impressive. The present essay is not a review of it, as the mere discussion of its enormous, heavy logical-mathematical apparatus requires more space than is available to me. I will, however, say that his book has proven to be a philosophical landmark — for better and for worse — and I will try to present the general, most philosophical aspects of the situation, of how the book fits and is meant to alter the situation, and my response to all that. I should say here that I am not going to suppress my bias but rather contrast it with his. Briefly, I think that his position is halfway pro- metaphysical whereas mine is fully pro-metaphysical. Whatever advances the theory of verisimilitude has made since, the disagreement here outlined is not ephemeral, and my view has led me to a specific view of verisimilitude that is at odds with almost all that has been written after Niiniluoto's mentioned book of 1987.

2. Anti-metaphysics and Excess Rigor

Niiniluoto presents his attitude as scientific or critical realism and thus as a metaphysics of sorts; his attitude, I will venture to show, is halfway pro-metaphysics, not a fully-fledged one. His opposition to specific metaphysics is hidden beneath the heaps of technical discussions of possible worlds and of the similarities between them, and these are intended to serve as a framework for his discussion as its unifying factor, namely to serve in the role traditionally assigned to metaphysics; thus his theory of similarity is a substitute for a metaphysics. His enormous labors will presumably succeed to propagate the idea of verisimilitude and even some of the scientific or critical realism that goes naturally with it. And this will possibly be a measure of success, as verisimilitude is these days still viewed with (just) suspicion in the anti-metaphysical camp (despite the authority of Einstein). Since the anti-metaphysical camp was never free of metaphysics, all its protests to the contrary notwithstanding, this new intrusion of a new metaphysics will not constitute a great upheaval — at least as long as it will be covered up with excessively rigorous difficult technical apparatus of mathematical and logical formulas (regardless of whether they represent a theory of similarity). This change, then, will not launch a revolution, yet it will clearly be a considerable change of scene, and so it merits some observation.

Major changes of the scene require some discussion of background material, and there is nowhere easier to smuggle ones biases than in the broad outline of the background material. And here, then, is room to introduce biases, Niiniluoto's and mine. We would both see the development of philosophy as deeply bound to two major factors, the overthrow of Newtonian mechanics and the development of modern logic. Yet, Niiniluoto's bias and mine differ as to the point of the application of logic to philosophy, and in the following manner. Niiniluoto cites (p. 488) on this matter of the application of logic an admonition by Popper against the excess rigor exhibited in the writings of some philosophers. For them it is a substitute for certitude, Popper says, and a useless one at that. One should not try to be more precise, he suggests, than one's current problem-situation prescribes. Niiniluoto disagrees. In science, Popper is in favor of the boldness and the extended applicability of every theory. This idea should apply to philosophy too, says Niiniluoto: to that end one must transcend Popper's demand to be no more precise than the intended problem warrants. Niiniluoto discuses this matter of method briefly in the end of his book, though this is a major difference between him and Popper. If he wants to have Popper, or anyone who agrees with Popper, read and comment on his text despite his excessive rigor, then he need say why they should nevertheless invest the effort to read him. In a way, Niiniluoto admits this and makes a concession or two. In his preface, he suggests that the philosophical reader who has no taste for logical niceties can skip the

chapters on the logical apparatus and come straight to the meat. Yet he knows that this is not the case: readers who skip the technical details may get an idea, perhaps, of the main message of the book, but they will be unable to assess that message, the position which book's excessive logical apparatus supports. Even the way in which Niiniluoto's compromise is effected can be seen only after some familiarity with the books logical apparatus is acquired. The end-point is clear enough without much logic, though, and it is this.

Niiniluoto's position is neatly situated between Popper's hypothetico-deductivism and Carnap's inductivism, even though by and large Niiniluoto thinks the inductive philosophy of the period prior to Popper's study of verisimilitude is by now passé, that Popper clearly won that round. He presents Popper's says, "contra Popper" (p. 421). This is very clear but it seems quite problematic: what is the role of induction? Induction as a mere heuristic, merely as a means to develop hypotheses, whose degrees of verisimilitude must be discussed independently of their origins (even if Einstein is right and induction is seldom useful as a heuristic) is not at all "contra Popper". Alternatively, induction as the process justifying hypotheses, "contra Popper", seems to be contra verisimilitude as well: the study of the degree of verisimilitude of an already justified hypothesis is rather a pointless repetition and raises the problem of consistency: which preference is preferable, that of the inductively better supported hypothesis or that of the more verisimilar one?

Niiniluoto knows this: he sees his own acceptance of the program to study verisimilitude as the discarding of the program to study induction. How then is his view of induction different from Popper's? This is a central question for the readers of Niiniluoto, as his chief point is to rescue some of the ideas from the inductive camp and weld them with one sort of scientific or critical realism. This is not to deny the possibility of Niiniluoto's program, but to say that as long as he does not explain himself or his criterion, the reader is lost. And unless we know how his program is at all rendered possible, the whole of his exercise is suspect of being nothing short of a waste of time.

