TRUTHLIKENESS WITHOUT TRUTH:
A METHODOLOGICAL APPROACH*

ABSTRACT. In this paper, an attempt is made to solve various problems posed to
current theories of verisimilitude: (1) the (Miller’s) problem of linguistic variance; (2)
the problem of which are the best scientific methods for getting the most verisimilar
theories; and (3) the question of the ontological commitment in scientific theories. As a
result of my solution to these problems, and with the help of other considerations of
epistemological character, I conclude that the notion of ‘Tarskian truth’ is dispensable in
a rational (and ‘realist’) interpretation of the scientific enterprise. As a logical result,
however, falsificationism will be vindicated.

καὶ πάντος λέγοντα τὸ δὴ εἰκὸς διωκτέον εἶναι, πολλὰ εἰσόντα χαίρειν τῷ ἀληθεί
... and in brief, a speaker must always aim at verisimilitude, and send the truth packing.
Plato, Phaedrus, 272 E

1. THREE STANDING PROBLEMS WITH TRUTHLIKENESS

A very large part of the work done in the field of verisimilitude in the
last fifteen years has been devoted to the problem of defining the
concept of truthlikeness, due mostly to Miller’s and Tichy’s demonstra-
tions of the inadequacy of Popper’s original definition. The work, de-
veloped especially by Niiniluoto and Oddie with the help of the well-
known tool of Hintikkian ‘constituents’, and by Kuipers with the help of
the Sneed-like concepts of ‘structure’ and ‘theory’ (a little simplified
for the occasion), has also been applied to a problem more ‘methodo-
logical’ in character, the question of estimating the degree of verisimili-
tude that one can expect his theories have when certain ‘(empirical)
evidence’ is given.¹

Efforts have been considerable, but many philosophers may think
that this is not the case with the results. The most uncomfortable
problem concerns the meaningfulness of the very notion of ‘logical
distance’ between constituents, and the similar distance between struc-
tures that is needed in the quantitative versions of the structuralist
approach (although structuralists are silent about this question, as far
as I know). These notions are rejoinders to Miller’s recalcitrant criticism

of linguistic dependence in a way that can easily seem ‘rhetorical’, for the following reason: Oddie and Niiniluoto have finally told us that the facts ‘picked up’ by different (but perfectly intertranslatable) languages are ‘in reality’ distinct facts; but one can remember that ‘translatability’ must mean something like ‘the capability of expressing the same facts in different ways’, and so Oddie’s and Niiniluoto’s responses to Miller’s problem are prima facie not convincing at all. From their approaches, we can still have very different (and even opposite) measures of the distance between several ‘possible states of affairs’, if we employ different languages to depict them (languages that express exactly the same facts).²

My impression is that the authentic objects for which the notion of ‘logical distance’ has been defined are not ‘the facts’ themselves (or the “worlds”, as Oddie likes), whatever these expressions mean, but our linguistic (or structuralist) descriptions of them. The question posed to those who still want to make sense of the idea of ‘false theories approaching progressively to the truth’ is that of finding a notion of truthlikeness that allows us to express at least some interesting judgements about verisimilitude in a way really independent of the language employed.

In the second place, although the standing definitions of distance between states of affairs (or, at least, the notions of comparative verisimilitude) can be rationally defended in some ways, the corresponding expositions about how to estimate the verisimilitude of the theories have some problems, too, that make this estimation practically an impossible job. I am thinking principally of Niiniluoto’s definition of ‘estimated truthlikeness’,³ a definition that presupposes a set of logical and mathematical calculations enormously intricate and almost interminable: if scientists had to calculate Niiniluoto’s measure for their competing theories and hypotheses, they would have to devote all their time to this labour, and other scientific investigations would perhaps be stopped!

Apart from this, Niiniluoto’s definition of estimated verisimilitude does not give us many ideas about what we can or must do to obtain the ‘apparently more truthlike’ theories; that is, it lacks (sufficiently interesting) ‘methodological consequences’, of the type that Popper was surely thinking when he introduced the concept of verisimilitude as a logical/ontological support to his falsificationist methodology of science.

Structuralists have perhaps been more incisive in dealing with this
issue, just because they have here taken a rather different, 'indirect' way: instead of defining a quantitative notion of 'estimated degree of truthlikeness' and trying to reach 'methodological consequences' from it, they have begun by exposing a concept of 'success of a theory with respect to the empirical evidence', and showing afterwards that this 'success' guarantees 'closeness to the truth'. In fact, I will do something similar in the present article, but I will adapt this 'methodological attitude' to Niiniluoto's and Oddie's views. However, for reasons that will be stated below (see Section 3), I think that the structuralist approach to this epistemological question is not completely problem-free, and several of Kuipers' and Zandvoort's ideas need to be critically considered.

In the third and last place, the logical definition of verisimilitude and the epistemological one of estimated truthlikeness have not yet been developed to respond to one of the most important questions that historically spawned increased interest with these matters: that is, do those notions support as it was expected the 'realist vision' of scientific investigation? The question is that, although the logical notion of 'distance from the truth' could make sense for false theories (as with the case of Miller's trouble), its brother concept of 'estimated verisimilitude' presupposes that the effective comparison of theories must necessarily be made in the language of the empirical evidence, and then the 'estimated) closeness to the truth' will seem to be reduced to the more instrumentalist concept of 'adequacy to the empirical data'.

In the following sections I will attack these problems, in the first place, within the frame of the 'similarity approach', as Niiniluoto and Oddie like to name it (Section 2). After this central section, I will try to make a 'translation' of my results to the 'structuralist approach' (Section 3). And finally (Section 4), I will contend that the (epistemological) notion of 'estimated truthlikeness' is more important than the concept of 'closeness to the absolute truth'; even more: that this last concept can be perfectly excluded from our philosophical perspective, according to a theory that could be called 'methodological realism'.

2. THE SIMILARITY CASE

2.1. Estimation of Truthlikeness

I will begin by supposing that a quantitative measure of distance between
constituents, \(d(C^{(d)}_i, C^{(d)}_j)\), has been already defined for every 'quantifi'd'
cational depth \(d\) of the constituents of a first-order language \(L\).\(^6\) (Don't
confuse 'd(x, y)' as a distance function between constituents with 'd' as a
measure of the quantificational depth of a sentence or single constituent!).
This measure will inevitably show some arbitrary features, but they will
be founded on our basic ideas about 'what is more or less similar to
what' in that field of scientific knowledge whose related conceptual
framework is 'logically reconstructed' by language \(L\). The only requi-
sites I put on 'd' are that, for every depth-\(d\) constituent, \(C_i, C_j\), its
value must always be positive and at most equal to one if \(C_i \neq C_j\), and
equal to zero if \(C_i = C_j\).

