Chapter 11

Thirty Years of Language Dependence

0 Historical Introduction

Language dependence has been described as 'the canonical objection against the whole verisimilitude enterprise' (Brink 1989, p. 186). I disagree heartily with this assessment. Those philosophers and logicians who maintain that verisimilitude or truthlikeness or approximation to truth has to be defined in terms of syntactical features of the object-linguistic theories under consideration should indeed regard language dependence as 'a serious problem for any explication of truthlikeness' (Kuipers 2000, p. 141). But I do not belong to this coterie, and have never done so. There exist also, after all, several language independent or translation invariant approaches to verisimilitude

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Previously unpublished. This chapter is dedicated in friendship to Vadim Sadovsky on his 70th birthday on 15 March 2004. It pays tribute also to his courageous introduction of critical rationalism into the former Soviet Union long before *glasnost*, and his continued championship of it since 1991. A shorter version, translated into Russian by Peter Bystrov, will be published in Miller (2005b).

A portion of the chapter, under the title 'Verisimilitude: A Pessimistic View', was presented at the workshop VERISIMILITUDE & ITS RELATIVES held at the Rijksuniversiteit Groningen on 3 July 1998. The title had been suggested by Theo Kuipers, and I did my best on that occasion to play the part of *Il Penseroso* as dispiritedly as Ilkka Niiniluoto, who spoke on 'Verisimilitude: An Optimistic View', spiritedly played the part of *L'Allegro*. The critical tone of the chapter must not be allowed to disguise my appreciation of the many contributions that those here criticized have made in the last 30 years to the problems of verisimilitude and approximate truth. Nor should it disguise the fact that I too am optimistic about verisimilitude, but not in the sense of knowing what will be learnt in the future.

It is not possible to list all those who have directly influenced my thought on this topic for good or ill, but I should like to mention my debt to Malcolm Forster, Deryck Horton, Jeffrey Ketland, and Sjoerd Zwart, for helpful recent discussions and correspondence.

The historical remarks in § 0, though intended to be of some interest, have no bearing on the negative and positive theses of the chapter. Where possible, I have added in parentheses brief references to items of correspondence in my possession. JHH, IL, AEM, KRP, PT, and DWM, are respectively John Harris, Imre Lakatos, Alan Musgrave, Karl Popper, Pavel Tichý, and myself.

and truthlikeness. One of these is Popper's original theory of (1963a), Chapter 10. Others are the metrical theory of my (1984), summarized in (1994), Chapter 10.4*b*, the theory of basic truth approximation (formerly called 'naive truth approximation') elaborated in Part III of Kuipers *op. cit.*, the measure of predictive accuracy championed by Forster & Sober (1994) and Forster (2001a), and the topologically sensitive treatment of Mormann (2005), (2006). These alternative theories may not satisfy everyone, but they do yield judgements of the relative truthlikeness of theories that are not affected by variations in the vocabulary in which the theories are expressed.

My earliest suspicions that some definitions of verisimilitude and of approximation to truth may be language dependent date, I see from the commonplace book that I kept in those days, from 16–17 March 1973; it was there that I wrote down for the first time an example of two interdefinable pairs of numerical quantities relative to one of which the theory **X** is to be preferred to the theory **Z** for accuracy whilst relative to the other pair **Z** is to be preferred to **X**. It may well have been in March 1973 also that on the other side of the globe Pavel Tichý proposed the first decently worked-out definition of propositional verisimilitude to be blighted by language dependence (JHH to IL, 20 July 1973). At a seminar at the Department of Logic & Scientific Method at the LSE on 20 February I had presented, to a barrage of unsympathetic criticism from Lakatos (which he withdrew in writing the next day, noting that politeness was not his long suit (IL to DWM, 21 February 1973)), my first public criticisms of Popper's qualitative theory of verisimilitude. These dated from the previous summer (DWM to PT, 22 July 1973), and were alluded to at the end of Miller (1972). Popper knew beforehand about this meeting, and about its content. Just before it took place the Poppers set off for a visit to the University of Otago, arriving (via Singapore, Bali, Sydney, and Christchurch) on 5 March (KRP to DWM, 16 March 1973). Tichý, diligently preparing for the distinguished visitor, had been studying Popper's theory, and had promptly detected its shortcomings (PT to DWM, undated (postmarked 2 July 1973); JHH to IL, 20 July 1973); according to Harris (1974), p. 164, note 2, at this stage Tichý's proof held for finitely axiomatizable theories only (and not, as later, for all theories), and it therefore did not go beyond the result of Hempel in 1970, reported by Hattiangadi (1975; see also Agassi 1976, pp. 340f.). Tichý announced his results at a seminar at Otago in March, at which Popper was present, concluding his talk with the declaration that Popper's definition was worthless. 'Popper took this criticism calmly' we are told (Watkins 1997a, p. 678). In his spoken reply to Tichý he said that he agreed with every word that Tichý had uttered except the last one: 'No theory can be worthless', he explained, 'that has stimulated such an excellent criticism' (AEM to DWM, 7 October 1998).

It seems that on this occasion Tichý also outlined an early version of his own theory of verisimilitude for propositional languages, using the standard representation of theories by their disjunctive normal forms; in any event, the paper (1974) that he submitted to *The British Journal for the Philosophy of Science* (*BJPS*) in July 1973 contained not only his criticisms of Popper's theory, but also a sketch (pp. 158f.) of his alternative theory. Lakatos, who was the Editor of the *BJPS*, sent the paper to me to referee (along with Harris 1974, which had been submitted at about the same time by a colleague of Tichý's in the Department of Mathematics at Otago). I realized at once that Tichý had discovered the defects of Popper's theory that I had pointed out at the LSE seminar, and that his new theory must, surely *must*, be susceptible to a variant of the language dependence objection for quantitative theories. It was in July 1973 also that I communicated both my results to the ASL Summer Meeting at Bristol (1974c, 1974d).

I had no hesitation in recommending Tichý's and Harris's papers for publication; and remembering that *The Journal of Symbolic Logic* had once published in one issue three papers (Shimony 1955; Lehman 1955; Kemeny 1955) that all expounded forms of the Dutch Book argument, I asked Lakatos's permission to submit to the *BJPS* for simultaneous publication a properly written exposition of my own work on Popper's theory of verisimilitude. When it eventually appeared as (1974a), it contained also a short section demonstrating that Tichý's theory is indeed language dependent. I thought that that would be the end of it. Little did I realize that minnesotan and arizonan weather would still be on the agenda 30 years later.

Language dependence may well be 'the canonical objection against the whole verisimilitude enterprise', but it has signally failed to reduce the popularity of language dependent definitions of verisimilitude, and many authors seem to have convinced themselves that it is an objection of little substance. The 29-page survey by Niiniluoto (1998) devotes fewer than three pages to the problem. The index to Kuipers's book (2000) does not mention it, and the problem is, as far as I can judge, discussed only on pp. 141f. (and in an accompanying footnote), where it is dismissed as 'at most an academic problem'. If by this Kuipers means that it is a philosophical problem, then I agree. The most substantial recent discussions are Chapter 5 of Zwart (2001) (originating in Zwart 1995 and Zwart 1998) and pp. 85–89 of Smith (1998b) (a slightly improved reprint of § 7 of Smith 1998a). Although I have in various places (especially 1976, 1978a, 1994, § 10.4*d*), responded to those who defend language dependent definitions, I think that there is still a good deal to be said, in particular about Niiniluoto's, Zwart's, and Smith's lines of defence.

After a rapid inspection in § 1 of the sectors in which battle has been waged over the language dependence of definitions of verisimilitude and approximation to truth, I shall take sword in hand and join the fighting with gusto. The order of action builds on and substantially refines § 5.2.3, entitled 'Four Kinds of Responses', of Zwart (2001), which deals only with responses to the sentential construction described in § 1.0. I have arranged the reactions of the belligerents in four groups corresponding to Zwart's fourfold division (which is to be found also in Forster 2001b). In § 2 are discussed those reactions that condemn the relativity of truthlikeness and approximation to truth that language dependence is claimed to introduce. In § 3 and § 4 are discussed a number of attempts that have been made to deny that these ideas are inevitably relativistic, in the one case by objecting to the new vocabulary (new primitive sentences, predicates, and functors) and in the other by challenging its intertranslatability or equivalence with the old vocabulary. In § 5, we turn to those reactions that acquiesce in and explain away the relativism condemned by those in the first group and contested by those in the two other groups. It need hardly be said that these groupings are quite rough, and that some of those involved in the debate will find their views discussed in both § 3 and § 4, for example. I shall try not to repeat myself.

A note on terminology: in this chapter the words 'verisimilitude' and 'truthlikeness' are used interchangeably, and signify *approach to the whole truth*. I purposely shun the usage of Zwart (2001), § 1.4.5, which employs the two terms for what (following Oddie 1987, § 1) he regards as interestingly, even essentially, different lines of solution to the problem of verisimilitude (which he sets out in *op. cit.*, Chapter 6, to unify). Verisimilitude is, however, distinguished from *approximation to truth* (Hilpinen 1976), which may be thought of as what remains of verisimilitude when content is factored out; that is, the [degree of] approximation to truth of a theory is its verisimilitude per unit of content (Miller 1994, § 10.4*c*). In what follows, § 1.0 and § 1.1 are for the most part concerned with verisimilitude; § 1.2 is concerned with approximation to truth. The remaining sections serve mixed fare.

1 A Tour of No Man's Land

In three different formalisms there have been proposed theories of verisimilitude or approximation to truth that, I argue, are language dependent in the sense of yielding different comparisons for expressively equivalent languages. In this section I describe each of them with a minimum of fuss. It is as well to stress that they are described from my point of view, not from the points of view of those whose theories are criticized. Where I see language dependence, my opponents see a host of different linguistic phenomena, as we shall discover in the sections that follow.