3. Niiniluoto's Program

Let me make some general observations about the waste of time. This waste is very common, though it is the worst ever, as time wasted is utterly irretrievable. We often spend time that in retrospect seems wasted: this is unavoidable and so in a sense not quite a waste. Yet we may seek techniques that may help us minimize this waste. There is an enormous, academically instituted hostility to the use of any time- saving technique: we teach students to memorize instead of teaching them how to use books and calculators

efficiently, and we tell them to read whole boring books because they have important kernels instead of teaching them to approach books as goal-directed searchers and pick from each book only the raisins they seek. The metaphor of science as the reading of the book of nature may be extended to the advocacy of reducing the tediousness of research too. Niiniluoto's approach to verisimilitude is rooted in the idea that similarity is a basic concept in the philosophy of science as well as in science. Hence, he should not object to the application of the observation on the importance of finding interesting books or passages of books to the study of the book of nature. Nor does he: whenever he reaches an impasse, he resolves it by allowing the choice between available alternatives to depend on one's interest.

The choice between following one's interest and doing boring detailed research was discussed by Sir Francis Bacon. He was convinced that following one's interests amounts to following a prejudice, whereas doing boring detailed research insures objectivity. Given a framework, the value of any detail may be assessable. But, observed Bacon, the framework is a prejudice unless it is based on experience, and so one has to begin without one, with no way to decide which detail is significant, with no right to omit any. The problem of induction then arises: having only details with no assessment of their relative significance we do not know what to do with them, whereas having a framework to assess their relative significance we have no way to assess the framework with the aid of empirical information without begging the question.

Here exactly Popper answered Bacon: refuting hypotheses is using empirical data without question begging. Bacon knew that negative results do not beg the question, but he declared negative results not sufficient. Niiniluoto agrees with Bacon: he seeks positive results. In order to obtain positive results without begging the question, he offers a highly complex, purely logical machinery, to enable him to work with similarities in general, and then with the similarity of a theory to the truth without begging any question.

Niiniluoto's project, then, is to justify science by verisimilitude and to justify verisimilitude with an appeal to logic alone. This would be an adequate solution, then, to the problem of induction. How then does he decide which similarities which science declares significant are those that our scientific theories declare significant. But then how do we decide which similarities between theories are to be taken seriously? Similarly, which similarities in experience are heuristically taken seriously? Which factual similarities are significant for building theories significantly similar to the given ones? This too is a difficult controversial matter. The believers in induction leave such matters to the researcher's intuition. This, Bacon has objected, begs the question and invites prejudices.

Analogy, he said, involves the process of inductive reasoning. In my opinion, the similarities between theories that count are those that exhibit the conformity of different theories to the same scientific metaphysics. Niiniluoto, however, refers to similarities between possible worlds, as these similarities are purely logical. The program is questionable from the start: similarities based on a logical theory are necessary (in the sense of modal logic), and if the bias research then logic is suspect and if not then they are unable to perform their intended task. It is quicker and nicer to go straight to the general point of the science at hand and posit them tentatively. As Erwin Schrödinger has observed (in the opening of his resounding "Are There Quantum Jumps?"), these general points are always metaphysical.

This, then, is the role played by excessive rigor in Niiniluoto's system: it is a tool to smuggle a specific metaphysics, and the specific metaphysics smuggled should be critically scrutinized. Niiniluoto's, we remember, suggests without discussion that excess rigor raises the level of openness to criticism of a theory and its ability to solve new problems. This is an empirically testable claim, as is Popper's suggestion that excess rigor is a new substitute to certitude (and an equally useless one). Why should rigor be at all related to certitude? After all, it is easy enough to replace every rigorous theory with an obviously false, equally rigorous alternative to it! Except that the obviousness of the falsehood of the alternative is the clue: advocating excess rigor may be suggesting that all alternatives to a rigorous theory are generally obviously false, so that by eliminative (second-level) induction its credibility rises to the maximum.

If so, then Niiniluoto renders verisimilitude credible by some second- level induction as a substitute for a first-level induction. This will make plausible his combination of inductivism and fallibilism.

This idea, thus, is a view of the progress without the aid of metaphysics. It is therefore important — perhaps even for Niiniluoto — to see that this idea is testable — and it may be refuted by any example of useless rigor from the history of philosophy. Now ever since the success of the heirs of Wittgenstein was established, certain rigorous treatments of certain questions were publicized and made centers of extensive studies by many scholars who published many papers about them. Verifiability, probability, theoretical and artificial languages, dispositional terms, basic or indefinable terms, synonymy state-descriptions and more. What has become of them? What knowledge has there been accrued and accumulated as a result? In what way, if at all, are we better off with them having taken place rather than not? They were all utterly boring and utter waste of time. Nor can one say that this waste of time was unavoidable. There were the criticisms leveled against Wittgenstein by Russell and by Popper, and had they been taken seriously — and

answered satisfactorily or not as the case might have been — research might have been more efficiently conducted. Clearly, then, the idea that excess rigor is induction by elimination is refuted: the elimination of the rigorous ideas of the analytical school usually led to naught.

Nor is this kind of waste a malady specific to philosophy. It is the result of exclusivity, of the technique of keeping the society of scholars engaged in a debate not as broadly based as possible. Even the history of science includes examples of critics wastefully not listened to because they were outsiders.

4. Science and Specific Metaphysics Intertwined

It is well known that ever since the development of Newton's theory of gravity, efforts were made to develop a Cartesian model for it, and one that will yield its predictions not as approximate but as precisely true. Newton, Huyghens, the Bernoullis, Le Sage Except that in the middle of the game a book appeared in which the game was proven hopeless. It was Joseph Roger Boscovich, Theoria etc. published in the middle of the eighteenth century. Boscovich's proof could be erroneous, of course. Yet, it is timesaving and even perhaps interesting to examine the proof for possible defects rather than ignoring it. The attempt to present one alternative Cartesian model or another is nothing but a cul-de-sac. Moreover, the very exercise is futile. What will be gained by the exercise from the empirical point of view? The historian William Shae has asked this question and his answer seems to be that nothing will have come of such an exercise.