Once given this measure, if we also suppose that a distance 'Tr'
between sentences of \(L\) and individual constituents has been defined
(with the same proviso as placed on 'd'),\(^7\) and that an epistemic function
of probability is known (say, a function 'P(h/e)'), that gives for every
generalization \(h,\) and for every singular sentence \(e,\) both expressible in
\(L,\) a measure of the 'rational degree of belief in the truth of \(h\) given
\(e)\), then a very natural way of defining the notion of 'expected value'
of the truthlikeness of \(h\) given evidence \(e\) is Niiniluoto's measure:\(^8\)

\[
ver_N(h/e) = \sum_{i \in I} Tr(h, C^{(d)}_i) P(C^{(d)}_i/e),
\]

where 'I' is the set of all indexes of the depth-\(d\) constituents of \(L\).
Definition (1) serves for stating a 'methodological rule', a rule that
expresses what for a scientific realist could be the 'right way towards
the truth':

\[
\text{Between competing theories } h_i \text{ in a moment } t, \text{ and given}
\text{ that } e, \text{ is the total 'evidence available at moment } t, \text{ select}
\text{ that theory that makes maximal the value of } ver_N(h_i/e_i).
\]

Niiniluoto shows that, following this rule and reinforcing the evidence
\(e,\) we cannot only get theories that have a higher apparent degree of
verisimilitude than the past ones, but, what is more important, theories
closer to the truth 'in the limit'. This occurs because, in certain systems
of inductive logic, it can be demonstrated that:

\[
\text{When the number } n \text{ of individuals examined to form the}
\]
evidence \( e \) grows without limit, there is a unique constituent \( C^* \) such that \( P(C^*/e) \to 1 \), and so, \( \text{ver}_N(h/e) \to \text{Tr}(h, C^*) \).^9

I think that Niiniluoto’s argument is completely sound, but, as I said in the preceding section, it cannot serve us at all as a workable method for comparing, in fact, our real-life theories in the light of their possible ‘(apparent) degree of verisimilitude’ because of the immense difficulty of calculation involved in it; inductive logic, on the other hand, is not developed enough to be put into practice apart from some extremely simple languages, and even measure \( Tr \) is subject to some disputations that can further disturb our already implausible results. Moreover, the number of depth-\( d \) constituents of even a first-order language is so high when \( d \) increases, and the determination of the distributive normal form of a theory is so difficult (if not impossible), that calculation of measure (1) is, rather, a question of philosophy-of-science fiction.\(^{10}\)

In the second place, function \( \text{ver}_N \) is also subject to the problem of linguistic variance: that is, when our theories are expressed in another language \( L' \), the theory \( h_i \) selected by rule (2) can be different.

I now centre my attention on the first of these problems, leaving the second for the next subsection.

My principal idea about the problematic issue of verisimilitude estimation is that, instead of a rule like (2), what we (and scientists) need is a theorem (or theorems) of the following form:

\[
\text{(4) } \text{Doing } X \text{ is a sufficient or necessary condition for the estimated verisimilitude of one theory to be high or to be increased.}
\]

Perhaps there are logicians or mathematicians that are able to deduce from definition (1) some strong consequences, like those presumed in our statement (4). Unfortunately, this is not my case. Instead, I will try to obtain some theorems of this type by reconsidering the concept of ‘estimated truthlikeness’.

I have said that it was very natural to explicate the notion of ‘apparent distance from the truth’ as an instance of the general concept of ‘expected value of a random variable’ (in our case, variables are theories \( h_i \)). But this concept is valuable only where we have some precise ways to measure the functions involved within it, and this is not the case now, as we have seen.

We shall here take a rather different alternative. The idea of ‘esti-
mated truthlikeness’ can be considered, not as an ‘expected value’ in statistical terms, but, more intuitively, as ‘apparent distance from the truth’ or ‘distance from the apparent truth’. How can we express these ideas in more precise terms? In the first place, it must be explained what the expression ‘the apparent truth’ means.

In (1), the ‘empirical evidence’ e was considered as a conjunction of singular statements, expressing the result of, say, a finite set of observations or observations of a finite ‘sample’. My own opinion about this issue is quite different: I think that the ‘empirical evidence’ that is important to estimate the truthlikeness of one or several theories is not the ‘samples’ or ‘particular instances’ of an experiment or observation. The very core of the experimental method is that we can control (or try to control) ‘all’ the relevant variables intervening in one experiment, and observe what happens when they change. A good experiment, realized only a few times, is perfectly convincing for scientists: they suppose that, if carried out again in ‘the same’ controlled conditions, it will always provide ‘the same’ results. The natural expression of this belief is, in fact, a general statement, not a singular one: it is an ‘experimental law’. Scientific observation, on its side, tends to be carried out in a similarly ‘controlled’ way. Because of this, I consider the ‘empirical evidence’ (that is: the ‘apparent truth’) as laws, as propositions without essential reference to concrete individuals, and then as sentences having, like the theories that are to be contrasted with them, a distributive normal form.

Of course, the ‘instances’ or ‘samples’ must play some role in this story; they are, after all, the basis of our knowledge. But, in the question we are now examining, they only intervene in the justification or refutation of empirical laws, and, once we have these, considering instances again will be clearly redundant. For example, if a theory explains an accepted empirical law, although they both have certain counterexamples, these will not disturb our considerations of the theory if they have not prevented us from accepting the law. Redundancy is still more obvious when instances ‘confirm’ the experimental laws (see Note 31 below).

In the relation between instances and empirical laws, ‘induction’ will be needed, of course, but not in the sense of the Carnapian or Hintikkian calculi that could underlie (1): science has worked very well in the last three centuries without their help; it has simply used the ‘inductive
(and conjectural) principle’ of ‘uniformity of nature’, that ‘the world is governed by laws’, or as we want to express it.¹¹

When evidence e is considered as a generalization or as a set of them, it is possible to apply rather different definitions to the concept of estimated truthlikeness, since e, like ‘theories’, will also be associated with a set of depth-d constituents. Let e be the conjunction of n empirical laws \{e₁, \ldots, eₙ\}, and let E, Eᵢ, and H be the set of indexes corresponding to the disjuncts in the distributive normal forms of these laws and theory h. Then, two different measures are reasonable at first sight:

\[
\begin{align*}
(5) & \quad (i) \quad d₁(h/e) = \frac{1}{|I|} \left( \alpha \sum_{j \in H} d_{\min}(e, h_j) + (1 - \alpha) \sum_{j \in E} d_{\min}(h, e_j) \right) \\
& \quad [0 \leq \alpha \leq 1] \\
& \quad (ii) \quad d²(h/e) = \beta \frac{1}{|I|} |Eₙ| - 1 \\
& \quad [0 \leq \beta \leq 1]
\end{align*}
\]

Here, ‘d_{\min}(a/bᵢ)’ is the minimal distance between one constituent bᵢ and a set of them (in this case, the set corresponding to the distributive normal form of a), and eₙ is the strongest conjunction of empirical laws \{e₁, \ldots, eₙ\} that is entailed by theory h; Eₙ is its associated set of constituents.

(5.i) can be seen as a definition of ‘distance from the apparent truth’ (it is the usual distance between a hypothesis and the known empirical laws), and (5.ii) of ‘apparent distance to the truth’ (it measures the ‘amount’ of empirical knowledge explained by our hypotheses; but see below for a more vivid explication). The first is, in reality, equal to Niiniluoto’s measure of ‘distance between statements’ that he himself rejected as an estimation of verisimilitude because he was thinking of e as a singular statement, though he has applied it to the explication of another related concept: ‘distance from indefinite truth’.¹² One measure more or less equivalent to (5.i) has been proposed by Kuipers as ‘approximative quantitative estimated distance’, and (5.ii) could be considered as a quantitative analogue to the idea of truthlikeness that underlies Zandvoort’s ‘rule of explanatory success’, both within the structuralist approach.¹³
The reason for including the expression ‘−1’ in (5.ii) is to assure that 
d(C_i/C_i) is null for every single constituent C_i.

(5.i) and (5.ii) lead us directly to the following definitions:

(8)   
   (i)  \( \text{ver}_1(h/e) = 1 - d_1(h/e) \).
   (ii) \( \text{ver}_2(h/e) = 1 - d_2(h/e) \).