1.0 Sentential logic

A straightforward truth table shows that if Φ is (logically) equivalent to $\Theta \leftrightarrow \Psi$, then Ψ is equivalent to $\Theta \leftrightarrow \Phi$, which implies that if q_0 is equivalent to p_0, and q_i is equivalent to $p_0 \leftrightarrow p_i$ for $0 < i < j$, then the language based on the letters $p_0, p_1, \ldots, p_{j-1}$ is expressively equivalent to the language based on the letters $q_0, q_1, \ldots, q_{j-1}$; everything that can be said in the one language can be said in the other. In particular the maximal sentence $T^p = p_0 \wedge p_1 \wedge \cdots \wedge p_{j-1}$ becomes $T^q = q_0 \wedge q_1 \wedge \cdots \wedge q_{j-1}$ when translated into the alternative language, whereas the maximal sentences $X^p = \neg p_0 \wedge p_1 \wedge \cdots \wedge p_{j-1}$ and $Z^p = \neg p_0 \wedge \neg p_1 \wedge \cdots \wedge \neg p_{j-1}$ become $X^q = \neg q_0 \wedge \neg q_1 \wedge \cdots \wedge \neg q_{j-1}$ and $Z^q = \neg q_0 \wedge q_1 \wedge \cdots \wedge q_{j-1}$ respectively.

Tichý (*op. cit.*) proposed that in a sentential language with finitely many primitive sentences (sentence letters) two maximal sentences may be compared in distance from a third one, which may be thought of as representing 'the truth', by comparing the number of primitives on which they disagree with it, the truth. This means that the distance between X^p and T^p is 1, and the distance between Z^p and T^p is j; and also that the distance between X^q and T^q is j, and the distance between Z^q and T^q is 1. It follows that whether the theory **X**, which may be expressed both by X^p and by X^q, or the theory **Z**, which may be expressed both by Z^p and by Z^q, is closer to **T** (T^p or T^q) depends on how we express them. Note that this simple argument is a genuinely order-theoretic one that attaches no importance to the cardinal distances that it manipulates; it suffices that the set of primitives p_i on which the theory **X** differs from **T** is properly included in the set of primitives on which **Z** differs from **T**, whilst the situation is the other way round with regard to the primitives q_i. In other words, there is a purely qualitative sense in which X^p lies between T^p and Z^p, whereas Z^q lies between T^q and X^q. The construction may therefore be extended without tears to a sentential language with infinitely many primitives.

To use the graphic example with which this argument has become associated, let one meteorological language contain the three primitives h ('It is hot'), r ('It is rainy'), and w ('It is windy'), and a second the primitives h ('It is hot'), m ('It is minnesotan'), and a ('It is arizonan'). It is understood that minnesotan weather is either hot and rainy [summer] or cold and dry [winter] (that is to say, m is equivalent to $h \leftrightarrow r$) and that arizonan weather is either hot and windy [day] or cold and still [night] (that is to say, a is equivalent to $h \leftrightarrow w$). Clearly r is equivalent to $h \leftrightarrow m$ too, and w is equivalent to $h \leftrightarrow a$. Let the truth be that the weather is hot, rainy, and windy; or equivalently that it is hot, minnesotan, and arizonan. Then, according to Tichý's proposal, it is a matter of sheer linguistic formulation whether the theory that the weather is cold, rainy, and windy (equivalently: cold, counter-minnesotan, counter-arizonan) is closer to, or further from, the truth than is the rival theory that it is cold, dry, and still (equivalently: cold, minnesotan, arizonan). Verisimilitude so defined is language dependent.

The same intertranslations have been used more recently by Humberstone (2000), §§ 3, 5, to impute language dependence to a proposed construe of the difference $\Phi - \Psi$ between the propositions Φ, Ψ. What is intended here, namely a 'connective whose work is to "undo" the work of conjunction' (*op. cit.*, p. 60), might better be called the quotient Φ/Ψ of Φ and Ψ, since it inverts the logical product $\Phi \wedge \Psi$. (The inverse to logical sum, disjunction, is the remainder operation, briefly discussed in Chapter 13, § 3, below. In the lattice of contents, of course, where the ordering is reversed, 'logical subtraction' is not an inappropriate name.) The identification of Φ/Ψ with

$\Psi \to \Phi$, made by Tuomela (1973), p. 59 (who defines the *Carnap sentence* t^c of a finitely axiomatizable theory t as the conditional $t^r \to t$, which is said to be the 'analytic or conventional component' of t, what remains when its Ramsey sentence t^r is factored out), Hudson (1975), Popper & Miller (1983), and (with some misgivings) Popper & Miller (1987), manifestly does not satisfy this condition, and Humberstone rejects it. Yet as he easily shows (*op. cit.*, p. 81), we cannot define the quotient in such a way that when Φ and Ψ are atomic $(\Phi \wedge \Psi)/\Phi$ is equivalent to Ψ. For then $(h \wedge r)/h$ would be equivalent to r, and $(h \wedge m)/h$, which is the same, would be equivalent to m, which is not equivalent to r. Interesting as this problem is, it would be out of place to pursue it further here.

There is a project in monadic predicate logic too that is endangered in a similar way, namely the attempt by Watkins (1984), Chapter 5, § 13, to compare the contents of conflicting theories, and for convenience we deal with it at this point. In this case primitive monadic predicates take the place of sentence letters, but (as Watkins recognized) adverse results are again obtained by the construction of languages that are intertranslatable using only Boolean identities. Slightly varying and simplifying Watkins's definition, let us say that two compound predicates are *incongruent counterparts* if they differ only in this: the sign attached to an essential occurrence of at least one primitive predicate in one of them differs from the sign attached to the corresponding essential occurrence of that predicate in the other. For example the predicate $(Hy \to Ry) \wedge (\neg Hy \to Wy)$, written $F(y)$ (floridian), and the predicate $(\neg Hy \to Ry) \wedge (Hy \to Wy)$, written $O(y)$ (oregonian), are incongruent counterparts. It is readily checked that Ry and Wy are equivalent respectively to $(Hy \to Fy) \wedge (\neg Hy \to Oy)$ and $(\neg Hy \to Fy) \wedge (Hy \to Oy)$. It follows that in the language in which H, R, W are primitives, F and O are incongruent counterparts, but R and W are not; while in the language in which H, F, O are primitives, R and W are incongruent counterparts, but F and O are not. Incongruent counterparthood so defined is language dependent. It may be noted that the corresponding propositional language with primitives h, f, o, induces the same verisimilitude ordering as the original language with primitives h, r, w.

An earlier (rather clumsy) construction of intertranslatable sentential languages was given by Black (1964), and can be used to undermine the idea that the set of atomic facts in the *Tractatus* (Wittgenstein 1921) is unique (Miller 1978b) or even that logical space, as there characterized, has a unique dimension (Miller 1977a). A series of generalizations of this result in Miller (1974b) demonstrates that in general a predominance of truths over falsehoods in one of the logically independent axiomatizations of a sentential theory is always matched by a predominance of falsehoods over truths in another (however 'logically independent' is understood). It may be noted that the definition above of the predicates 'floridian' and 'oregonian' is not formally different from Goodman's definition of the predicates *grue* and *bleen* (1954, Chapter III, § 4): *grue* is defined to mean 'if first observed before Judgment Day then green, otherwise blue', and *bleen* is defined to mean 'if first observed before Judgment Day then blue, otherwise green'. It is inaccurate to compare 'grue' and 'bleen' with 'minnesotan' and 'arizonan' (as does Miller 1978b, p. 177), since 'grue' means 'green if & only if first observed before Judgment Day' only under the assumption that all objects are either green or blue; under which assumption, 'grue' and 'bleen', unlike 'minnesotan' and 'arizonan', are disjoint predicates, mutual contradictories.

1.1 Monadic predicate logic

As far as the verisimilitude of theories expressed in predicate logic is concerned, making the charge of language dependence stick is more intricate than might have been expected. Again discussion is restricted to the most primitive calculus of this kind, one based on a finite vocabulary of monadic predicates, and no individual constants. From j monadic predicates we may construct 2^j Q-predicates (as they were called by Carnap 1945, p. 46) or attributive constituents (Hintikka 1965, p. 52), just as in sentential logic we construct 2^j maximal theories from j sentence letters: a Q-predicate affirms or denies each of the primitives. A maximal theory of the calculus (or constituent in the terminology of Boole and of Hintikka) then states for each Q-predicate whether or not it is instantiated. Since at least one Q-predicate must be instantiated in a non-empty universe, there are $2^{2^j} - 1$ maximal theories.

Niiniluoto (1977), p. 130, and (1987a), pp. 310–313, unaware initially that he was repeating an idea advanced by Clifford a century earlier, proposed that the distance between two constituents can be measured by the number of Q-predicates about which they make different assertions. This proposal was later refined in various ways, as noted below, but the simple original idea suffices for our destructive purposes. What follows here is a summary (with slightly different notation) of the investigation of § II of Miller (1978a).

It is quite complicated enough, and also quite representative, if the vocabulary of the calculus consists of only two monadic predicates P, Q. Let the four Q-predicates be Ay, which is equivalent to $Py \wedge Qy$; By, equivalent to $Py \wedge \neg Qy$; Cy, equivalent to $\neg Py \wedge Qy$; and Dy, equivalent to $\neg Py \wedge \neg Qy$. Here are three sentences that may be formulated in this language:

$$Z^{PQ}: \quad \neg\exists u\, Au \wedge \neg\exists u\, Bu \wedge \neg\exists u\, Cu \wedge \exists u\, Du$$
$$X^{PQ}: \quad \neg\exists u\, Au \wedge \neg\exists u\, Bu \wedge \exists u\, Cu \wedge \neg\exists u\, Du$$
$$T^{PQ}: \quad \exists u\, Au \wedge \exists u\, Bu \wedge \exists u\, Cu \wedge \neg\exists u\, Du$$

Each of them expresses one of the 15 maximal theories of the language. According to Niiniluoto's proposal, X^{PQ} is closer to T^{PQ} than Z^{PQ} is; that is,

X^{PQ} has greater truthlikeness than Z^{PQ} has. For X^{PQ} differs from T^{PQ} with regard to two Q-predicates, while Z^{PQ} differs from T^{PQ} with regard to all four Q-predicates. Indeed, the set of Q-predicates on which X^{PQ} differs from T^{PQ}, namely $\{A, B\}$, is included in the set of Q-predicates on which Z^{PQ} differs from T^{PQ}, namely $\{A, B, C, D\}$.