The proposal to consider specific metaphysics beneficial to science, endorsed here, rests on the view that the aim of science is not merely to promulgate theories with high degrees of empirical character - - explanatory power, testability and all that — but also, if not chiefly, to seek a true, unified picture of the universe. If the aim of science is possibly also to seek a comprehensive view of the world, and Cartesian metaphysics seemed then the most promising proposal in this respect, then the conduct of the Newtonian followers of the Cartesian program, from Newton to Laplace, is understandable.

Let me mention one argument in favor of this program, from Leonhard Euler's profound Letters to a German Princess: if we want to define things by their essences and yet allow for their interaction, then the only view known permitting it is the Cartesian view of matter as imponderable: one piece of matter, insisting on being what it is, i.e., occupying space, thereby refuses to allow another piece of matter to take its place: resisting each other's effort to continue its own inertial motion they collide. The abandonment of the Cartesian philosophy, thus, amounts to the revolutionary abandonment of essentialism.

The rejection of the Cartesian program of the Newtonians forced those who wished science to be comprehensive to seek an alternative to Cartesian philosophy. This Boscovich himself tried, as did Kant and others who followed them. The final product in this direction was the field theory of matter, as I have tried to describe in some details in my Faraday as a Natural Philosopher.

5. Science and Epistemology Intertwined

There was a serious defect to all of the programs preceding field theory: their proponents all took Newtonian philosophy to be perfectly correct. And any attempt to see Newton's theory of gravity as perfectly correct had to be limited to the proposal to deny that any force of nature can alter an orbit of a planet even minimally. Hence, the idea that the only force of nature is the force of gravity that was so popular in the early nineteenth century, and of Boscovich's program of adding only small-range forces to it as these would presumably not interfere with stellar motions. Yet as long as approximationism was not developed, it is quite understandable that according to the received opinion Newton's theory was perfect: the alternative of viewing it as just another prejudice was inconceivable, of course.

The alleged perfection of Newtonian philosophy had to rest on its alleged certitude, of course and the alleged certitude of Newtonian philosophy created epistemological difficulties: whence this certitude? Science has an input and an output. Some philosophers centered on the output and ignored the input as uninteresting; others centered on the input and tried to see how it effected the output. The result was that either the output was not certain or that the output was different from what was naively presumed to be the case. Einstein has confessed in his scientific autobiography that it was this literature that gave him the courage to transcend Newtonian philosophy: the failure to solve the problem of induction adequately, i.e., to correlate the input and the output without radically altering the output, plus the staunch realism that prevented him from reinterpret the output, together forced him to the conclusion that Newtonian mechanics was not certain after all.

The view that output of science says what it seems to be is scientific or critical realism, and the views that the input of science is what it seems to be is naive realism; the two are in opposition, as A. S. Eddington has discovered (Preface to his The Nature of The Physical World). The rejection of scientific or critical realism, then, allows naive realism, which, however, carries no comprehensive view of the world, or else about the universe is usually some dogmatic view, such as the view that the truth has been revealed in full — to Aristotle or to St. Thomas or to St Karl; it does not matter much to whom. We may dismiss such views as dogmatic, namely, as of rather limited interest. The view that science is

certain and not an iota of it will be changed is also to be dismissed as dogmatic: perhaps scientific dogmatism is the worst kind — as it is, on top of becoming boring pretty soon, also quite opposes the spirit of science as traditionally understood. We are stuck, then, with scientific or critical realism that sees science as the unreliable, at least not the utterly reliable, guide to the answer to the question, what is the universe we live in like? For, any other view of science — whether it sees science as not saying what it seems to say or whether it dismisses what science says as utterly false — is either a dogmatic view of the universe, or no view of the universe at all. It is impossible to have no view of the universe at all and it is terrible to refuse to examine the question, what is the universe like? Science then offers very partial answers to this question.

Many philosophers of science are anti-realists; Niiniluoto cites some of them and argues with them, claiming that critical realism — Popper-style or not — is the only viable alternative available. To assert this, as Niiniluoto does (and I join him in this), is to assert that the current opposition to critical realism — though it includes some very famous names — unless it is dogmatic, it is either unintelligible or unintelligent. Now, before we can hold a serious debate with opponents we have to clarify what they say and what are the strong arguments in favor of what they say. The views of the traditional opponents of scientific realism, the idealists and phenomenalists, are better defensible than those of their contemporary heirs; they were driven by logic to their views of the universe, since they accepted the idea that one must believe all and only what is proven and they found hopelessly insoluble the traditional problem of induction, namely, the problem, how can we prove the certitude of today's scientific theories? But as it is now admitted that utter certitude is impossible to achieve, the very demand to believe this or that theory is gone. This is so even if high probability is admitted as a certitude substitute, since the highly probable is not final and so one who does not like a current theory may try to tip the balance of probability in favor of an alternative, whereas the verdict of certitude is without appeal. This, however, is no endorsement of any certainty-substitute; as many thinkers have claimed — including Robert Boyle, Benedict Spinoza and Charles Saunders Piece — we are not master's of our beliefs. What is important in science is not what one believes but what ideas one puts forward. Thus, when Einstein argued in 1905 once that light consists of waves and once that light consists of particles, no one cared about his innermost beliefs. Rather, the search began for a still newer comprehensive view of the world. And so, leaving the question of personal beliefs aside and taking scientific questions objectively, the question is what are the anti-realist assertions and why, if at all, are these assertions of any interest?