The second of these concepts could serve immediately as a plausible explicant of the expression ‘estimated truthlikeness’, since it just measures the magnitude of that part of the ‘available truths’ that is explained by one theory. Intuitively: if we imagine the filter of possible intersections of sets \( E_i \), ‘\( \text{ver}_2(h/e) \)’ indicates how far from the limit (e) of that filter we stand in proposing the theory \( h \). The only problem with respect to this definition is that it does not discriminate between theories having exactly the same known empirical consequences.

On the other hand, ‘\( \text{ver}_1 \)’ is less appropriate by itself as a measure of estimated truthlikeness, because it has the unpleasant consequence that \( \text{ver}_1(e/e) \) equals 1 for every generalization \( e \), and we want, in reality, a kind of measure that indicates to us ‘how far we are still from the truth’: a theory can show a very high apparent truthlikeness in the sense of ‘\( \text{ver}_1 \)’ with respect to a very weak evidence, and this must not be taken as a reason to think that it is ‘probably closer to the truth’. What ‘\( \text{ver}_1 \)’ lacks is precisely an indication of the fact that evidence \( e \) is not normally the full truth.\(^{14}\)

A little reflection shows, then, that measures ‘\( \text{ver}_1 \)’ and ‘\( \text{ver}_2 \)’ complement each other for being adequate explicants of the notion of ‘estimated truthlikeness’; and so, this leads us to our ultimate definition, a new measure that is just a weighted sum of both:

(7) \[ \text{ver}_3(h/e) = \gamma \text{ver}_1(h/e) + (1 - \gamma) \text{ver}_2(h/e) \]
\[ = 1 - (\gamma d_1(h/e) + (1 - \gamma) d_2(h/e)) \quad [0 \leq \gamma \leq 1] \]

It is convenient to keep in mind that this is the sum of two independent quantities that can be considered as two ‘versimilitude factors’, each with its proper interest.

Let us now proceed with the promised theorems of the kind expressed by (4).

Some very simple consequences of our last definition are the following (multiple indexation means that a statement is true for each respective definition of ‘\( \text{ver} \)’):
(8) If $e' \vdash e$ and $h \vdash e'$ (and so $h \vdash e$), then: $\text{ver}_{1,2,3}(h/e) \leq \text{ver}_{1,2,3}(h'/e')$.

(9) If $h \vdash h'$ and $h' \vdash e$ (and so $h \vdash e$), then: $\text{ver}_{1,2,3}(h/e) \leq \text{ver}_{1,2,3}(h'/e)$.

(10) For every $e$ there exists a generalization $h$ such that $\text{ver}_{2,3}(h/e) \leq \text{ver}_{2,3}(h'/e')$ only if $e' \vdash e$.

(11) If $e \vdash e_h$, then: $\text{ver}_2(h'/e) \leq \text{ver}_2(h/e)$.

We can translate these theorems\textsuperscript{15} into more colloquial expressions. (8) states that if one theory that explains all the available evidence $e$ also explains a reinforcement $e'$ of $e$ (that is, if it 'resists an attempt of falsification'), its apparent truthlikeness will increase. Having success in the light of new evidence is, then, for one theory, a sufficient condition for the increase of its estimated verisimilitude.

Theorem (10) asserts the opposite: a given piece of 'empirical evidence' (being a partial one in the sense already explained) imposes a limit to our 'desires of truth(-likeness)'. Then, if we want to continue increasing the values of 'ver\textsubscript{2}' or 'ver\textsubscript{3}' of our hypotheses, we can be in a state that gives us no solution other than 'to discover novel facts' (that is, to increase the available evidence). This makes the growing of (at least the empirical) knowledge a necessary condition for the augmentation of 'ver\textsubscript{2}' and 'ver\textsubscript{3}'. Of course, the generalization having maximal estimated truthlikeness with respect to the empirical evidence $e$ is $e$ itself in the case of 'ver\textsubscript{3}', but, in the case of 'ver\textsubscript{2}', it is any theory $h$ entailing $e$.

Results (8) and (10) have an evident Popperian flavour that is not shared by (9). Sir Karl would have liked, in fact, the contrary. My opinion is that we must prefer 'bold' hypotheses, not because of an 'innate quality' of them, but, rather, because theories must be able to do at least two things for us: first, they must help us to explain an 'increasingly stronger knowledge about the (empirical) facts' (and then it can be easily seen that, in the long run, the conditions of our (10) will compel us to invent in the future new theories stronger than past ones); second, 'bold' theories are the most useful and systematic instrument for helping us to discover 'novel facts', since they make us centre our attention on some special points – those states of affairs allowed by $e$ but not by $h$.

Nevertheless, if in the long run 'bold' theories are preferable and
even necessary, when one thinks about the evidence actually held by scientists, the best that one theory can do is to explain all this evidence; the more situations it forbids, the less 'verisimilar' it will seem. In this sense, theorems (8) and (10) on one side, and (9) on the other, express a dilemma that is essential, I think, for the scientific investigator: the more 'promising' his theories are, the more 'incredible' they seem until new 'evidence' supporting them is found. The cost of getting (possibly) more verisimilitude in the future is a lesser degree of it in the present.

Lastly, theorem (11) expresses certain conditions that allow us to judge the estimated verisimilitude of theories that have no deductive relations between them, and even that can be falsified by several laws in $e$ (the theorem remains valid although $e \vdash \neg h$ and $e \vdash \neg h'$). It is clear that the same is not true in general for 'very', because of its first addend; but, what must we do when, of two theories, one explains more accepted laws than the other, but the second is 'closer' than the first to the total evidence (and hence supposedly will give us more accurate predictions)? (See Figure 1.) However, our preferences about this question can be expressed by choosing one $\gamma$ or another in definition (6).

2.2. Invariance

Our theorems (8)–(11) have, in my own view, one essential feature: they give scientists some criteria to know when a theory has a higher degree of estimated truthlikeness than another, without the necessity of effectively calculating these measures, and then they express indirectly some pragmatical methods for getting theories 'apparently more truth-like'.

But, on the other hand, my own definitions of 'estimated verisimilitude' are dependent, like almost every other definition presented until now in the literature, on the concrete measure 'd' that has been chosen, and on the language in which theories and empirical laws are expressed. This is, I think, an inescapable destiny for the type of quantitative 'distances' that are normally used: different languages reflect in some way the different 'degrees of interest' that we put into the manifold features of the surrounding world, as the philosophers troubled with Miller's problem have finally told us, and then what counts as 'similar' will be forever an arbitrary question to a certain degree. Likewise, the almost Byzantine disputation (to which I have also contributed) on the
Fig. 1. Theory $A$ is closer to the ‘apparent truth’ $E$ than theory $B$ in the sense of ‘ver$_1$’ (its ‘distance’ is less); but $B$ is closer to $E$ than $A$ in the sense of ‘ver$_2$’ (it entails three empirical laws, $E_1, E_2, E_3$, whose conjunction is stronger than the conjunction of laws $E_4$ and $E_5$ entailed by $A$).

‘more appropriate’ measures of ‘$Tr$’ and ‘ver’ also reflect the existence of several more or less plausible (and sometimes incompatible) intuitions about the very notion of similarity. But, do these facts imply that all our judgements about the similarity between several things or states of affairs are nonsense? For example, have we no reasons at all for claiming that, at least in certain conditions, one attempted solution to a problem is better than another, in spite of the fact that both are ‘only approximate’? It is not possible to say that a theory is ‘a better approximation’?

To respond this question, I will demonstrate in the present subsection that our (8)–(11) also give us a nice escape from relativism, although I think nevertheless that the problem’s apparent seriousness depends only on an irreflexive assumption made in considering the very notion
of similarity; if this assumption were left out, the problem would fully vanish.