Now an expressively equivalent language can be constructed by replacing the primitive predicate P by a predicate R where Ry is equivalent to $Py \leftrightarrow \exists u\, Qu$. In the new language there are four Q-predicates: Ay, By, which are the same as before, My, which is equivalent to $Cy \leftrightarrow \exists u\, Qu$, and Ny, which is equivalent to $Dy \leftrightarrow \exists u\, Qu$. The three maximal sentences

$$X^{QR}: \quad \neg\exists u\, Au \wedge \neg\exists u\, Bu \wedge \neg\exists u\, Mu \wedge \exists u\, Nu$$
$$Z^{QR}: \quad \neg\exists u\, Au \wedge \neg\exists u\, Bu \wedge \exists u\, Mu \wedge \neg\exists u\, Nu$$
$$T^{QR}: \quad \exists u\, Au \wedge \exists u\, Bu \wedge \exists u\, Mu \wedge \neg\exists u\, Nu$$

are logically equivalent to X^{PQ}, Z^{PQ}, and T^{PQ} respectively. But T^{QR} differs from Z^{QR} with regard to two Q-predicates and from X^{QR} with regard to four. In other words, whether the theory **X**, which is expressed both by X^{PQ} and by X^{QR}, or the theory **Z** (Z^{PQ} and Z^{QR}), is closer to the theory **T** (T^{PQ} and T^{QR}), and so has greater truthlikeness, depends on the language that we take as primitive. Truthlikeness so defined is language dependent.

As in § 1.0, the argument is order-theoretic, and attaches no importance to the measures of distance beyond the order in which they stand: as already noted, the set of Q-predicates on which X^{PQ} differs from T^{PQ} is included in the set of Q-predicates on which Z^{PQ} differs from T^{PQ}; and the reverse obtains when we consider X^{QR}, T^{QR}, Z^{QR}. There is indeed a purely qualitative sense in which X^{PQ} lies between T^{PQ} and Z^{PQ}, while Z^{QR} lies between T^{QR} and X^{QR}.

For the sake of completeness, it should not be forgotten that among the numerous complicated measures of verisimilitude in predicate logic that have been proposed, there are some that are susceptible to the more elementary criticisms outlined in § 1.0 above. Under the proposal in Niiniluoto (1978), for example, the distance between two constituents is allowed to depend not simply on the number of Q-predicates on which they differ, but also on the number of primitive predicates on which these Q-predicates differ among themselves. It is not hard to see that even simple applications of this measure, or of a measure proposed by Tuomela (1978), can be subverted by replacing primitive predicates such as P and Q by the predicates P and $P \leftrightarrow Q$, and treating these as primitive in their stead. For details see Miller (1978a), note 5, and Niiniluoto (1987a), § 13.2, pp. 452–454.

1.2 Quantitative languages

Popper (1979b), Appendix 2, pp. 372–374, gives an elegant example to illustrate how a change of language, or transformation of variables, can reverse the relative fortunes of two numerical theories. Table 11.0 below gives the values predicted by the theories **X** and **Z** for the two quantities φ and ψ, and also for the quantities η and ξ, which are defined from them. The true values are those given by **T**.

Table 11.0 Reversal of accuracy (after Popper 1979b, p. 373)

	φ	ψ	η	ξ
Z	0.150	1.225	0.925	2.000
X	0.100	1.000	0.800	1.700
T	0.000	1.000	1.000	2.000

X is plainly more accurate with respect to φ and ψ than **Z** is, and less accurate with respect to η and ξ. But η and ξ are supposed to be defined from φ and ψ by the equations $\eta = \psi - 2\varphi$ and $\xi = 2\psi - 3\varphi$, while φ and ψ are defined from η and ξ precisely analogously, by the equations $\varphi = \xi - 2\eta$ and $\psi = 2\xi - 3\eta$. The moral is plain: which of the theories **X** and **Z** we judge to be more accurate and therefore, by a widely held criterion, a better approximation to the truth depends on whether φ and ψ are the primitive quantities, and η and ξ are defined from them, or the other way round. Approximation to truth so defined is language dependent.

The particular example can be generalized. Suppose that the quantities φ and ψ are sufficiently different functions of an independent variable t. We may show that whenever **X**'s predictions for φ and ψ lie (weakly) between **Z**'s predictions and **T**'s predictions (the true values), then there are other quantities, interdefinable with φ and ψ, that reverse the ordering. In more detail, let $\varphi_{\mathbf{X}}, \varphi_{\mathbf{Z}}, \varphi_{\mathbf{T}}$ be those functions of the independent variable t that give the values that **X**, **Z**, and **T** predict for φ (and likewise for ψ). Suppose that for all values of t the predictions for φ and ψ given by **X** lie between **T**'s predictions and **Z**'s predictions. Then it may be shown that for all values of t the predictions given by **Z** for the quantity $\vartheta(t) = \varphi(t) + \lambda\psi(t)$, where

$$\lambda = \rho \cdot \frac{\varphi_{\mathbf{X}} - \varphi_{\mathbf{Z}}}{\psi_{\mathbf{Z}} - \psi_{\mathbf{X}}} + (1 - \rho) \cdot \frac{\varphi_{\mathbf{Z}} - \varphi_{\mathbf{T}}}{\psi_{\mathbf{T}} - \psi_{\mathbf{Z}}} \qquad (0)$$

and ρ is any number between 0 and 1, lie between **T**'s predictions and **X**'s predictions.

We may therefore proceed in two ways:

- Choose two different numbers μ, ν between 0 and 1, and define η, ξ as ϑ was defined, with μ, ν in place of ρ.
- Define η as before, and for ξ interchange φ and ψ, using either a different weighting factor ν or the same one μ.

$$(1) \qquad \eta(t) = \varphi(t) + \mu \cdot \frac{\varphi_X - \varphi_Z}{\psi_Z - \psi_X} \cdot \psi(t) + (1 - \mu) \cdot \frac{\varphi_Z - \varphi_T}{\psi_T - \psi_Z} \cdot \psi(t)$$

$$(2) \qquad \xi(t) = \psi(t) + \nu \cdot \frac{\psi_X - \psi_Z}{\varphi_Z - \varphi_X} \cdot \varphi(t) + (1 - \nu) \cdot \frac{\psi_Z - \psi_T}{\varphi_T - \varphi_Z} \cdot \varphi(t)$$

The latter method is more symmetrical, but not necessarily to be preferred. In each case the definitions are normally reversible (though there are cases in which singularities appear). For details see Miller (1975), § V, and (1994), Chapter 11.

It is to be noted that except in very special circumstances the quotient

$$\frac{\varphi_X - \varphi_Z}{\psi_Z - \psi_X},$$

and the others like it, are not constant functions of t. On the other side, it is not required that ρ is constant. But in general, the quantity λ can be expected to be a function of the independent variable t. We return to this point in § 4.4.

One thing that these simple transformations surely show is that the accessible empirical evidence (that is, the 'evidence we can procure by today's means') relevant to two competing theories will almost never point in one direction only. I am therefore at a loss to understand how Agassi (1981), p. 578, can venture in response to the problem of verisimilitude the suggestion that 'a theory is more verisimilar than its predecessor if and only if all crucial evidence concerning the two goes its way'. He even endorses the conjecture that '[w]hen crucial evidence repeatedly points one way it is unlikely that it also point[s] the other way' (*loc. cit.*; contrast Agassi 1976, p. 344). Unlikely perhaps, but true.

2 Unacceptable Relativism

The reactions responded to in this section maintain that truthlikeness and approximation to the truth cannot be permitted to be as language dependent as the arguments of § 1 show them to be.

2.0 *'The investigation is closed'*

Flight is a natural first reaction to a frightful event, and some writers have reacted accordingly to the proof of the language dependence of the method of assessing verisimilitude advocated by Tichý. Urbach for example concludes that 'the attempt to make sense of an objective notion of degrees of closeness to the truth for false theories is fundamentally and irretrievably misguided' (1983, p. 267, emphasis suppressed).

Response As indicated in § 0 above, and made clear also by Niiniluoto (1987a), p. 456, language dependence hits only 'essentially syntactic [or]... "linguistic" definitions of truthlikeness' and approximation to truth. These approaches by no means exhaust the possibilities, and a reaction more restrained than panic is the appropriate one. I stress again that I have never claimed 'that it is impossible to "make... correct judgements of... comparative proximity to the truth" ', as alleged by Tichý (1976), p. 34. On the contrary, I have tentatively volunteered several such judgements. See for example the opening paragraph of my (1976), § 2, and also § 5.

2.1 *'One day a solution will be found'*

Although Schurz & Weingartner judge similarly that to 'require language-independency in Miller's sense... would trivialize or destroy the whole idea of verisimilitude' (1987, p. 50), they are more positive than Urbach is about the prospects for a decent theory of verisimilitude, and indeed provide a language dependent theory of their own. They adopt a policy of pretending that the problem of language dependence is susceptible of some solution or other, but do not pursue the matter. In a different context Paris (1994), p. 191, also notes the objection but does not pursue the possibility of answering it.

Response The only appropriate response is to suspend judgement.

2.2 *'Science aims at knowledge, not merely at truth'*

E. C. Barnes too reaches an interim conclusion that is much the same as Urbach's: 'the entire project [of providing a theory of verisimilitude]... is, to some extent, misconceived... [since] Miller has succeeded in showing... that the notion of "truthlikeness" is a non-objective notion' (1991, p. 310). He goes on to offer a theory of scientific progress that is claimed to be immune to the language dependence argument. Stated simply, the idea is that although 'one of the most crucial aims of science is truth' (*loc. cit.*), this is because truth is one of the principal components of knowledge (as that term is understood by knowledge professionals); and there are other conditions on knowledge, which Barnes holds to be the real aim of science, that can be mobilized to disqualify the translations proposed above. Indeed, 'while... the number of true atomic sentences of a false theory is language dependent, the number of known sentences ... is conserved under translation' (p. 309). Barnes (1990) applies similar ideas to the arguments of § 1.1 above.