My answer to this question is, quite to my surprise, that though there is nothing of any interest in the anti-realist assertions about the universe, they are parts of interesting criticisms of the theory of verisimilitude: the anti-realist views hardly constitute an alternative to verisimilitude, but they do constitute interesting criticisms of it. Much of Niiniluoto's book is devoted to the criticisms of the idea of verisimilitude, but I do not think he does these criticisms much justice. Let me present Popper's view of verisimilitude and air some of the central difficulties involved.

6. Layers of Reality Introduced

Let me revert to Popper's already mentioned "Three Views". Essentialism is the theory that human knowledge offers us the true picture of the universe, as it gets at the very nature of things. Instrumentalism is the theory that human knowledge offers us no picture of the universe beyond the surface that we can see anyway, that it has nothing to do with the nature of things but only deals with successful predictions. A via media between these two traditional epistemologies is very hard to find. The essentialist view of science does not allow for human fallibility of science. It was refuted by the very presence of revolutions within science. (Here we see that Popper's demarcation of science is too wide, as the whole of classical meta-science has been irrationally some metaphysical system of some religious sect or another — and it does not even permit us to explain the success of science, since it opposes all explanation anyhow.

What is hard to visualize is not the need for a via media a candidate for this position. The need itself was expressed even in the rather insignificant book (Materialism and Empiriocriticism, 1908) of Vladimir Illich Lenin, that would have hardly been noticed were its author not a significant political leader. For political reasons the philosopher Paul Feyerabend praises Lenin, who merely cites approvingly some recent writers on the need for a via media, as if he were the initiator of what was needed. Even Niiniluoto approves of Lenin. This is not to belittle Lenin but to notice that before Einstein's contributions of 1905 it was extremely hard to come up with a reasonable alternative to the two traditional views. Nor is this to belittle Popper, though his contribution is both indebted to Einstein and unsatisfactory: Popper himself stresses his own debt to Einstein and admits the validity of the criticism of the detail of his proposal.

The difficulty of coming up with a viable via media is well illustrated by the wealth of criticisms of Popper's contribution. Yet, many critics of verisimilitude suggest that there is an alternative to it, the relativist theory of truth. This theory is explicitly endorsed by Thomas S. Kuhn, one of the most popular philosophers of science today. What does this theory

mean? It may be a theory of verisimilitude: we do not possess the truth, only the partial truth - - whatever this may mean — and we treat the partial truth pro tem as if it were the truth — until we have a better partial truth. This is distinctly not what Kuhn says: he explicitly attacks verisimilitude (say, in his "Replies to My Critics", in Imre Lakatos and Alan Musgrave, Criticism and the Growth of Knowledge). Another alternative reading of relativism is the view that Sun and Moon have behaved according to Aristotle once, and then they decided to obey Copernicus for a while and then Newton — until they decided to honor Einstein for a while too. I refuse to ascribe this view to Kuhn. Lenin has rightly poked fun at it: it clashes with the scientific idea that humanity is a relative newcomer to the universe. I do not know what else Kuhn means when he advocates relative truth if not the idea of verisimilitude and not the idealism which Lenin ridiculed. Still, Kuhn's criticism of verisimilitude is important.

The idea of verisimilitude is Popper's theory of the via media: science describes reality, but not the ultimate reality. By tradition the universe has two layers, Appearances and Reality. This was a great discovery of the Presocratics, the discovery of Nature, as Erwin Schrödinger has called it(Science and the Greeks). This way the dichotomy induced, between essentialism, the view of science as describing Reality and thus explaining Appearances, and instrumentalism, the view of science as describing Appearances and explaining nothing. (The third view allowed by the traditional dichotomy is that both the science of Reality and the science of Appearances are true. This was posed by Claudius Ptolemy and of Pierre Duhem: Aristotle has correctly described Nature, they assumed, and Ptolemy or Newton has described the Appearances. By this view the science of Appearance is in principle redundant and is used as a stopgap until the theory about Nature, becomes manifestly explanatory. But then, for the time being, the theory about Nature is but a dogma.) The division of the universe to Appearance and Reality is too poor, and Popper has announced that Reality comprises many layers — perhaps infinitely many.

This idea is new and frankly metaphysical. It is fraught with difficulties; so much so that it has been abandoned by an enthusiastic advocate, John Watkins, Popper's chief follower and the successor to his academic position. Watkins is cited by Niiniluoto (p. 444) to say, that Popper has done very well without the idea of verisimilitude for a few decades. Where then does the change originate? What new problem does verisimilitude solve and how well does it perform?

The chief problem at hand is, whence the success of Newton's theory? Though Popper operated in 1935 without a theory of verisimilitude, he already had to face this problem, and he solved it well enough for the time being: the success of the old theory is explained by the new, since any new theory should account for the facts accounted for by its

predecessor. This is the guarantee

This ushers neatly the idea of approximations. When Einstein published his early world-shaking papers in 1905 he consciously attempted to explain the predictive and explanatory success of preceding theories as approximations and their descriptions as approximate only in limited, special cases. (He did so also in his presentation of his very last theory, as he explained in the final Appendix to the final edition of his The Meaning of Relativity) Under certain circumstances which just happen to be prevalent, the difference between the predictions based on the predecessor and those based on the successor were too small to have been noticed and/or measured.