Before being more explicit about this last possibility, here are the ‘anti-relativism’ virtues of our preceding definitions and theorems:

(12)(i) The truth of theorems (8), (9), (10), and (11) does not depend on the measure ‘d’ chosen to express the ‘distance’ or ‘degree of similarity’ between constituents.

(ii) Likewise, it does not depend on the languages in which theories and empirical laws are expressed, if these languages are intertranslatable.

Theorem (12.i) says that, whichever ideas you have about ‘how to measure similarity’, you will increase the estimated verisimilitude of your theories by getting a situation like those stated by (8)–(11). The proof is absolutely simple, if one recalls definitions (5) and (7) and remembers that ‘d’ is always a non-negative number. Any other characteristic of ‘d’ is perfectly inessential to reach the consequents of our (8)–(11) from their antecedents.

In the same way, (12.ii) says that, whatever languages scientists employ (or philosophers who ‘reconstruct’ the scientific research), if the conditions given by the antecedents of our (8)–(11) hold and if those languages are mutually intertranslatable, then the consequents must always be true.

The proof here is a little more delicate; of course, the essential issue is what ‘intertranslatability’ means. As we saw in Section 1, two languages are mutually intertranslatable if and only if everything that can be said in the first can also be said in the second, and vice versa. To be more precise, a translation from language \( L \) to language \( L' \) is a function \( \tau \) from sentences of \( L \) to sentences of \( L' \), such that

\[
(13) \quad m \models a \text{ if and only if } \rho(m) \models \tau(a),
\]

where \( m \) is an \( L \)-structure, and \( \rho(m) \) is its equivalent \( L' \)-structure.\(^{18} \) If the inverse of \( \tau \) is also a translation from \( L' \) to \( L \), we say that these two languages are intertranslatable.

From (13) it follows that

\[
(14) \quad a \models b \text{ if and only if } \tau(a) \models \tau(b),
\]

and, if \( a [\tau(a)] \) and \( b [\tau(b)] \) are generalizations with sets of indexes \( A \)
\[A']\] and \(B [B']\) associated with their distributive normal forms, this implies that

\[(15) \quad A \subseteq B \text{ if and only if } A' \subseteq B',\]

which is just what was needed in the proof of (12.ii).

The importance of theorem (12) lies in the following fact: although our notions \(\text{\textit{ver}}_{1,2,3}\) are \(L\)- and \(d\)-dependent \textit{measures}, in certain cases we can know that \textit{comparisons} of estimated verisimilitude made with the help of them are invariant with respect to the languages and the notions of similarity employed. If several scientists reach equivalent results in their investigations (that is, if they concur about what are the \textit{deductive connexions} between hypotheses and empirical laws), they can agree on what hypotheses are \textquoteleft more apparently verisimilar\textquoteright, notwithstanding their use of different languages or their conflicting intuitions about similarity.\(^{19}\)

The situation we have now reached with respect to the concept of \textquoteleft estimated truthlikeness\textquoteright is reminiscent, in my own view, to that of temporal concepts in the wake of the special theory of relativity, because Einstein\'s laws showed that, although time measures are always frame dependent, some judgements of the form \textquoteleft event \(v\) is posterior to event \(w\)\textquoteright can be objectively true for any observer.

Moreover, if we think of verisimilitude as an \textquoteleft \textit{epistemic utility}\textquoteright,\(^{20}\) it would also be convenient to remember that, in the field in which \textquoteleft utilities\textquoteright have reached their greatest heights (i.e., economics), \textit{cardinal measures} of utility left their place to merely \textit{ordinal comparisons} many decades ago.\(^{21}\) To a certain degree, I have intended something similar to keep truthlikeness from its \textquoteleft relativistic impasse\textquoteright.\(^{22}\)

Let us now go one step further in demolishing relativism: I have mentioned above a feature shared by almost every definition of \textquoteleft similarity\textquoteright known to me, at least in the truthlikeness literature. This feature is that \textquoteleft similarity\textquoteright has been considered \textquoteleft as a dimensionless magnitude\textquoteright.

Suppose I am 1.80 metres tall and 27 years old; that John believes me to be 1.81 metres tall and 37 years of age; and that Joseph thinks that I am 2 metres tall but 26 years and 11 months old. John makes a mistake of 1 centimetre and 10 years, whereas Joseph\'s error is 20 centimetres and 0.0833 years. If we sum up both quantities, John\'s distance from the truth is 11, and Joseph\'s is 20.0833. But, if we measure height in metres and age in months. Joseph\'s error is only 1.2,
and John's has become 120.01. Who is closer to the truth, John or Joseph?

This is just an instance of Miller's recalcitrant objection. But, why do we suppose that length and time can be added in this simple way? One of the first things that children learn in primary school is that 'one cannot add apples and pears', and this so simple lesson seems to have been forgotten by 'similarity philosophers'. Of course, if kindergarten is not an academic authority of sufficient respectability, the highly sophisticated discipline of dimensional analysis will also give us the reason here.

From this point of view, the errors of my friends John and Joseph must be considered not as single numerals, but, rather, as two couples of magnitudes. John's mistake is the couple (1 cm; 10 years); Joseph's, (20 cm; 1 month).

This can easily be expressed for the general case of quantitative sentences. If language $L$ has a set of $n$ primitive metrical functions \{f_1, \ldots, f_n\} (and qualitative predicates can also be seen as functions taking the values 0 or 1), degrees of similarity between objects or states of affairs must be expressed as ordered $n$-tuples (i.e., vectors) \langle \alpha_1 \cdot m_1, \ldots, \alpha_n \cdot m_n \rangle, where the $\alpha$s are numerals, and the $m$s are the 'physical' magnitudes that these numerals measure.

If language $L$ and $L'$ are intertranslatable, this means that for every $n$-tuple of $L$-distances there will be an equivalent $n'$-tuple of $L'$-distances, but not in the sense that the sums of their members give the same result (they would be sums of different and independent primitive magnitudes!) the important thing is that, following the rules that transform $L$-in $L'$-quantities, the knowledge of $L$-distances leads to the knowledge of $L'$ ones. For example, it is the same thing to say that John's error is '1 cm and 10 years', as to say that it is '0.01 m and 120 months'; but without essential loss of information there is no way of transforming these expressions into single numerals, and then if we express distances as vectors, and not as scalars, Miller's problem cannot appear anymore.

Our theorems (8)–(11) also remain valid under this 'vectorial' notion of similarity, with only the adjustment of multiplying 'd_2' by the unit vector \langle 1 \cdot m_1, \ldots, 1 \cdot m_n \rangle in (5.ii), and admitting that the '≤' relation between vectors holds just when it holds between each pair of their $i$th components. This last move entails that '≤' is not a connected relation, but, if this is unpleasant for some readers, please return to the John-
and-Joseph example and try to find out an absolutely objective sense for ‘≤’, according to which one of my two friends is ‘closer to the truth’ than the other.

2.3. Ontological Commitment

To close this section, I will attack the third general problem stated above: In what a sense do the notions of ‘truthlikeness’ and ‘estimated truthlikeness’ support the ‘realist’ vision of scientific knowledge?

Here there are two different questions. On the one hand, there is the problem of giving some meaning to the idea of ‘false theories that are better descriptions of reality than other false ones’: to show (through definitions, like Oddie’s, Niiniluoto’s, Kuipers’, or mine) that such a meaning exists and that it satisfies certain intuitive requisites (including invariance) is to give an adequate answer to this first problem.