Response Even if Barnes is right that it is knowledge, rather than truth, that science aims at, a Platonic doctrine criticized in Chapter 3, § 4 and Chapter 5, § 7*g* of Miller (1994), the problem remains of giving a theory of truthlikeness (which no one has yet shown not to be objective), approximate truth, and of the distance between theories; in particular, this is needed to sustain the idea, central to 'scientific realism', that the success of present-day science is to be explained in part by its approximate truth. Similar remarks pertain to Barnes's later account (1995) of scientific progress in terms of 'approximate scientific explanation'.

3 Unacceptable Vocabulary, Acceptable Translations

It is not denied that the new predicates ('minnesotan' and so on) introduced in § 1.0 and § 1.1, and the new quantities introduced in § 1.2, are in many cases artificial. A constant theme is that they are unacceptably artificial, even if in some sense they are mutually definable with and translatable into perfectly acceptable items of regular vocabulary.

3.0 'Natural language is more natural'

Watkins, for example, claims that such predicates as 'minnesotan', 'arizonan', are 'undersensitive and oversensitive to observable differences' and that 'we want our predicates to be well adjusted to those properties with which we are concerned' (*op. cit.*, p. 181f.). These are the predicates in terms of which we should decide whether two compound predicates are or are not incongruent counterparts. In a similar way Brink & Heidema (1987), p. 548, write: 'the *h-m-a* language simply does not fit the world of heat, rain and wind. After all, it seems excessively awkward to herald rain by shouting "Hark! It is hot if and only if it is Minnesotan!".' Read straight, this passage seems to acknowledge that 'It is hot if and only if it is Minnesotan!' is a legitimate way of saying 'It is raining', merely a cumbersome and prolix way of doing so; that is, that r is indeed a translation of $h \leftrightarrow m$. No doubt this is why Zwart (*op. cit.*), pp. 173f., associates these authors (about whose approach I shall have more to say in § 4.3 below) with the '*privileged language argument*', which he describes as an 'objection of epistemological asymmetry'. Different forms of this objection will recur persistently throughout this section.

Response Projects to compare theories for content, or for verisimilitude, have never been concerned only with statements formulated in an observation language. It is beside the point, though perhaps true, to say that the constructed predicates are epistemologically less accessible, or functionally more unwieldy, than those from which they are defined. In theoretical science, after all, even natural history, most predicates and functions are highly artificial; that is, they are historically dependent on a more matter-of-fact vocabulary, but no longer regarded as reducible to that vocabulary. Think of 'acid', 'migrates', 'marsupial', 'energy'. What is more, several authors have sketched fictional contexts in which terms such as 'minnesotan' are quite natural. Mott, for example, writes (1978, pp. 251f.):

> Let us imagine a primitive tribe dependent upon hunting for the means of subsistence. When the weather is wet and windy, but at no other time, they are able to approach their prey close to and club it to death. They also have bows and arrows, but if it is windy they can't shoot straight and if it's wet the feathers fall off with the same effect. One member of the tribe may say to another that it's hunting weather. By this he means exactly that it's wet if & only if it is windy.... But in his language this is said by a single sentence ... 'It's hunting weather'.

Barnes (1991), p. 313, suggests that 'the statement "It's Minnesotan" might serve an important purpose – say, if one's children's schools close on hot and rainy, or cold and dry days, but are otherwise open.' Forster imagines 'an alien culture living in a valley where they grow two kinds of corn: Minnesotan corn and Arizonan corn.... They need to tend to the Minnesotan corn if & only if the weather is Minnesotan' and so on (2004, p. 9).

3.1 'Odd predicates do not designate properties'

The intrusion of pragmatic factors does not necessarily mean a surrender to relativism or subjectivism. A perspicuous vocabulary or system of notation may confer objective benefits, even if its expressive power does not exceed that of its competitors (think of the advantages of Leibniz's notation for the calculus over Newton's, or of Peano's logical notation over those of Frege and of Chwistek 1939). Zwart (*loc. cit.*) is correct nonetheless to say that '[t]he privileged language argument deprives truthlikeness of its objectivity' if the privileged position that one language enjoys over another is supposed to be earned by mere custom or habit or operational smoothness. Objective truthlikeness (and truth), as classically understood, are not dependent on pragmatic features such as convenience and simplicity. A number of authors have offered in response what can only be intended as more immanent explanations for the superiority of some languages over others. One of them is Niiniluoto, who at the same time is prepared to join forces with the doctrine that 'truthlikeness is pragmatically ambiguous' (see § 5.0 below).

Whatever Niiniluoto's view may be about molecular predicates like 'minnesotan' (contrast his 1978, note 10, and his 1987a, pp. 450–452), he is in no doubt that the new predicate Ry defined by $Py \leftrightarrow \exists u\, Qu$ in § 1.1 above

> has a very strange character: it does not designate a monadic or a relational property. The question of whether or not R applies to an individual y cannot be decided on the basis of the genuinely monadic properties of y and the relations which y bears to other individuals. Rather it depends upon the question whether there exist individuals satisfying the predicate Q, even though this question is quite 'accidental' so far as y is concerned, as it is not required that y has any relationship with these other individuals. Let us say that this sort of predicates are *odd predicates*.

(*Op. cit.*, p. 457; again I have brought the notation into line with that of this chapter.)

Niiniluoto goes on to suggest that 'odd predicates do not designate any properties at all' (p. 458). Watkins takes a similar, though more moderate, stand. Both cite in support of their views (different) writings by Armstrong. Along the same lines, Tuomela *op. cit.*, note 4, declares that 'obviously R cannot possibly be a *nomological* predicate (or a "good" scientific predicate), *given that Q is*, no matter how one analyzes nomologicality'.

Response Odd predicates are not as odd as Niiniluoto would have us believe. Consider for example the predicate 'appropriately dressed'. At least in some parts of Europe, from September to April you are (as far as bodily comfort is concerned) appropriately dressed if & only if you wear warm clothing, while at other times you are appropriately dressed if & only if you do not wear warm clothing. We may define Ay as $Wy \leftrightarrow r$, where r is here the sentence 'There is an "r" in the month'. The example is admittedly not perfect. Here is another one, slightly more complex. I am interested whether my pension provision will suffice for my needs in old age. My assessment is that it will suffice if inflation is kept firmly under control, and not otherwise. How is my status to be described? I suggest it may be described by the predicate $P(y) \leftrightarrow \exists u\ Q(u)$, where P is the predicate 'penurious', and Q the predicate 'spendthrift Chancellor'. Neither 'appropriately dressed' nor this predicate is so very odd. Nor is the predicate 'happy if & only if there are no casualties'. And what about the predicate 'odd predicate' itself? It is at least debatable whether the oddness, or lack of oddness, of a predicate A ('appropriately dressed') can be decided 'on the basis of the genuinely monadic properties of ... A [whatever 'genuinely monadic' properties are], and the relations which ... A bears to other predicates'. To be sure, A is odd if there exist a genuinely monadic predicate W and a sentence r such that Ay is equivalent to $Wy \leftrightarrow r$. But it is 'not required that ... A has any relationship' with either W or r.

Tarski's definition of satisfaction says that the formula $\exists u_j X$ is satisfied by the sequence **m** if & only if X is satisfied by a sequence that differs from **m** in at most the jth place. This seems to make the satisfaction of $\exists u_j X$ by **m** depend not just on properties of the formula $\exists u_j X$ and its relation to other formulas, or on properties of **m** and its relation to other sequences, but on the existence of a sequence **n** satisfying X, 'even though this question is quite "accidental" so far as ... [the formula $\exists u_j X$ and the sequence **m** are] concerned'. It would be helpful if Niiniluoto could further clarify what it takes for a predicate to be odd, and to say whether it is disturbing that the central predicate of semantics appears to be an odd one.

A minor remark that may be made about Niiniluoto's reaction is that his characterization of odd predicates seems to exclude from the category of oddness predicates of the form $Py \leftrightarrow \exists u\ Quy$; for whether 'there exist individuals satisfying the [relational] predicate Q' is presumably not 'quite "accidental" so far as y is concerned'. It would be worth investigating whether such predicates can be used to establish the language dependence of his measures of verisimilitude (and those of others) for polyadic languages.

As for Tuomela's dismissal of the constructed predicates on the grounds of their not being nomological, 'no matter how one analyzes nomologicality', it may be noted that the universal statements $\forall y\ (Py \rightarrow Qy)$ and $\forall y\ (Py \rightarrow Ry)$ are logically equivalent, so that if one is lawlike so is the other. If nomological predicates are those that occur in lawlike statements, as I rather thought they were supposed to be, then the defined predicate R seems just as good as the predicate Q that features in its definiens. For further criticism of Tuomela's reaction, see Miller (1978a), note 5.

3.2 *'Only physically significant quantities matter'*

There are some interesting connections between approximate truth and chaos. For one thing, the existence of chaotic systems calls dramatically into question the value of theories of approximate truth, since these systems infringe the specious principle that the deductive consequences of approximately true theories are themselves approximately true (Weston 1992, § 2; the principle was cautiously stated in my talk 1980b, and by Laudan 1981, pp. 30f.); infringe that principle, that is, if approximate truth is identified with closeness of numerical values, as it is universally taken to be. This identification, for all its obviousness, is diminished by the results of § 1.2.

Another connection, of more immediate concern, is that 'worldly phenomena of the kinds typically modelled by chaotic theories cannot exemplify in their time-evolutions the infinitely intricate patterns characteristic of chaos' (Smith 1998b, p. 71), and that in consequence chaotic theories, theories of non-linear dynamics, cannot be strictly true. But, Smith maintains, 'they can still be more or less *approximately* true' (*loc. cit.*). He does not pretend to provide a general account of approximate truth, but proposes a definition for 'geometric modelling [GM] theories', theories that set out to describe trajectories in physical space. The proposal, in short, is that greater approximation to truth is to be gauged (at least in the simplest cases of GM theories) by greater accuracy. Smith recognizes that this proposal is hit by the arguments and constructions of § 1.2. His way of dealing with the difficulty (*op. cit.*, pp. 85–89) is to dismiss as in general illicit the concocted quantities used to reverse orderings of numerical accuracy, a similar defence to that embraced by Niiniluoto (see § 3.1 above), though the specifics of the arguments are quite different. In this subsection, and in § 4.4 and § 5.2 below, I shall address and criticize several aspects of Smith's response.