A very obvious and thus far neglected aspect of the situation is that predictions based on the old theory appear in the light of the new not only as approximately true but also that this holds only in limited, special cases. Other cases are either glaring refutations of the old theory or they were not touched by it. For example, classical atomic theory is glaringly refuted by atomic disintegration and it ignores many atomic facts. The success of the old atomic theory is rooted in the fact that atomic disintegration is usually both small and rare; the usual is the special case of rare atomic disintegrations characterized by the paucity of heavy atoms. Hence, the predictions based on old theory are approximately true, but only in limited, special cases. Likewise, Newton's theory of gravity tells us why Galileo's theory of gravity had met with empirical success: the large size of the earth's radius as opposed to the small size of the tower from which projectiles fall or as opposed to the small distance traveled by a canon ball are essential to the success of the predictions based on Galileo's theory of gravity as constant. Even the most ardent followers of Galileo thought so even before Newton: gravity should be different and universal, said Marin Mersenne: a body traveling from Earth to Moon should gradually shift gravity from the one to the other.

The layers of reality are now confusing. We could live with the fact that the layers are not quite precise. This makes the theories representing them false, but correctable at the small cost of seeing every one of the successive physical theories as a refinement of its follower — refinement in the precise mathematical sense. Many physicists insist that this correction and refinement should be read into each refuted theory in retrospect. The expression "always", should read to say, "always, within the given limits of accuracy". This is seductive. It has suggested to a few thinkers, notably Moritz Schlick and Werner Heisenberg, to compare the succession of scientific theories to the succession of geographic maps. This is a bit of a distortion even of cartography, as it presents the field as entirely commonsense and unproblematic: it presents maps as simply ever more inclusive, spreading both in the large and in the small, but always while the new refines the

old. This is the suggestion that there is no revolution in science! The accommodation of curvature into the maps is in agreement with this theory in the small and flagrantly refutes it in the large. So much so that the survival of so poor a theory. and it is still advocated, is more politics than intellectual activity. (I will soon return to Heisenberg.)

7. The Challenge of Verisimilitude

Physicists are not happy with the situation: they often find it upsetting that an old, once well-established theory can at times be used for predictions well enough, at times not, even though they have no practical difficulty here. Some of them have even denied this obvious truth, though only by implication, of course, as the stress the matter of approximation under certain conditions and overlook the case when the conditions in question do not hold. At times they stress that the conditions in question were not known to the old thinkers. Thus, Newton did not know of nuclear disintegration. Yet he said quite clearly and emphatically that atoms are immutable; the reluctance to hear of this fact is telling.

The idea of domains of applicability of the old theory relative to the new one has been presented as an essential characteristic of science in the theory of closed theories of Werner Heisenberg, insistently defended by Wolfgang Pauli and by Carl-Friedrich von Weizsäcker. It suggests that the instruments of the old period could not penetrate the domain and of the old theory and they could not be used to go beyond the limits of precision of the old theory: given technical means and instruments by which to study nature. Thus, the cartographers who were flat-earthers are vindicated as the curvature of the earth was not given to them either in the small, for want of accuracy or in the large, for want of ocean-sailing boats.

This theory is very interesting in that it shows awareness of the problems with verisimilitude, even though it was presented in 1948, years before Popper conceived his theory. Yet Heisenberg's theory is refuted: often instruments of the old period could easily be used to discover the basic facts of the new. In a sense this is commonplace: every case of a theory refuted before its successor is discovered is such a case. In a sense it is surprising: most of the data for atomic spectra — and atomic spectra are the chief data for quantum theory of 1926 — are given to any user of an instrument as simple as the spectroscope, discovered by Joseph Fraunhoffer a century earlier. Yet it was far from clear that the spectroscope presents phenomena alien to classical theories.

We see how right Watkins was to observe that Popper had done very well without verisimilitude for two decades. He could accept with ease Einstein's idea of the new theory explaining the old theory's successes by reference to them as approximations in special

cases; he could do so as long as he insisted on the view of scientific theories as testable explanations. And this idea is presented in his classic Logik der Forschung of 1935, which is frankly metaphysically indifferent: it is indifferent to the choice between realism and idealism, we are told there, (very much in line with the desiderata declared by (Kant and) Ernst Mach). He confesses there his realist metaphysical bias, but he insists that it is utterly a private affair with him. In the fifties he spoke of realism as imposing itself on us. Why, then, did Popper convert to metaphysics in his old age? The answer seems to be that in his combat against the conventionalists view of science as a set of a priori truths, Popper repeatedly bumped into the instrumentalism of the conventionalists and he was thus forced to acknowledge the question they had raised, what is the aim of science? (The very title of Pierre Duhem's magnum opus is, The Aim and Structure of Physical Theory.) And then, in order to avoid instrumentalism Popper had to change his opinion and agree with the essentialists that the aim of science is the discovery of the truth, the unveiling of Reality — even if it is possibly unattainable. The idea of the approximation to the truth was then at hand. (Before that his early rejection of the concept of truth, in line with his early Machian indifference, gave way to Tarski's theory of truth.) The ability of science to enlighten, then, lies in the fact, if it is a fact, that its theories are reflections, however poor, of the truth about Nature, and that these reflection of the truth are ever improving: science seeks the truth but finds only falsehoods which, however, are ever increasing degrees of approximation to the wanted truth.