But, on the other hand, our definitions must help us to affirm that the ‘correct’ interpretation of scientific theories is the ‘realist’ one. That only the possession of an adequate definition is not enough to reach this goal can be shown by imagining a phenomenalist who thinks that there exists an absolutely true description of the world in terms of phenomena. Suppose that he wants to estimate the degree of truthlikeness of one theory expressed in a language that contains ‘phenomenal’ and ‘theoretical’ concepts, and that his ‘evidence’ is expressed only in ‘phenomenal’ terms (suppose also that he joins to this ‘evidence’ a statement rejecting the existence of any entities of ‘theoretical’ type). It is clear that our phenomenalist will accept that the theory has a degree of (estimated) verisimilitude, but he will think that this degree is extremely low.

What we need, then, is a demonstration of some statement like the following:

\[(16)\quad \text{The enrichment of the conceptual framework of scientific investigation is a sufficient or necessary condition to increase the degree of estimated verisimilitude of our theories.}\]

Conceptual frameworks are ‘reconstructed’ by languages, hence I will employ these instead of those. Under this point of view, ‘enrichment’ can mean simply the addition of new (non-logical) terms to a language \(L\), transforming it in \(L'\). It seems rational to suppose that, if this is the only modification, language \(L\) is translatable to language \(L'\),
although the converse need not be the case, and usually is not. But, nevertheless, it can also be possible that $L$ is not a sublanguage of $L'$, since each one can refer to some kinds of entities or qualities whose existence are not ‘allowed’ by the other. In any case, if translatability between $L$ and $L'$ occurs, this means (by (12.ii)) that comparisons of estimated verisimilitude made with the help of (8)–(11) for $L$-theories will also remain valid under their translation to $L'$.

Let us suppose now that, in practice, $e$ is the strongest experimental evidence that we have been able to reach using $L$ alone. If we ‘jump’ to the new language $L'$ (that is, if we admit the existence of other kinds of entities or properties), perhaps we can discover some new experimental law $e'$, and so new theories can be stated with higher estimated verisimilitude than the old ones (in general, theories $h$ such that $h$: $(\tau(e) \& e')$). This possibility is given by (8) and (10). In more formal terms:

\[(17) \text{ Suppose that: (i) } \tau \text{ is a translation from } L \text{ to } L'; \text{ (ii) there is no fully adequate translation from } L' \text{ to } L; \text{ (iii) } e \text{ is the strongest accepted empirical evidence that can be expressed in } L; \text{ (iv) between proposed } L\text{-theories, } h \text{ has the higher degree of estimated verisimilitude (in the sense of } \text{‘ver}_{2,3}\text{’); and (v) } e' \text{ is an accepted experimental evidence expressible in } L' \text{ but not in } L, \text{ and such that } (\tau(e) \& e') \text{ is consistent.} \]

Then, there can exist an $L'$-theory $h'$ such that $\text{ver}_{2,3}(\tau(h)/\tau(e) \& e') \leq \text{ver}_{2,3}(h'/\tau(e) \& e')$.

Several points about this theorem need some clarification. In the first place, we should be freed from the old empiricist prejudice according to which ‘empirical laws’ must always be expressed in a ‘purely observational’ language; so, in our case, several $L'$-statements can be ‘experimental laws’, ‘reports of observations’, etc. These laws will have a higher ‘theoretical import’ than pure $L$-statements, but only a few people believe nowadays in a ‘theory-free’ language. For example, Galileo’s law of fall was in his time a strongly idealized (and hence ‘theoretical’) hypothesis, but is now considered a ‘low-level’ empirical statement.

In the second place, it is not even necessary that $e'$ or $h'$ is expressed in $L'$: in many cases it is sufficient that the experiment whose report is $e'$ has been designed with the help of an $L'$-theory; this would show ‘indirectly’ the convenience of accepting the new and richer language.
In the third place, antecedents (iii), (iv), and (v) of (17) give this theorem a certain pragmatic character: the important question with respect to it is whether we are or are not able to find certain empirical laws. It is by no means a logical necessity that the languages in which the facts can be better described have one degree of ‘ontological depth’ or another.

One last comment: I think that ‘h’ and ‘τ(h)’ express one and the same theory; they are only two distinct formulations of it. Because of this fact, I would not admit the possible criticism that is suggested because the ‘theories’ whose estimated verisimilitude is compared in (17) are h’ and τ(h), and not h’ and h ‘directly’.

I end this section indicating that (17) is analogous to (8) and (10): we stated there that the discovery of novel facts can be a sufficient (or necessary) condition to increase estimated verisimilitude; now, we go one step farther, and state that the discovery of novel kinds of facts (in the sense of facts that involve the existence of new kinds of entities or qualities) can be also sufficient (or necessary). So, if we take for granted that the maximization of estimated truthlikeness is the real epistemic aim of science, then the running of an actual investigation can make it necessary to employ and accept increasingly stronger ontological frameworks.

3. THE STRUCTURALIST CASE

The most apparent difference between the ‘similarity approach’ to verisimilitude and the ‘structuralist approach’ is that they use two distinct logical spaces for defining their respective notions of ‘verisimilitude’: depth-d constituents on one side, and structures of a given ‘similarity type’ on the other. In both cases it seems possible to find reasonable definitions of ‘distances’ between individual points of these spaces, but the situation becomes more problematic for structuralism, especially when it tries to define the ‘distance’ or ‘closeness’ between one structure or model and a set of them (usually, the set of models of one statement), or between two such sets. The question is that, in the similarity approach, the syntactical form of one statement makes it possible to find some systematic ways of defining distances (though not of calculating them), either through sums (if the set of ‘logical points’ is discrete), or through integrals (if it is continuous). The set of structures of a certain type, on the contrary, is rather unmanageable by itself, especially when
we think of the kinds of comparisons that are needed in our epistemological examples, since here not only *mathematically defined* sets of structures are handled (which could be systematically studied), but also sets of *empirical models*, such as ‘planetary systems’, ‘particle mechanics’, ‘competitive markets’, and so on. If we add the fact that this set can contain ‘actual’ as well as ‘potential’ models, one can hardly see how distances between sets of structures can be constructed starting from distances between individual systems. The reason is that all these sets of *L*-structures (‘empirical’, ‘potential’, etc.) are not describable by language *L* itself.

Still more: In the case of individual structures, which will be the distance between two models having disjoint domains? And between isomorphic structures? It is not clear that these distances could be defined *if we don’t take into account the sentences satisfied by those structures*, and this will again carry us to the similarity approach.

However, my doubts about this question must not be taken very seriously: I admit that structuralists *could* find some adequate measures of distance like those needed to deal with our problems, though I think that, in this case, the syntactic approach has a great advantage.\(^{24}\) But, from the point of view that has been developed in the previous section, there is not a significant difference between both approaches, since our theorems (8)–(12) and (17) employ only the *logical connections* between theories and empirical laws, and (as is shown in statements (14) and (15)) it does not matter whether these connections are expressed in syntactical or semantical terms. Moreover, by (12.i) it will not be important which definition of ‘distance’ has been proposed from the structuralist view.

So, the ‘translation’ of our central theorems to the structuralist approach is quite straightforward, permuting the set of indexes of the constituents in a statement’s distributive normal form by the set of models of that statement.\(^{25}\)

Considered in structuralist terms, our approach to the problem of truthlikeness has been in a certain way similar to Kuipers’ and Zandvoort’s, as I already indicated in Section 1, since my theorems (8)–(11) can be understood as kinds of ‘rules of success’, like theirs.\(^{26}\) But behind this resemblance there are, I think, two main differences.

In the first place, what structuralist rules tell us is, roughly, that between two given theories, one must choose the one that is ‘closer’ to the empirical evidence according to a preferred definition of ‘close-
ness (in our version, the more directly related rules will be: ‘select the theory that maximizes $ver_{1,2,3}$’, that is, a modification of Niiniluoto’s statement (2)). The alleged virtue of those rules is shown by the following thesis:

(18) If one theory is closer to the empirical evidence than another, then the latter cannot be closer to the truth than the former.