Although, as we shall see in § 4.4, Smith takes exception to transformations of the kind introduced in § 1.2, the criticism on which he evidently relies for most support makes a distinction between physically significant quantities and those that have no significance (in which class, of course, are most of the manufactured ones). He writes (p. 88):

> But if... [the concocted quantities] aren't of interest, why care whether a theory close-tracks them [that is, provides accurate values for them]? Dynamical theories aim to track the time evolution of physically significant quantities, and a theory will count as approximately true just so long as it gets the values of *those* quantities near enough right for long enough....

In a similar but earlier reaction to the charge of language dependence, Weston (*op. cit.*), p. 68, protects his theory of approximation to truth by claiming that some quantities in a theory are 'causally significant', and others (such as centrifugal forces in classical mechanics) not significant; and that it is only the significant ones that 'strong realism' is concerned to approximate.

Response A concentration on physically significant quantities would be splendid, to be sure, if it were clear what makes a quantity physically significant. Smith suggests (*loc. cit.*) that 'the quantities represented by variables in a particular dynamical theory... will (according to other theories) feature in a wide range of functional relationships to other quantities such as pressure, volume, viscosity, etc.' Yet since the quantities η and ξ are defined in terms of φ and ψ, they feature in just as many functional relationships as do φ and ψ (Miller 1975, § VI). If quantities are interdefinable, no tenable distinction can be drawn between those that do and those that do not participate in universal laws. To imagine that those quantities that occur in the laws as they are formulated at present are automatically to be preferred is a kind of essentialism.

The Stefan/Boltzmann law of black-body radiation states that 'the total emissive power J, or total energy of all wavelengths emitted per unit time and per unit area, is directly proportional to T^4, the fourth power of the absolute temperature of the surface' (Holton & Roller 1958, § 31.3, emphasis suppressed). This suggests that those who think that some magnitudes are causally significant may count T^4 as a causally significant magnitude. Yet it is hard to credit that anyone would have imagined any such thing prior to the derivation of the law from Maxwell's theory (and its success in tests). A sceptic such as myself can only wonder about T^3 and T^5, not to mention other functions of T that might turn up in laws in the future. The same can be said about λ^{-5}, which is a factor in the expression given by Planck's law for J_λ, the emissive power of a surface at wavelength λ (*op. cit.*, § 31.7). Forster (2004), p. 9, observes that causal significance seems not to be an empirical

characteristic of a magnitude; certainly no one has suggested how we may submit a claim of causal significance to independent test. It is therefore worrying that, according to some authors, the truthlikeness of empirical theories is crucially dependent on it. For further critical comments on the usefulness here of the idea of causal significance, see Wilson (1993), § 13.

Weston disagrees, and holds that we can obtain by induction knowledge of what are the empirically significant quantities. He states that 'the brand of realism I wish to amplify and support is unregenerately inductivist' (*op. cit.* p. 55), meaning that evidence for the truth of a theory constitutes evidence also that the significant quantities are truly those that the theory deems to be significant. I hope that I may therefore be forgiven in turn for harbouring doubt as well as perplexity. Even if I could make sense of the idea of causal significance, I should not like it that a false theory can be judged approximately true simply because it performs well on those quantities that it falsely identifies as causally significant. Nothing said by Weston or by Boyd (1973), on whom he frequently relies, provides any satisfactory answer to questions such as this.

It should be remembered that most quantities measured in physics laboratories are contrived quantities by any intuitive standard, yet predicting accurate values for them is normally thought to be an aim of experimental science. I do not know whether any such quantities (such as the distances between specks on photographs) count as physically or causally significant. See also the example in Table 11.5 below. It should be remarked too that the urge here criticized to reduce the class of genuine physical quantities gets little support from physics itself. Wilson (*op. cit.*, p. 76) quotes from Thirring (1978, p. 5): 'We should therefore allow arbitrary functions of coordinates and momenta as observables, subject only to boundedness and, for mathematical convenience, differentiability.' Wilson's own analysis shows dramatically that the class of functions that are explicitly definable from intuitive primitives are the least problematic of all. What are much more in need of scrutiny are those quantities, like the eccentric anomaly of a planet ('roughly... its angular position in the solar sky as a function of time'), that evidently exist but are not evidently susceptible of any mathematical definition (Wilson *op. cit.*, p. 75).

A word or two should be said about the example that Smith himself introduces into the discussion. To call attention to Smith's peculiar concern with trajectories in physical space (see § 4.4 below), the example is presented in the first four columns of Table 11.1 using x and y (rather than φ and ψ) as

Table 11.1 Reversal of accuracy (after Smith 1998b, p. 87)

	x	y	x^*	y^*	$y=f(x)$	$y^*=f^*(x)$
Z	$5t$	$3t$	$t/2$	$-3t/4$	$y=3x/5$	$y^*=-3x^*/2$
X	$4t$	$2t$	t	$-t$	$y=x/2$	$y^*=-x^*$
T	t	t	$-t/2$	$t/4$	$y=x$	$y^*=-x^*/2$

the initial coordinates, which are transformed by the equations $x^* = x - 3y/2$ and $y^* = y - 3x/4$ into the new coordinates x^* and y^*.

The final two columns of Table 11.1, which have been added at the suggestion of Deryck Horton, show how the quantities y and y^* vary as functions of x and x^*. Since x and y are spatial coordinates expressed as functions of the time t, the function f is the trajectory through the points with those coordinates; and likewise for x^*, y^*, f^*. (Compare also Wilson *op. cit.*, p. 77, text to note 5.) What is interesting is that although **X** is more accurate than **Z** is on x and y, it is less accurate on the trajectory f; and although **Z** is more accurate than **X** is on x^* and y^*, it is less accurate on f^*. In other words, the transformations concocted by Smith are hardly needed in order to demonstrate that what is more accurate from one perspective may be less accurate from another perspective. Indeed, if we are interested in the movements of particles in physical space, then the separation of their trajectories seems as valid a quantity to be interested in as the separation of their individual x and y coordinates.

4 Acceptable Vocabulary, Unacceptable Translations

A number of authors accept that the new propositions and predicates introduced in § 1.0 and § 1.1, and the new quantities introduced in § 1.2, are legitimate, but from a number of different points of view deny that they are in any rewarding sense equivalents of their predecessors.

4.0 'Translation is indeterminate'

Watkins asks how a dictionary between the *H-R-W* and *H-F-O* languages could have been established: 'are we to suppose that its compiler discovered an empirical correlation between uses of, for example, *F*...and uses of $(Hy \rightarrow Ry) \wedge (\neg Hy \rightarrow Wy)$?' (1984, § 5.14, p. 179; I have brought Watkins's notation into line with my own). He goes on: 'But empirical correlations do not establish semantic equivalences, as we know from Quine.'

Response The answer is straightforward. The new terms 'floridian', 'oregonian', and so on were first introduced by stipulative definition. Having acquired a use, they usurped the place of the original primitives 'rainy' and 'windy'. This story is of course somewhat fanciful as far as these particular predicates are concerned, but it is not at all fanciful with regard to 'congruent', which was not a primitive term for Euclid but became a primitive for Nicod and for Tarski (see § 4.3, below), or with regard to many other terms in science ('action', for example). In any event, Quine's arguments for indeterminacy were concerned not with '[t]ranslation between kindred languages...[which] is aided by resemblances of cognate word forms, [nor] translation between two unrelated languages,...[which] may be aided by

traditional equations that have evolved in step with a shared culture' but 'radical translation, i.e., translation of the language of a hitherto untouched people' (1960, § 7, p. 28). There is no suggestion that meteorologists who prefer to describe the weather in geographical terms constitute an untouched people.

4.1 'The argument proves too much'

'An argument which purports to show that the notions of accuracy, truthlikeness, structure, change, sameness of state, confirmation and disconfirmation, are all spurious (or "fail to have any objective significance at all") must harbour a defect somewhere' (Oddie 1986a, § 6.5, p. 158). Kuipers (2000), p. 141f., concurs. Schurz & Weingartner (*op. cit.*), p. 50, Niiniluoto (1998), § 6, p. 16, and Zwart (2001), § 5.5.2, p. 189, agree about the pervasiveness of language dependence, without drawing the conclusion that on this account arguments that appeal to it must inevitably involve a mistake.

Response Compare: 'Mystery, being everywhere, is therefore nowhere' (Infeld 1941, Book Three, Chapter 9, p. 251) and 'The universality of Humean scepticism is also its weakness' (Sokal & Bricmont 1998, p. 53); I explain above in Chapter 6, § 1, why sceptics should be universal sceptics. One might as well say that an argument that purports to show that there are no such things as banshees, ghosts, goblins, kelpies, trolls, warlocks, werewolves, and witches must harbour a defect somewhere. But as already noted, Oddie's fear is somewhat misplaced. It is not that the notions that he lists have no objective significance (though some have little), but that he insists on defining them in such a way that they have no objective significance. For Oddie's identification of the supposed defect in the language dependence objection, see § 4.3 below.

4.2 'According to Tarski, truth is language dependent'

'Truth and verisimilitude are of course, language-independent in that they cannot be excogitated from the mere *definition* of the language in hand.... But the assertive *force* of...statements is an exclusively linguistic matter... [and] it would be absurd to demand that the truth and verisimilitude of a statement should be independent of its assertive force,' wrote Tichý (1976), p. 36. Earlier Tichý had claimed in correspondence (PT to DWM, 14 August 1973) that, according to Tarski's definition, which we all accept, truth is language dependent, and asked why we should expect verisimilitude to be any different.

Response Tarski's definition tells us (via Convention T) that the truth value of a sentence is determined by two things: its assertive force ('meaning'), and the way the world is. There is no third factor. The same dependences hold also for verisimilitude. It is a language dependent idea only in the sense that the verisimilitude of a sentence depends in part on what the sentence asserts. Anyone who denies that $\neg h \wedge r \wedge w$ and $\neg h \wedge \neg m \wedge \neg a$ have the same verisimilitude must therefore deny that they have the same assertive force. See further § 4.3 below.