But the moment the idea of the degrees of approximation was presented, it brought with it the idea of a truth-metric, of a single measure of proximity to the truth, yet no one has asked, in which space. In the pioneering studies of Einstein there was no problem: the new theory tells us where the application of the old will be more successful, where less, and there was no suggestion of a metric of any sorts, at most the ordering of a historical series of successful theories. In particular, there is no reason to deny, as David Bohm has observed, that of the series of layers of reality, one is causal, the next is statistical, and the next is causal, and repeatedly so. This, briefly, is the stumbling block noticed by some critics of verisimilitude — including the already mentioned Thomas S. Kuhn. And we have to concede that this is quite the common case, not the exception. For, from the statistical or atomistic viewpoint it is preferable to try to reinterpret the scientific theory which exhibits causality or continuity as a surface level and to attempt to uncover under the causality or continuity which that theory postulates a statistical or an atomic structure; yet from the continuity or the plenist viewpoint the very opposite is the case. This situation may - - and it did — bring about the fruitful process leading to great scientific progress.

This situation seems to some philosophers to defy any theory of science as a rational quest for the truth. They are thereby forced to follow Kuhn's theory of paradigm-switch as it presents all paradigms as equal. (Allegedly, the new paradigm determines the

The question is, how can we notice an increased degrees of proximity to the truth if we have theories going back and forth between specific metaphysical systems? How do we judge a debate progressive or regressive when the contenders continue for centuries? My answer hinges on the theory that the progress towards the truth is multi-dimensional.

8 Niiniluoto on Verisimilitude

Niiniluoto does not take up the challenges from the history of physics. Rather, he prefers to follow the footsteps of his hero Rudolf Carnap and take resort to many logical and mathematical ideas which he employs with amazing dexterity. I will not discuss the ideas he owes to Carnap. Rather, let me say, he adds to Carnap's machinery a few more instruments; perhaps the crucial among them is the popular though controversial theory of possible worlds semantics. Before the close of this discussion I should add a few non-technical comments on this "semantics". Were the possible-world theorists to propose the far-reaching assumptions that possible worlds semantics is a final theory of language, a theory of the essence of language, as it were, and that possible worlds semantics is more-or-less satisfactory theory, then the very idea of possible worlds would be amazingly close to, not to say identical with, Husserl's original idea of "the phenomenologically given". But no one insist on these assumptions; nor does Niiniluoto; so let us examine the system in their absence. Were a metaphysical sentence shared by all possible world descriptions then that sentence would be necessary in the sense of modal logic. Are there such sentences in possible worlds semantics"? We do not know and unless we make the far-reaching assumptions mentioned above, there is no telling. Perhaps the possible worlds "semanticists" will rightfully refuse to answer this question of mine. They may be forced to answer the following staple metaphysical question, though, as naming is inherent to the system: is there a proper name in each possible world properly naming some proper thing, or do all proper names designate in all possible worlds only ephemeral phenomena, even though relatively rather durable? To put this (Whiteheadian) question in Quine's jargon, is the Gavagai in every possible world but a rabbit-phenomenon or are there real rabbits in some of them? Niiniluoto is aware of this sort of question (which is hardly possible to overlook when a book by Saul Kripke, the father of possible worlds semantics, called Naming and Necessity); he declares them not pressing. When, then, do they get pressing and how does one handle them then? And how does one then improve one's language? And how does this kind of exercise effect verisimilitude? This question is too realistic for

Niiniluoto and so he leaves it untouched, even though he does raise it and even though he does assume that it is soluble, as he assumes that the whole truth about the universe is expressible in his primitive, first-order languages contrary to Gödel's second theorem. Overall, the remarkable thing about the treatment offered by Niiniluoto is its great distance from the actual scientific process. The only mentions of Einstein and of Newton in his book, for example, are in quotations from Popper. He discusses at length such questions as, given a set of some true sentences (singular or universal or mixed) and their negations as well as set of all the consistent conjunctions of all the combinations of sentences selected from the initial set, what is the degree of nearness to the truth of each of the conjunctions? Whereas Carnap studied the degrees of probability of state-descriptions, Niiniluoto studies degrees of verisimilitude of state-descriptions (generalized), given the truth.

No doubt there is a place for the discussion of this kind of verisimilitude; it belongs not to science, however, but to engineering, to the theory of technological approximation, where distances from give targets are very significant. To take a concrete example, the distance from a healthy environment is very significant and it is intuitively clear that the target, as legally or medically defined, is very distant when one breathes polluted air and drinks polluted water, and remains practically as distant when one also eats organic food. Here the question of distance matters a lot.

The same holds for the theory of errors, which tells us what errors are within the limits of permissible inaccuracy and which not. It is far from being obvious and there is a literature devoted to it, a literature well enough captured by Niiniluoto, though regrettably he ignores it. The point about this literature applied mathematics.

Despite the significance of the problems studied by Niiniluoto, the relevance of his study to the problems raised by Popper, his search a tenable critical realism, is questionable. Niiniluoto manages nicely to show that the critical realist attitude is imposing and perhaps also that without verisimilitude critical realism is lame. But beyond this he does not go. In particular, since he uses the logic of questions and permits incomplete sets of alternative answers to a given question, he could use his ideas to argue the case of the theory of gravity.