Hence, the problem with respect to these rules is *how can one know that this relation holds between theories and evidence*. Only Kuipers’ ‘naive comparative’ definition of ‘closeness’ is more or less easy to check (since in this case it suffices to determine the logical connections between the three statements), but it is very implausible that two theories are precisely in this relation to a given empirical evidence: ‘naive comparative closeness’ is very naive. The naive quantitative case can also be easily determined only when the naive comparative holds.

And the difficulty to establish the other two relations is as high as it was in the similarity case. In addition to this, (18) does not hold for quantitative closeness. Kuipers’ definitions of ‘success’ have, then, very little usefulness in practice, and this usefulness is what we were looking for in our previous section.

Zandvoort’s rule of ‘explanatory success’, on the contrary, works a bit better. It states that, between theories $A$ and $B$ (considered as the set of their models), if every (experimentally) realized structure belonging to $A$ also belongs to $B$, and if $B$ entails more accepted laws than $A$, then we must select $B$. Zandvoort’s use of this rule, notwithstanding, is a little ambiguous: if the expression ‘more laws’ means strictly ‘a larger number of them’, it can be the case that one theory entails fewer but much stronger laws, and the other entails many more but of little importance. If, on the other hand, it means that $B$ entails at least all the accepted laws entailed by $A$, the situation is then more reasonable, and it becomes entirely similar to that stated by our (11). However, one should remember that this theorem held only for ‘$ver_2$’, since it did not take into account the (so to say) ‘degree of approximation’ between theories and evidence, something that Zandvoort’s rule also lacks (because at heart it is based on Kuipers’ ‘naive comparative’ definition).

The second main difference between my own approach and the structuralist one is that I have disregarded the ‘factual instances’, or realized
observations and experiments. To the reasons I gave for this in the preceding section, I can now add that the consideration of a sophisticated structuralist space (see Note 25 above) makes the use of ‘actual structures’ in the comparison of theories still more difficult.

This is so because the set of points of the sophisticated logical space corresponding to a statement, like ‘such-and-such structures have been realized’, is not the set of these structures (let be $X$), but, rather, the set $X$ formed by whichever set of structures containing $X$. To a theory $A$ will correspond the set $A$ of every set of structures whose elements are models of $A$. $X$ can in no way entail $A$, since, as Hume showed, there is no necessary connection between “this has always occurred until now” and “this will occur the next time”, and hence there are sets of possible structures in which $X$ is included, but that contain some elements that are not models of $A$.

In particular, it is not possible that, for two theories $A$ and $B$, it occurs that $A \cap X$ are included in $B \cap X$, as it is required by Zandvoort’s rule and Kuipers’ definition of ‘naive comparative truthlikeness’, since we can add to $X$ whichever structure $x$ such that $x \models A$ and $x \models \neg B$, and the resulting set $Y$ will be contained in $A \cap X$ (if all members of $X$ are models of $A$) but not in $B \cap X$ (see Figure 2).

So, the situation, if not fatal for the structuralist attempts of finding a notion of ‘closeness’ that makes use of empirical instances, becomes, at least, a little more impracticable than it seemed at first sight.

I shall close this section indicating that, despite some coincidences, my approach to the problem of truthlikeness has led us to a methodological conclusion rather opposed to Zandvoort’s, since he criticizes several Popperian claims that we have defended. Above all, he thinks that there is no logical connection between the successful prediction of novel facts and the growing of verisimilitude; in spite of this, our theorems (10) and (17) show exactly the contrary: there can be cases where the increment of ‘ver$_{2,3}$’ must make it necessary to find novel facts. Moreover, this discovery (if successful for one theory) will always increase (by (8)) its estimated verisimilitude.

It is clear that, as Zandvoort concludes, there can be ‘successful investigations’ not leading directly to the discovery of novel facts; they can, for example, lead only to ‘systematizations’ of the available knowledge. Nevertheless, I think that, if this is so, the ‘epistemic utility’ that is increased through this research is not ‘estimated truthlikeness’, but ‘coherence’, ‘simplicity’, or something similar.

So, I believe that my paper vindicates the Popperian programme in
the philosophy of science (or, rather, its Lakatosian version (see Note 31 above)), by demonstrating the old intended connection between falsificationism (as a theory of the scientific method) and verisimilitude (as a theory of the scientific aim).

4. METHODOLOGICAL REALISM, OR TRUTHLIKENESS WITHOUT TRUTH

My approach to the verisimilitude disputation has been ‘methodological’ from the beginning, as one can also easily confirm through an examination of the kind of results that I was looking for. Theorems
(8)-(11), or their structuralist translations, do not tell us anything about the meaning of the expression 'similar to the truth', nor does theorem (17) about the notion of 'a language more apt to describe the true features of the world'. They only tell us that, using a certain scientific method (a method that is a more or less sophisticated member of the falsificationist family), one can get theories that have a high degree of 'estimated truthlikeness', in the sense made explicit by definitions (5)-(7). But, what does this really mean? I now invite the reader to follow some considerations of a more 'metaphysical' character than those made formerly by us.

(A) In the first place, I am very skeptical about the very notion of 'truth as correspondence (with the facts)', in the sense that is usually ascribed to, e.g., Tarski's theory. Supposed that our 'knowledge' is exclusively a set of physiological reactions and states of our brain's neurons. The problem is that we have no way at all of comparing this knowledge with the supposed 'external facts' that we think it 'represents'. What is more important: we perceive things and facts as 'external' to us, and can compare this 'external experience' with other representations that we feel as 'internal' or 'mental' (e.g., opinions or beliefs, including 'theories'); the job of psychology and neurophysiology is to find the 'laws' that connect these two things: representations experienced as being 'outside of our mind', and representations experienced as being 'inside ourselves'. To suppose that there is another additional sense in which the words 'external' and 'internal' refer to 'things-in-themselves' is a hypothesis without any solid ground.

Before stigmatizing this opinion with the label of 'idealism', the reader should recall that it is only a consequence of taking seriously the scientific hypothesis that our knowledge is a biological fact, and then a 'material' thing or process. The question is: What do we want to say with the expressions 'material', 'real', or 'objective' (as opposed to the merely 'mental' or 'subjective')? And I am not able to apprehend about this question a more 'profound' concept than the simple idea of 'these things or facts actually or potentially experimented or imagined as external, and that usually behave in a regular way, independently of personal desires'.

My own approach to scientific investigation is 'realistic' because I do want to explain just what it means that our supposition that the objects investigated by science are 'real and objective'. If this meaning is based,
as I have said, on the ideas of ‘regularity’, ‘intersubjectivity’, and ‘(experienced) externality’, then: (i) scientific observation and experimentation should be the most preferred method for establishing the ‘regularities that external objects are manifest for whichever trained observer’; and (ii) verisimilitude can be understood as approximate correspondence between our ‘internal’ ideas about the world, and these regularities (estimated truthlikeness will then be the correspondence between our theories and the regularities observed or experimented up to the present). And this is just what I have done in the preceding sections.

Truth as correspondence to ‘transcendental Ding an sich’ has no place in my exposition.