4.3 'These are not genuine translations'

In § 3.0 and § 3.1 we reported the objection that the *h-r-w* language is epistemologically or pragmatically (or in some other way) objectively preferable to the *h-m-a* language. A contrasting view judges the primitives of the one set to be objectively as good as those of the other, but to be so differently applicable that there can be no talk of translation between the two. As noted above in § 4.2, those, such as Tichý, who deny that $\neg h \wedge r \wedge w$ and $\neg h \wedge \neg m \wedge \neg a$ have the same verisimilitude must deny also that they have the same assertive force. Tichý goes further and denies even that $h \wedge r \wedge w$ and $h \wedge m \wedge a$ have the same force, though he grants them equal verisimilitude (1976, pp. 35f.):

> Now what is the affirmative force of a statement? It has been a commonplace at least from Carnap's *Meaning and Necessity* to identify this force with the *range* of that statement.... Now *h* & *r* & *w* is a statement in *h-r-w*-ese, hence its range consists of functions from {*h*, *r*, *w*} to the truth-values; *h* & *m* & *a*, on the other hand, is in *h-m-a*-ese, hence its range consists of functions from {*h*, *m*, *a*} to the truth-values. Since {*h*, *r*, *w*} and {*h*, *m*, *a*} are clearly two distinct sets of propositions, no function defined on the former set can be identical with one defined on the latter.... The two languages have completely different logical spaces and therefore no statement made in one of them is translatable into the other.

Similar claims are made by Oddie (1986a), § 6.1, p.141, and (2001), § 6. It seems to be the view too of Brink & Heidema (*op. cit.*), pp. 547f.:

> A world... is made up entirely of atomic facts: different atomic facts yield different worlds. And we take the world comprised of the atomic facts that it is hot, raining and windy to be different from the world comprised of the atomic facts that it is hot, Minnesotan and Arizonan.
>
> ... the *h-r-w* language [is]...'more fundamental' than the *h-m-a* language in order to describe the world of heat, rain and wind. And the *h-m-a* language is more appropriate to the world of heat, Minnesotaness and Arizonaness, which is a different world.

This passage may be contrasted with the passage quoted in § 3.0 above, in which the redescription of rain as weather that is hot if & only if minnesotan is set aside by Brink & Heidema as 'excessively awkward' (but no more).

Response There are many examples in abstract algebra of theories that can be formulated in different ways using different primitive vocabularies. The simplest is perhaps the theory of ordering, which may be based on the primitive $<$ or on the primitive $\leq$ (or indeed on $>$ or $\geq$). The axioms of group theory are standardly presented in terms of the group operation (represented by concatenation), the inverse operation $^{-1}$, and the unit element 1. But the theory may be axiomatized also by the single statement $xxx\rho y\rho z\rho xx\rho x\rho z\rho\rho\rho = y$, where ρ is the operation of right division (Higman & Neumann 1952; Cohn 1965, p. 165). It is well known to students of classical elementary logic that there are several distinct sets of sentential connectives in terms of which all the others may be defined: for example, $\{\neg, \rightarrow\}, \{\downarrow\}$, and $\{\leftrightarrow, \nrightarrow\}$. Euclidean geometry can be axiomatized using a variety of primitives distinct from those used by Euclid (*point*, *line*, *distance*). Nicod (1930), Chapter II, mentions *congruence* and *sphericity* as two primitives that, in association with set-theoretical ideas, separately suffice for the formulation of geometry. For a version of geometry that dispenses with all set-theoretical ideas, Tarski (1959), p. 17, adopts *betweenness* and *congruence*. Even elementary arithmetic can be formulated with unusual primitives: for instance, the set $\{0, \leq, \cdot\}$, which contains symbols for neither successor nor addition, suffices (Boolos & Jeffrey 1974, p. 220; Boolos & others 2002, p. 295). Classical physics too can be presented indifferently in equivalent Lagrangian and Hamiltonian formulations.

It may be admitted that, strictly understood, different choices of primitives lead to different structures: a Boolean lattice $\langle B, \preceq \rangle$, for example, is palpably not the same structure as any Boolean algebra $\langle B, \cap, \cup, ', \perp, \top \rangle$. For an explanation of why two such structures are nonetheless regarded as equivalent, the reader may consult Nicod (*loc. cit.*), or Kanger (1968), or Pearce (1983), § 2, or Hodges (1997), § 2.6. Only writers in the truthlikeness debate, as far as I know, have contested the mathematicians' commonplace that the same theory permits equivalent formulations using different primitives, but no one has ever properly explained why the mathematicians are wrong. It hardly suffices to appeal to the principle that 'the world has a structure', of which Oddie writes: 'if there are any respectable theses at all, that...must surely be among them' (*op. cit.*, § 6.3, p. 149). For if this principle means that the world has one structure rather than an equivalent one, then it deserves no respect at all. (One must not confuse the truism that 'structural laws', if there are any, are objectively true with the sophistry that the world has an objective structure. See Chapter 4, § 6, above.) But it hardly suffices either to appeal to the contrary principle that 'the world has no structure', as Brink & Heidema do (*loc. cit.*): 'We do not believe, Wittgenstein-like, that the real world out there is conveniently divided into basic building blocks called atomic facts. But...we do believe that we can model the world. Not all at once, probably, but focusing attention on one particular aspect or context at a time.' What is left unexplained here is why heat, minnesotanity, and arizonanity are aspects of the world so different from heat, rain, and wind, in terms of which they can be defined, that a model appropriate to the latter is inappropriate to the former. In any case it seems needlessly ascetic, even myopic, to withdraw from the real world, with its abundance of rain, heat, wind, minnesotan weather, and arizonan weather, into models in which only some of these climatological features are in focus. We shall return to this point more forcefully in § 5.4 below.

Further criticisms of the claim that the purported translations are not genuine translations may be found in Urbach (*op. cit.*) and in Pearce (*op. cit.*) The points made here suffice to deal also with the proposal of Berkson (1985) that the accuracy of a theory should be assessed only with regard to what he calls its 'direct claims', and that their logical consequences (and equivalents given background knowledge) should be ignored.

4.4 *'Transformations should not be time dependent'*

In his discussion of how reports of chaotic motion can be approximately true, part of which was criticized in § 3.2 above, Smith pays no attention to the general proof reported in § 1.2 that there is always a transformation of variables that reverses the ordering by accuracy of any set of sufficiently different hypotheses in the independent variable t, and considers only an illustrative example (Miller 1994, p. 230), shown slightly simplified in Table 11.2.

Table 11.2 Reversal of accuracy (after Miller 1994, p. 230)

	φ	ψ	η	ξ
Z	t	$5mt$	$t+25a/12$	$5mt^2/2a+5mt$
X	$t+a$	$2mt$	$t+22a/12$	$5mt^2/2a+9mt/2$
T	$t+2a$	mt	$t+29a/12$	$5mt^2/2a+6mt$

In this example the theories **X**, **Z**, and **T** state explicit functional forms for the quantities φ, ψ, η, ξ, where η and ξ are defined from φ and ψ by the equations $\eta = \varphi + (5a/12mt) \cdot \psi$ and $\xi = (5mt/2a) \cdot \varphi + \psi$. Smith omits the constant m which was introduced in part to deflect objections concerning dimensionality, such as the objection that Smith himself raises and answers on (1998b, p. 88). More significantly, he writes x, y, x^*, y^* for φ, ψ, η, ξ, in order to stress that, in most of the cases in which he is interested, the dependent variables are spatial coordinates. The transformation from φ, ψ to η, ξ is to be thought of as a transformation of coordinate axes. In the same way Smith takes t as a time coordinate, although in principle it could be any independent variable.

Questioning the legitimacy of the transformations in the example, Smith writes (p. 86; again I have brought the notation into line with that of this chapter):

> The coordinate transformation required to reverse the fortunes of **X** and **Z** is time-dependent in a pretty odd way. Consider: a constant unit line along the original φ-axis becomes...a line whose length grows with time.... With this kind of gerrymandered time-dilation, no wonder we can get strange results ('lengths' will vary without cause or effect, laws will cease to be time-invariant, and so forth).

The numerical example devised by Popper (§ 1.2 above), and Smith's own example (§ 3.2 above) show that there exist cases in which a reversal of accuracy can be contrived without resort to time dependent transformations. It is plain that each of the quantities in these examples may be functions of t, yet the transformation equations, together with their inverses, are time independent. Smith concludes (without the ghost of a proof) that '[o]nly in special cases can well-behaved transformations lead to reversal' (p. 88). After that, he drops the objection.

Response The one-dimensional Galilei and Lorentz transformations are time dependent in much the way that Smith finds obnoxious.

GALILEI	LORENTZ
$x^* = x - vt$	$x^* = (x - vt)/\surd(1 - v^2/c^2)$
$t^* = t$	$t^* = (t - vx/c^2)/\surd(1 - v^2/c^2)$

No one thinks that on this account lengths in classical and relativistic mechanics vary 'without cause and effect' (the movement of one frame relative to another explains why some lengths change), nor that classical and relativistic laws 'cease to be time-invariant'. Of course they are not time invariant if that means that a law $f(x, t) = constant$ in one frame transforms into the same law $f(x^*, t^*) = constant$ in a frame in uniform relative motion. A suitcase at rest on a station platform is not at rest relative to a train that passes through the station. But the moving frame does obey a law $f^*(x^*, t^*) = constant$, where the form of the function f^* is straightforwardly obtained by applying the coordinate transformations. Smith's objection is accordingly without much force even in the case of greatest interest to him, in which the quantities transformed are spatial coordinates. It is even flimsier in the more general case, where what are transformed are physical magnitudes such as temperature and circulation velocity (see Smith *op. cit.*, the foot of p. 87).