9. Verisimilitude

Consider the theories of gravity offered by Galileo, Newton and Einstein and consider the set of empirically testable corollaries to them, their corroborations and their refutations. Gaileo's theory was corroborated first, and then in crucial experiments between it and Newton's theory the latter regularly won. Regardless of whether Newton's theory is true,

then, Galileo's is contradicted by experience. And so, admitting this experience entails admitting that Galileo's theory is false. The results of the crucial experiments corroborate Newton's theory. What has then happened to the corroborations of Galileo's theory? The fact is simple: the same facts were tested again in the light of Newton's theory and they were found refutations rather than corroborations. Now some facts agree with both Newton's and Galileo's theory, of course. be tested again, provided technical means for a crucial experiment can be found. These statement will remain true when N is replaced by E and G by N.

All this is covered by Popper in his classic 1935 study — without reference to verisimilitude, of course. It does tally well with the idea of a search for the truth — provided one takes seriously the theory of Plato's Socrates that it easier to find where the truth is not than to find where it is. This idea is explicitly stated by Popper in his 1978 preface to his earliest, previously unpunished work, Die Beiden Grundprobleme der Erkenntnislehre.

Let. us now admit a measure of verisimilitude. The simplest is to introduce the comparative measure directly:

morver(A, B, e)

should read, given the evidence e theory A is more verisimilar than theory B. The historical cases need scarcely more than

If e contradicts A and is explained by B, then morver(B, A, e)

This is not contrary to Niiniluoto's text, but it is not approach, and he should tell us why not. Let us follow him for a while anyway, and consider the degree of verisimilitude of a theory given some evidence:

ver(A, e) = r .

We may prefer a normalized measure, between zero and unity or between minus and plus unity, and we may prefer the measure to diverge. What should he prove at once for any theory of the measure of verisimilitude worth its salt is, of course,

ver(N, e) > ver(G, e)

and

ver(E, e) > ver(N, e)

or, perhaps,

ver(N, e) > ver(G, e) ,

We can, of course, take distances more literally and define d(A, B) to be a distance between verisimilitudes: d(A, B, e) = | ver(A, e) — ver(B, e) | , and set e as the tautology or as a constant for any single discussion. Then, by the axioms of distance, d(A, A, e) = 0 . not d(A, C, e) > d(A, B, e) + d(B, C, e) . Until the theory of the measure of verisimilitude yields these theorems, it is still in its very preliminary stage. But why admit C always equals the distance between A and C.

Why do all this? And why assume a known target sentence and study its distances from known false alternatives to it? As distances are easily definable in different spaces differently, say, in Euclid's and Hilbert's but not in Minkowski's space, why should distances of verisimilitude be easy to define? Moreover, there is no inconsistency in defining different measures of distance, even if transition between them is not order preserving. Popper has claimed in his Logik der Forschung that all important characteristics of scientific theory grow together (are monotone functions of) its degree of refutability: its empirical content, testability, explanatory power, and simplicity. And he has offered a measure of the degree of refutability of a theory by defining a field of its potential falsifiers, which is relative to both theory and means of testing factual claims by empirical means. Clearly, Popper was in error here, yet he was opening here a very exciting field of study, and one which is still crying out to be developed — as a much more realistic alternative to state- descriptions. He was in error about ever so many things. First and foremost, attempting to re-institute unanimity in science he demanded that the most testable theory should be considered best. (Best. for what purpose?) Second, he was clearly in error about simplicity, especially since it pertains, as Leibniz has postulated, both to the paucity of hypotheses used as explanatory and to the richness of the observed facts to be explained. Yet we have no measure for either, of course. Nevertheless, the very idea of fields of potential falsifiers is exciting as it is somewhere in between the classical ideas of objectivity and subjectivity: it is relative to the given and changing state of the art (of testing theories). It will be seen as possibly useful if we take Niiniluoto's metaphor a bit further and say, the square of the distance between the verisimilitude of two theories is the sum of the squares of the distances of their contents, degrees of simplicity, explanatory power, corroboration, depth, etc. etc. This is but a metaphor, designed to raise the question, how much of Niiniluoto's study is to be taken literally?

10. What Verisimilitude Cannot Do

Popper's theory of verisimilitude postulates the existence, in some sense, of levels of reality, where each level is fully adequately described or explained or "covered" by one of successive theories. As such it requires no measure of verisimilitude, except for a

comparative measure for historical cases of successful theories: The later is nearer to the truth than the earlier. The empirical reason for stating this is always the simplest: the later stood up to a crucial test against the older.

Yet the success of the later prior to the crucial test is already a remarkable breakthrough: The new candidate explains the success of the old as a special case and as approximate, thereby proposing ways to set the crucial test. This is what gave Einstein the conviction that his theory would win in the crucial test against Newton. Of course, this is no guarantee: had the test gone the other way, there is no doubt that Einstein wound have yielded to experience; but his sense that he would win is the sense that the test was not a great risk for his theory. This concept of risk was first introduced by Popper when he said, a test must be an attempted refutation, where the whole of our background knowledge comes to bear on the likelihood of a refutation: when a test is very likely to come out as a refutation yet the theory survives that test, says Popper, then the corroboration is impressive.