(B) I fully reject, too, the idea that the world has in itself a well-defined structure that an ontologically perfect language could grasp completely. Structures are human creatures, and serve only as a ‘frame of reference’ to put one order or another in our experience (the result of this operation will be, of course, independent of our subjective desires). But Tarskian truth only makes sense when the language in which a proposition is expressed and the ‘reality’ to which it refers have ‘homogeneous’ structures.\textsuperscript{33}

Then, it seems perfectly logical to me that, if reality has no ‘structure’ at all by itself, truth as absolute correspondence with transcendental facts cannot take place in any way. The supposed ‘transcendental world’ is not the kind of thing that can be compared with linguistic expressions, at least in the sense intended by the correspondence theory of truth.

On the other hand, because our ontological and linguistic frameworks are always provisional and tending to be improved, a statement made within them will always be an ‘approximation’\textsuperscript{34} (except in one special case that I will indicate below). I think also that the newer and deeper ontological frameworks not only improve but also ‘falsify’ the older ones (as Popper claimed about theories three decades ago\textsuperscript{35}): this means that a statement expressed within a given framework (e.g., the statement of an established experimental law) will have to be considered as false when we pass to a new frame ontologically incompatible with the precedent, although the law itself remains perfectly valid in its ‘translation’ to the novel language. For example, the theorem of Carnot’s cycle, as stated originally by him (that is, expressed in its original ‘language’), is strictly speaking false (because it refers to entities that do not exist, at least according to our present theories); but we think
(because of these same theories) that it is still correct, that it depicts correctly the ‘thermodynamical reality’ (of course, within the limits of strongly counterfactual assumptions).

Therefore, I think that this character of scientific statements, of being ‘(more or less) correct’ although ‘strictly speaking false’, is the fundamental idea behind the concept of truthlikeness; but not in the traditional sense of ‘being closer to the truth’, or ‘containing more truth than falsity’, because what is good for a scientific theory is not ‘to have a high truth content’. The full truth (and this is the ‘special case’ referred to above) is always a trivial matter – it is enough to deny that the world has the features corresponding to a given framework, and if we decide to rest within this frame, then ‘informative’ statements will always be false; therefore, ‘truthlikeness’ must not be understood as a combination of information and Tarskian truth, as Popper and Niiniluoto maintain.

An interesting ‘content’ is just a ‘highly verisimilar content’ – or, in other terms, ‘content’ that explains to a high degree our (relatively) empirical knowledge.

In this sense, my opinion is that the authentic meaning of verisimilitude is the older, pre-Popperian one: the meaning according to which ‘the present state of our experimental knowledge makes our theoretical hypotheses more or less verisimilar, more or less credible’. This does not mean that I equate verisimilitude with probability: ‘verisimilar statements’ are those that seem true because they are useful to explain the facts ‘known’ by us, not because of their triviality. In this sense, we can affirm that authentic truthlikeness is estimated truthlikeness, and that the notion of ‘absolute distance from the full truth’ is of no great value in epistemology.

(C) This does not preclude that we can consider scientific investigation as a process that tends to bring us closer to the discovery of ‘the laws of nature’. These laws are independent of our subjective conceptions and desires, and their existence is the principal ground of our idea of ‘reality’. Still more, we have seen that the supposition of the existence of ‘strong laws’ and the idea that we can approach them progressively, in spite of the fact that the referents of these laws are not directly given to our senses, are the fundamental movers of scientific research, serving us as a supreme ‘guide principle’ in our investigation of this striking
object given to us in our external experience (not ‘through’ it!), and that we tend to name ‘the world’.  

My thesis is, then, that a high (estimated, or ‘epistemic’) verisimilitude is the authentic cognitive aim of scientific research, and that, if our theoretical and empirical knowledge have reached this, there is no other epistemic goal to fulfill. Perhaps some philosophers regard this objective as a devalued one, and state an additional problem: Why does our external experience show the regularities expressed in our scientific laws (empirical or theoretical)? But to ask this question is, in my own opinion, to take the first steps not only towards metaphysics, but towards theology; since, why would we be content with an answer like ‘. . . because there is a substantial reality behind this experience’? Does this answer not claim the following question: And why has this reality its regular character? It seems to me slightly Pharisaic to be content with a reply to the first problem only and to underestimate the second, but my own modest arguments, unfortunately, do not reach either of them.

NOTES

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3 ‘Miller’s problem’ was originally stated in Miller (1974). See also Barnes (1990).

4 Niiniluoto, op. cit., Ch. 7.


6 I only know of one exception, Kuipers (1987c), where the ‘observational/theoretical’ distinction is taken into account with respect to verisimilitude. My own approach to this problem will have, however, a few different aims and methods (see especially Sections 2.c and 3, below).

As an abstract, we can say that the most interesting characteristics of constituents are the following (where ‘quantificational depth’ means the minimal number (d) of nested quantifiers that are needed to express a sentence logically equivalent to a given one):

(i) the disjunction of all depth-d constituents is logically true (i.e., there is at least one true depth-d constituent);
the conjunction of any two depth-d constituents is logically false (i.e., there is at most one true depth-d constituent); and

(ii) every depth-d sentence of language $L$ that does not contain individual constants (that is, a 'generalization') is logically equivalent to a disjunction of depth-e constituents (its 'distributive normal form'), for every $e \geq d$.

I shall restrict myself to first-order languages and theories, but, with adequate modifications, our conclusions serve also for higher-order languages, if 'distances' between sentences and the relative 'sizes' of distributive normal forms can be defined for them.

7 A catalogue of some reasonable measures of distance between sentences and constituents, exposing the virtues and defects of each one, is given in Niiniluoto (op. cit., Ch. 6.5–6.6).

8 See, by example, Niiniluoto, op. cit., p. 269.

9 Niiniluoto 1984, p. 92. See also his 1987a, p. 275.

10 For the number of depth-d constituents, see Niiniluoto (1987a, pp. 70–71); for the difficulty of effectively establishing distributive normal forms, see Niiniluoto (ibidem, p. 75).

11 What I have said about induction does not preclude, of course, the use of statistical or probabilistic instruments in scientific investigation and theorization. I am only affirming that this use is not excessively necessary in epistemology, at least to resolve the kind of problems that I am attacking in this paper. (Let us reflect a bit on theorem (3): Has the 'growing without limit' of his 'samples' size any practical meaning for an actual investigator? When does a number become a 'big number'? In contrast with the dimension of the universe, mankind has learned not only 'from a little evidence', but from a truly infinitesimal one).

12 Niiniluoto, op. cit., p. 287. His definition of 'distance between statements' is closer to mine (see pp. 246–47). For the concept of 'distance from the indefinite truth', see Niiniluoto (ibidem, Ch. 6.8; and 1987b).

13 See Kuipers 1987b, p. 98; Zandvoort 1987, p. 238.

14 If $e$ is a disjunction of more than one constituent, the mutual incompatibility between the components of its distributive normal form entails that only one of them can be true, and then that there is at least one true sentence stronger than $e$ (assuming that $e$ itself is true). Hence, our empirical evidence will be only 'a partial truth'.

15 For the proof, remember only that $a \vdash b$ if and only if $A \subseteq B$ ($A$ and $B$ being the set of indexes of the constituents in the distributive normal forms of $a$ and $b$), and that distances are always non-negative reals. Theorem (10) makes use of an additional and rather idealized requisite: the 'cumulativeness' of empirical evidence (that is: if $e$ is the strongest empirical evidence available at moment $t$, and $e'$ the corresponding one at the posterior moment $t'$, then it will occur that $e' \vdash e$).

It can be convenient to remember that $e$, $h$, etc., are not such simple kinds of entities as it seems at first sight; $e$ is, as we have seen, a conjunction of empirical laws, and $h$ can be described as the conjunction of the laws that form the 'hard core' of one theory, plus the 'auxiliary hypotheses' that allow them to have valuable consequences (it would be possible in principle to define the estimated verisimilitude of an 'isolated hard core' at a given moment, for example, as the higher degree of ver shared by one of its 'derived
theories', or in any similar way). Because of this fact, considerations of simplicity will naturally be welcome in addition to those of verisimilitude.