At one point Weston too, referring to Boyd (1973), asserts that 'mathematically equivalent versions' of General Relativity, 'one with real gravity and the "wrong" geometry, the other with fictional gravity and the right geometry' can have 'potentially different empirical consequences' (1992, p. 69). If this assertion, which is at best tenuously related to anything said by Boyd in the cited paper, means that a theory can acquire or lose consequences by being translated into a formally equivalent language, then it is false. It is formal equivalence that is at issue here.

It may be admitted that the transformations (1) and (2) are not straightforward linear transformations, and it is not quite obvious how they might arise from a change in the frame of reference. Horton has suggested that if the new coordinate axes rotate (at different angular speeds) to the old ones, then something like (1) and (2) might be the appropriate transformations. He has drawn my attention also to the use in general cosmology of comoving coordinates, described thus by S. Weinberg (1972, p. 413): 'One can imagine the comoving coordinate mesh to be like lines painted on the surface of a balloon, on which dots represent typical galaxies. As the balloon is inflated or deflated the dots will move, but the lines will move with them, so each dot will keep the same coordinates.' Any transformation between standard coordinates and comoving coordinates is of course time dependent, and perhaps time dependent 'in an anomalous way' (as Smith later describes the transformations under discussion). I conclude that the existence in physics of time dependent transformations is hardly to be doubted. I have no wish to defend any naturalistic doctrine to the effect that scientific practice can do no wrong (see Chapter 4, § 5), but I do think that an objection to an established scientific practice needs to do more than take umbrage at its imagined unnaturalness. According to Wilson (*op. cit.*), pp. 78f., 'finding a complete set of foliating quantities [for a dynamical system] evinces a skill in mathematical gerrymandering comparable to that of our [that is, the USA's] best party bosses'.

In conclusion it should be noted that whenever a theory **X** performs better (more accurately) on the values of the quantities φ and ψ than does the theory **Z**, where each of the theories **X**, **Z**, **T** (the truth) asserts that φ and ψ are constant multiples of t (or more generally, constant multiples of some fixed function $g(t)$ of t), then there exist any number of time dependent transformations that reverse the ordering by accuracy. This can be seen by looking at equations (1) and (2) in § 1.2 above. Each of the quotients in these equations is a constant, and accordingly the transformations η and ξ take the forms $\varphi(t) + \sigma\psi(t)$ and $\psi(t) + \tau\varphi(t)$, where σ and τ are constants. It is easy to check that Smith's preferred transformation, which for clarity we now write as $\eta = \varphi - 3\psi/2$ and $\xi = \psi - \varphi/4$, is obtained by taking $\mu = 1/2$ and $\nu = 5/6$.

4.5 *'Homeomorphisms need not preserve metric structure'*

Without giving any indication of why the sentences Z^{PQ} and Z^{QR} of § 1.1 are not translations of each other, Niiniluoto (1987a, p. 454) suggests that the solution to the problem of language dependence is that 'Miller's concept of intertranslatability is too weak, that is, too liberal'. What he means is evidently that the requirement that verisimilitude comparisons be invariant under all translations is too strong. In a later paper (1998, § 7, p. 16) he writes:

> A diagnosis of Miller's examples shows that they are instances of a well-known fact in mathematics: homeomorphisms or continuous bijective mappings between topological spaces need not preserve the metric structure.... So Miller's invariance requirement is too strong: it is reasonable to expect that truthlikeness orderings are preserved only under a proper subclass of homeomorphisms.

He gives an example, reproduced in Figure 11.0, in which the relative distances between three non-collinear points in $\mathbb{R}^2$, namely $a = \langle 0,0\rangle, b = \langle\alpha, 0\rangle$, and $c = \langle\alpha, -\alpha\rangle$, are reversed by the bijection f that sends the point $\langle x,y\rangle$ to the point $\langle x, x+y\rangle$. For $\partial(a,b) = \alpha$ and $\partial(a,c) = \alpha\sqrt{2}$, while $\partial(f(a),f(b)) = \alpha\sqrt{2}$ and $\partial(f(a),f(c)) = \alpha$. Much the same observation is offered by Brink (1989), p. 200, who notes that 'least squares fit is not preserved by every linear transformation, and the technique has prospered despite being thus not invariant under translation'. Williamson too uses features of the standard metric on the real numbers to conclude that '"— is nearer to $\cdots$ than to _" does not express a topological relation' (1981, p. 20).

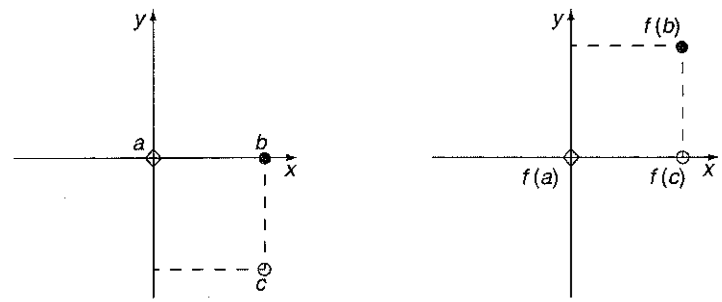

Figure 11.0 Translation of $\mathbb{R}^2$ on to $\mathbb{R}^2$ (after Niiniluoto 1987a, p. 454)

Response In each of the three principal examples given in § 1, the theories in question, whether in the original vocabulary or in the invented one, are collinear. One of the theories, that is to say, lies strictly between the other two. The maximal theory X^{PQ}, for example, lies strictly between the maximal theories Z^{PQ} and T^{PQ}. No metric for truthlikeness is mentioned. This feature of the examples – that each of them is non-metrical, so that monotone changes of scale make no difference to any of the comparisons made – has been overlooked, or disregarded, not only by Niiniluoto, but by Agassi (1975), pp. 203f., Berkson (1985), p. 317, Mormann (1988), § 2, and Weston (1992), p. 68. Now a homeomorphism from a space into itself cannot upset betweenness relations. Niiniluoto's example is therefore quite irrelevant. No one expects a truthlikeness metric to be preserved under all homeomorphisms; but the truthlikeness betweenness relation is a different matter.

A rejoinder that could be made at this point would be to disavow any interest in the topological relation of betweenness on which, I maintain, Niiniluoto's definitions of metrics of truthlikeness are intuitively based. It could be claimed that the metrics stand on their own, and are not derived in any sense from an underlying ordering. This response would go against the spirit of a huge literature in measurement theory (for a glimpse of which see Suppes 2002, § 3.4), but it is not logically impossible, and is said to have been the preferred direction of investigation of Carnap (Niiniluoto 2001, p. 774). What it plainly loses sight of is that it is because metrics are almost always somewhat arbitrary that mathematicians became interested in topology.

The truth is that Niiniluoto's example is no more than another case of the phenomenon noted in § 1.2. With respect to each coordinate, the point b lies (weakly) between the point a and the point c. Under the transformation f the ordering of the points b and c relative to a is inverted: each coordinate of $f(c)$ lies (weakly) between the corresponding coordinates of $f(a)$ and $f(b)$. The lesson to be learnt is that pointwise superiority does not guarantee overall superiority. The lesson not to be learnt is that overall superiority need not be a topological invariant.

4.6 *'Truthlikeness is not purely topological'*

Shortly after introducing the above example, Niiniluoto makes it plain that he would not subscribe to the rejoinder just considered. He writes (1987a, pp. 472f.):

> It might be objected that our theory is too strong, since it is not necessary to introduce *quantitative* degrees of verisimilitude in order to obtain a comparative concept of truthlikeness – and the latter is our real goal....
>
> The quantitative approach would be unnecessary, if truthlikeness were only a topological concept. It was seen ... [on pp. 458f. of Niiniluoto *op. cit.*] above that this would also to help us solve Miller's problem of linguistic invariance, since purely topological notions would be preserved within translations (homeomorphisms). However, truth-*likeness* involves essentially the idea of similarity, and the explication of that concept requires something more than mere topology.

Response A retreat to a comparative theory of verisimilitude would not necessarily solve the problem of language dependence. Let X, Z, T be constituents in the language of monadic predicate logic. Let $C(X, Z)$ be the set of cells (Q-predicates) about whose occupancy X and Z differ. Then, as seen in § 1.1, the definition 'X is closer to the truth T than Z is if & only if $C(X,T) \subset C(Z,T)$', though purely order-theoretic, is not language independent.

Niiniluoto does not explain further the final sentence of the quoted passage, and it is hard to evaluate. Let me say, however, that even if there is no purely topological theory of similarity, that does not force us to adopt anything as highly structured as a quantitative or numerical theory (in which degrees of similarity are linearly ordered). The 'geometry of logic' of Miller (1984) presents axioms for a distance (that is dissimilarity) function ∂ on a Boolean algebra that can take values in any (partially) ordered abelian group G with cancellation. If x, z, t, are atoms in a Boolean algebra that represent respectively the theories X and Z, and 'the whole truth' T, then $\partial(x,t)$ and $\partial(z,t)$ may not be comparable elements of the ordered group G. The theory is not purely topological. Since G need not be a set of numbers, the theory need not be numerical. What must not be suggested is that language dependence is a price that we have to pay for a theory of similarity. This is a recurrent theme in the writings of Oddie, Kuipers, and Zwart, who hold that similarity or likeness can be explained only in terms of syntactic structure, but it does not withstand serious examination.

5 Acceptable Relativism

At the end of his on-line survey (2001) Oddie writes of the reaction outlined and criticized in § 4.3: 'Ultimately, however, this response seems less than entirely satisfactory by itself. If the choice of a conceptual space is just a matter of taste then we may be forced to embrace a radical kind of incommensurability.' In this section we discuss those reactions that accept this descent towards relativism.

5.0 *'Truthlikeness is pragmatically ambiguous'*

Niiniluoto (*op. cit.*), p. 459, acknowledges that '[t]ruthlikeness should be preserved in a translation ... *if the cognitive problem does not change* within this language shift. It is by no means clear that a *purely* semantic criterion can be given for this condition.' The implicature is that truthlikeness need not be preserved (though it may be) when the cognitive problem does change. Shortly afterwards he describes measures of truthlikeness as 'pragmatically ambiguous' (*op. cit.*, p. 469; see also 1977, p. 129). By a cognitive problem Niiniluoto means the task of identifying the one true member of a sentential partition: that is, a finite set of sentences that are pairwise incompatible and jointly exhaustive relative to background knowledge (1987a, § 4.2). The appeal to pragmatics is plainly intended to disable for good the accusation that language dependence implies a pernicious relativism.