Popper did not explain what background knowledge is. It certainly does not exclude the idea that when one theory explains another but not vice versa, then the crucial test is likely to go its way. Why? Perhaps because the new theory prescribes the terms of the (con)test, because it declares under what conditions the other theory will fail the test; perhaps the crucial test is simply a good tool with which to refute a theory that had resisted refutation in the past. This, however, is not to say that whenever a new theory explains another one its success in the contest with it is guaranteed: otherwise there would be no need for a test. The most dramatic example, perhaps, is the discovery by Lee and Yang of non-parity. They did not even have a theory proper, only a fact, the tau-theta paradox, so-called, namely the fact that two different elementary particles looked suppose that it is only approximately true; what will be the special case of the success to that theory? Once they had a conjecture, they could decide what circumstances would violate the laws perceptibly. Their prediction was corroborated even before they had a theory to which the refuted law was supposed to be a special case.

Popper wanted to prove that all this is possible. He overlooked the problem and solution, namely the need to explain the success of a false theory and the explanation of it that approximations. He ignored the fat that the approximation is of past successful predictions not of anything else. He ignored the fact that the success of the old theory is explained as due to special conditions. He suggested that the new theory is more verisimilar than is predecessor, quite generally and in the abstract. He postulated that verisimilitude increase occurs, if and only if the new theory includes the truth content of the

old and the old includes the falsity content of the new. This makes the verisimilitude of false theories either incomparable or of identical level of verisimilitude, since a false theory belongs to its own falsity content.

Can the theory be repaired? It should be clear from the analysis of the fault that the answer is in the negative: the question, how much the new goes beyond the old and how successfully so is an open factual question.

This is not just a matter of lack of generality: it renders obvious that the idea of layers is but a metaphor: there are no layers, and the complete adequacy with which a theory covers a layer should be a warning: the layer resides in the theory, not in the universe; it is what philosophers condemn as hypostatization.

There is a difference between Einstein and Popper regarding verisimilitude, and evidently Einstein is right and Popper has overreached: Einstein's view is this: **given that the new theory is true, the success with which old one has passed tests is due to its being approximately true under special circumstances that happen to prevail.** Popper's view is this: **a theory is more verisimilar than the one that it replaces**.

In an effort to rescue Popper's idea I suggested that the truth content and the falsity content discussed in Popper's criterion should be replaced by the truth content and falsity content within the field of potential falsifiers. One reason I had is that on my proposal the refutation of Popper's criterion mentioned above is disqualified at once. Another is that I do not think there is such a thing as truth content or falsity content of a theory in the abstract: we do not operate in science with all possible propositions. Consider the statement that Newtonian kinetic energy approximates Einstein's mass- energy equation. If it is not analytic, then it can be made so in not too obvious a fashion. Will it then belong to the truth content of Galileo's theory of gravity? The very question makes the concept of truth content too close to possible worlds semantics, and thus rather too abstract for words.

Evidently much as Popper went too far in the direction of the abstract, I went too far in the direction of the concrete. His theory of verisimilitude is exciting, as it manages to avoid the traditional extremes of full objectivity and mere subjectivity. As it stands it is hard to comprehend, since the successive theories are not refinements of each other; we will easily be confused unless we remember that the approximations are of successful predictions whose success is explained by the comparison of the older theories to the newer ones within their domain of success. But as I have qualified it, it has lost all its attraction. Is there a theory that can present verisimilitude with not too much abstraction and yet with no regard to any information about the world?

Popper's theory of fields of potential falsifiers is nicely situated between Reality and Appearances, and it does offer a tool for introducing a measure of sorts, but it was never taken up in the literature. One reason is that the potential falsifiers are not in any way about sensations and they are observation statements or observation reports only in the sense recognized in physics, not in meta-science. For example, Popper mentions sense, as we do not see a planet's position. What observation leads an astronomer to say, "the position of planet p at time t is (x, y, z)" is not easy to say, and whatever one says on this is a theory and as a theory it should be testable and tested. Yet, taking such statements as observational, then Kepler's first law requires five such statements to constitute a potential falsifier, as four positions are required to place an ellipse. Of course, the potential falsifier of Kepler's theory as a whole is much more complex, but this is the road to a measure of verisimilitude, if one is needed.

Why should one be needed? The answer to this question, if well formulated, should offer us a clue. Niiniluoto knows all that is narrated here, of course, yet he prefers not to consider any real cases of verisimilitude proper; even when he considers alternative measures and discusses the intuitive appeal of each of them, he does not use the known and intuitively recognized cases such as the theories of gravity. Niiniluoto is familiar with my work. He quotes me and after placing my claims in his formal system he disproves my claims — quite correctly as far as I can see. Hence, either he should give up his critical realism or he should give up his use of excessive logical machinery. He has tried, he tells us in his introduction, to speak with Popper's voice but use Carnap's hands. This is very nicely put, does it work? Possibly; but I doubt it: the hands are characterized by an abstract approach, and with little or no reference to historical case studies. When Popper uses these hands he fails as much as Carnap, and I fear Niiniluoto fares no better. Also, I think it useful to discuss the questions and difficulties raised by Popper and by Kuhn and by others first. And here, too, both Niiniluoto and Popper are too cavalier concerning criticism.

Nevertheless, even if Niiniluoto's approach does not work, it may be the fault of the hands and it might be the fault of the voice. There is no guarantee that the theory of verisimilitude in our present stage of knowledge is at all near the truth: we may need a revolutionary approach to the matter.