16 See once more the paragraphs of Niiniluoto (1987a) and Oddie (1986) referred to in Note 2.

17 The 'Byzantine' character of this polemic does not preclude that one's opinions can be closer to some theses than to others. It only means that the magnitude of the energy expended in the controversy is not (until now) in relation to its fruitfulness.

18 For this formal concept of translation, see Pearce (1987, pp. 56ff.). If the notion of 'equivalent structures' seems problematic, one can go directly to proposition (14) and take it as a definition of 'translation'.

19 It is important to remark that deductive connections between hypotheses and empirical laws are the only things needed to reach theorem (12.ii), and then, if scientists agree about these connections, their comparisons of estimated verisimilitude will be language invariant in the cases assumed in theorems (8)–(11), although their languages are not fully intertranslatable.

20 See Niiniluoto 1987a, Ch. 12; Oddie 1986, pp. 178ff. Recently, Ruth Weintraub, reconsidering Niiniluoto's ideas and their subjection to Miller's criticism, has reached a sceptical conclusion about the possibility of providing "a measure of epistemic utility which is objective enough to be worth maximising...[and] accessible to a decision-theoretic epistemology" (see her 1990, p. 174). My own conclusion, although a little different from standard decision theory, is optimistically opposed to Weintraub's.

21 This was the 'Hicksian Revolution' in consumption theory. See a lucid exposition of it in Walsh (1970, Chs. 4, 5, and 7, passim, and esp. pp. 45ff.).

22 Another very interesting solution to this problem has been offered recently by Eric Barnes (1990). In spite of the big differences existing between our two approaches (principally with respect to our technical methods), we both accept the fundamental idea that it is in the relation between 'theories' and their 'empirical justification' where the solution to Miller's problem must (or can) be found; so, my theorems (8)–(11) relate hypotheses and empirical laws by their deductive connexions, whereas Barnes' definition of 'knowledge-likeness' (instead of 'truthlikeness') is based on the relation between quantitative assertions about facts and the reliable measurement devices that allow the first to count as 'knowledge'.

23 A structure is an ordered \( n + p + q \)-tuple \( S = (D_1, \ldots, D_n; R_1, \ldots, R_p; f_1, \ldots, f_q) \), where the \( D_s \)s are sets (the 'domains'), the \( R_s \)s are relations (sets of \( n \)-tuples) on the elements of these domains, and the \( f_s \)s are \( n \)-ary functions of these same elements into some \( D_i \). Two structures \( S \) and \( S' \) have the same similarity type if \( n = n', p = p', q = q' \), and for every \( i \), \( D_i (f_i) \) is an \( n \)-ary relation (function) if and only if \( D'_i (f'_i) \) is an \( n \)-ary relation (function).

When language \( L \) has just \( p \) relation-predicates and \( q \) function-predicates, it can roughly be said that \( S \) and \( S' \) are \( L \)-structures.

24 In spite of this, I think that structuralism (or the 'non-statement view') has offered some of the sharpest insights about most of the philosophical problems concerning science.

25 I think in fact that the 'points' of the most sophisticated structuralist 'logical space' must not be isolated models but sets of structures, because the sentences that we must
employ to describe them are, so to say, 'two-level' statements: what a scientific theory asserts is, on one side, that certain structures are models of one or several laws, and, on the other side, that certain 'constraints' (in Sneed's sense) are satisfied simultaneously by all these structures. The (sophisticated) 'set of models' of an L-statement A is, then, the set formed by all sets of structures whose members are (all of them) models of A.

For the notion of 'constraint' and its difference with 'laws', see Balzer et al. (1987, pp. 40ff.)

26 See, for instance, Kuipers 1987b, p. 96; Zandvoort 1987, p. 238.
27 Kuipers gives four distinct definitions of 'closeness', this being a ternary relation between sets of structures (i.e., 'A is closer to C than B'). They are 'naive comparative', 'approximative comparative', 'naive quantitative', and 'approximative quantitative'. The first employs only the logical connections between A, B, and C (it holds when (A ∆ C) ⊆ (B ∆ C)). The second presupposes a ternary comparative relation on individual structures, and holds when for every model x of B (A) and every model y of C (C) there is at least one model of A (B) closer to y than x. The third relation holds when (C ∆ A) has more elements than (C ∆ B). The fourth is almost identical with our (5.i) in the structuralist version, although suppressing the weights and the denominator '1/|l|'.

The four relations can be 'weak' or 'strong' (if '⊆' and '≤' are in their definitions), and 'internal' or 'external' (if we centre our attention on C or on C, respectively). The reference set C can be 'the set of physically possible structures', 'the set of realized experiments or observations', or 'the models of the strongest accepted empirical law'. See Kuipers (1987b, pp. 85ff.).
28 Cf. ibid., pp. 97–98.
29 Zandvoort, loc. cit.

This last proposition is not necessary from Zandvoort's approach, because the discovery of the new law can be made through the realization of new 'instances' that are not models of the theory, in which case this theory will not 'explain' the new law in Zandvoort's sense, although in our more liberal sense this is still possible.

It may sound astonishing that the realizations of new instances falsifying one theory can be interpreted as one success of this. Of course, I do not think that this way can be taken systematically by scientists (if many new instances are not models of the theory, it is not rational to infer from them new empirical laws that are still consequences of that theory). But, if a cautious investigator has found some actual structures that are 'closer' to his/her theory, though strictly speaking they falsify it, he/she can induce from these structures only a 'weaker' law whose theory can entail, at least until 'new measurements' are given or until some 'auxiliary hypothesis' can be removed.

Therefore, our approach also gives the possibility of dealing with more or less recalcitrant anomalies, and becomes more directly related to Lakatos' falsificationism than to Popper's.
32 Zandvoort, op. cit., p. 247.
33 If this is not clear, think about the semantical relations that must be established between one language and 'the reality' (objects, relations, properties, etc.) before defining the very concepts of 'true' or 'satisfaction' in Tarski's sense; in fact, in these definitions it is not 'the world' that is put directly into contact with the language, but one 'structure' or 'model', and these are only formal constructions made by ourselves.
The proper term 'approximation' is not adequate enough, because it suggests an 'ideal limit' to which the 'approximated things' tend: in our case, the truth in itself. I have in mind, instead, the idea that the older laws are 'approximations' in the sense that they are worse explanations than the modern ones of the facts experimentally known today, as today's theories will probably be worse than tomorrow's in the light of tomorrow's empirical knowledge.

Cf. Popper 1972, Ch. 5. (It is a 'revised version' of his old article 'The Aim of Science' (1957).)

I use the expression 'guide principle' in a sense closely related to that explained by Moulins (1982, Ch. 2.3), but the two 'principles' that I have used (the idea of nature as governed by 'exact laws', and the idea that between all the possible, relatively complete descriptions of a part of the world, one and only one of them can be true) are more general than the mechanical and thermodynamical examples referred to by Moulins.

I do not affirm, of course, that these 'guide principles' are 'self-evident', nor anything similar; there is one case, at least, where my first principle (and perhaps also the second) seem to be false: the apparent validity of the 'indetermination principle' in quantum mechanics, and the now established non-existence of 'hidden variables' (but note that 'established' is not the same as 'demonstrated'). Perhaps my 'guide principles' are only 'verisimilar', though not with respect to 'the truth in itself' but in the sense that they are the 'source' of the 'appearance of truth' that statements found with their help have.

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