Response What Niiniluoto calls a cognitive problem sounds to me more like a question from a multiple choice test than anything intellectually stimulating, a Kuhnian puzzle (which, to be sure, may not be easily answered) rather than a genuine problem. It would be decidedly artificial to represent the achievement of Newton's *Principia*, for example, as a cognitive problem or series of cognitive problems of this sort. In any case, as Zwart rightly observes, '[if] Miller's substitution changes the cognitive problem ... [Niiniluoto's] formal representation of the cognitive problem does not reflect this change' (*op. cit.*, § 5.4.4; emphasis suppressed). For the 15-fold partition generated by the two predicates *P* and *Q* of § 1.1 is identical with the partition generated by the predicates *P* and *R*. In other words, Niiniluoto's stance here suffers from much the same fault as that of Brink & Heidema discussed in § 4.3 above. In each case, differences that do not exist, and are fully invisible, are invented in order to legitimize differences that do exist. There is no added value.

5.1 *'Even truth is context dependent'*

Although, as far as I know, no one has publicly endorsed it, this is perhaps the best place to recognize the existence of a more spacious argument – the argument that since truth itself suffers from pragmatic ambiguity, there is no disgrace if approximate truth (and perhaps also truthlikeness) do so too. The argument is suggested by these passages from pp. 142–144 of Lecture XI of Austin (1962/1975):

> ... But consider also for a moment the question of whether truth or falsity is so very objective.
>
> Suppose that we confront 'France is hexagonal' with the facts, in this case, I suppose, with France, is it true or false? ... It is good enough for a top-ranking general, perhaps, but not for a geographer. ...
>
> ... Consider the constative, 'Lord Raglan won the battle of Alma', remembering that Alma was a soldier's battle if ever there was one and that Lord Raglan's orders were never transmitted to some of his subordinates. Did Lord Raglan then win the battle of Alma, or did he not? ... As 'France is hexagonal' is rough, so 'Lord Raglan won the battle of Alma' is exaggerated and suitable to some contexts and not to others; it would be pointless to insist on its truth or falsity.

Austin does not say here that the truth or falsity of a sentence depends on the context of utterance, or that truth is not objective. But another person might well be moved to say these things, and to conclude that the pragmatic ambiguity of truthlikeness or approximate truth has no untoward consequences.

Response Given what the present chapter is about, the answer to this concocted objection must be obvious. 'France is hexagonal' is not true, and there is indeed no finitely long sentence (except negations such as 'France is not hexagonal' and uninformative truisms such as 'France is gallomorphic') that truly describes the details of its shape. 'France is hexagonal' is false, despite Austin's misgivings. But if approximate truth makes good sense, it may be approximately true (or it may, like 'Lord Raglan won the battle of Alma', be some appreciable distance from the truth). Top-ranking generals

and geographers may well disagree about the degree of approximation that is acceptable for their purposes, but that does not mean that they are compelled to disagree about how close to the truth 'France is hexagonal' is. The pragmatic element is there, but not in a place where it is offensive to objectivists.

The objection indeed rebounds on anyone inclined to press it in defence of the pragmatic ambiguity of approximate truth and truthlikeness. For if approximate truth makes good sense, then truth and falsity cannot seriously be thought to be context dependent. By contraposition, therefore, ...

It may follow from these considerations that most of the factual sentences that interest us are false. That does not seem outrageous. (See also Chapter 9, § 3, above.) The negative tone of this chapter should not be allowed to disguise the fact that I too want to be able to talk sensibly about approximate truth.

For the sake of completeness I ought to mention here that in his (1987b) and at several (indexed) places in his (1987a) Niiniluoto tackles the problem of defining verisimilitude in languages where the truth is indefinite, so that, for example, some sentences are 'too sharp to be true' (1987b, p. 189, emphasis suppressed). He does not, however, sign up to the doctrine of context dependence of truth that we have considered in this section.

5.2 *'Occasional reversals do not matter'*

Smith concludes his discussion (*op. cit.*, pp. 88f.) with these words:

> So the only case we would really have to worry about is the case where transformations which were not time-dependent in an anomalous way took us from one space to another, reversing the fortunes of some hypotheses, but where the state variables of both spaces had equal claim to physical interest.
>
> I know of no such case: but if there were one, we could just live with it – the relevant hypotheses **X** and **Z** would, as it were, score one goal each, so the match would be a score draw, and neither preferable. An account of approximate truth doesn't have to rule out such cases.

The implicature of the last paragraph here, and of Smith's entire discussion, is that such undecidable cases are bound to be exceptional. A serious theory of approximate truth, he hints, need not concern itself too much with such uncongenial artificialities, and is absolved from further investigation.

Response What is troublesome here is not any argument submitted by Smith, but a disappointing lack of argument.

It was because I too thought that the occasional instance of incomparability amongst numerical theories would not be devastating to an account of approximate truth that I attempted to establish that the order in which two sufficiently different theories are ordered by accuracy can always be reversed.

For if incomparable theories are the norm, an account that says nothing about them is at best incomplete, and perhaps (like Popper's original theory) of only limited value.

Smith does not attack directly the thesis of my (1975) and (1994), Chapter 11, and (as I have noted) ignores the two proofs of it that I offered (§ 1.2 above). Instead of trying to demonstrate that there exists even a single case where no admissible transformation can affect the order in which two theories are ranked by accuracy, he raises objections to particular aspects of a particular example that I presented as an illustration of my thesis (see § 3.2 above). The objections, I have argued above, lack bite. But even if Smith is correct that time dependence is a blemish, and that some well-defined quantities can be disqualified as not physically significant, he has hardly begun to restore respectability to his theory of approximate truth. The inelegance of Table 11.2 is admitted, but that does not, after all, show that there may not be some time independent transformation that does the trick, not only in this case but in every similar case. My proof in effect employed only transformations that are weighted averages of $\varphi(t)$ and $\psi(t)$, but there are innumerably many other possibilities.

The failure of the example in Table 11.2, if indeed it does fail, to construct bona fide physical quantities also tells us little about what happens in the general case. Mormann (1988), p. 516, affords a nice example of two interdefinable pairs of physical quantities that under the most stringent interpretation of significance must count as significant. Let $\varphi(t)$ be the square of the momentum at time t of a particle of mass m in free fall, and $\psi(t)$ its distance above the ground. (Once more I bring the notation into accord with that of this chapter.) We may define the Hamiltonian $\eta(t)$ and the Lagrangian $\xi(t)$ by the equations $\eta = \varphi/2m + mg\psi$ and $\xi = \varphi/2m - mg\psi$, where g is the acceleration due to gravity. The inverse definitions $\varphi = m(\eta + \xi)$ and $\psi = (\eta + \xi)/2mg$ are immediate. Note that these transformations are all time independent in the sense that Smith demands. Of course this example too does not tell us much about what happens in the general case. Unfortunately it is not quite the 'important step forward' that Mormann believes it to be. For it can be shown that, even if 'η and ξ give the basis of a new metric... [that] is *not* equivalent to the metric defined by φ and ψ' (*loc. cit.*), this particular transformation can never completely reverse in the required way the ordering by accuracy of two theories giving values for φ and ψ.

Choose units so that $m = 1/2$ and $g = 2$. Then the transformation we are interested in is: $\eta = \varphi + \psi, \xi = \varphi - \psi$. It will be shown that if both the values φ_X, ψ_X of φ and ψ given by **X** lie strictly between the true values φ_T, ψ_T and the values φ_Z, ψ_Z given by **Z**, then it is not the case that both the values η_Z, ξ_Z given by **Z** lie strictly between the true values η_T and ξ_T and the values η_X, ξ_X given by **X**.

There is no loss of generality in supposing that the values given to the four quantities by the three theories are as laid out in Table 11.3, where each of *a, b, c, d, e, f* is positive. If necessary we can multiply all entries by -1 to ensure that the

Table 11.3 Mormann's transformation

	φ	ψ	$\eta = \varphi + \psi$	$\xi = \varphi - \psi$
Z	a	$d+e+f$	$a+d+e+f$	$a-d-e-f$
X	$a+b$	$d+e$	$a+b+d+e$	$a+b-d-e$
T	$a+b+c$	d	$a+b+c+d$	$a+b+c-d$

values of φ decrease as we move from **T** to **X** to **Z**, in which case the values of ψ must increase; otherwise there can be no reversal of order for $\eta = \varphi + \psi$. For much the same reason, namely that $\varphi = (\eta + \xi)/2$, the values of η increase as we move from **T** to **Z** to **X** if & only if the values of ξ decrease. A simple calculation now shows that if $\eta_T < \eta_Z < \eta_X$ then $b + c < e + f$ and $f < b$, whilst if $\xi_T > \xi_Z > \xi_X$ then $b + c > -(e+f)$ and $-f > b$. That is to say, $-f > f$, which is impossible if f is positive. The inequalities are all reversed if the values η and ξ are otherwise ordered. In this case we may conclude that $-(e+f) > e+f$, which is equally impossible.

In short, Smith's discussion focuses too much on specific cases and takes insufficient account of the wealth of alternative possibilities. To indicate what may be lost, I conclude with an example (illustrated in Figure 11.1), an example that is evidently open to considerable generalization, in which a transition from Cartesian to polar coordinates in the plane reverses the order of accuracy of the theories **X** and **Z**. Recall that angles are measured counterclockwise from the positive x-axis, so that the polar angle $\vartheta = \tan^{-1}(y/x)$, while the radius vector $r = \sqrt{(x^2 + y^2)}$. The values of the Cartesian coordinates x and y, and their polar equivalents r and ϑ (in radians) are given in Table 11.4. This transformation is not time dependent, and it would be far fetched to consider it far fetched.

My complaint is not that Smith does not acknowledge the possibility of genuine reversal of accuracy, for he does acknowledge it (*op. cit.*, p. 89). What is discouraging is that he makes no effort to assess its scope. Parrying

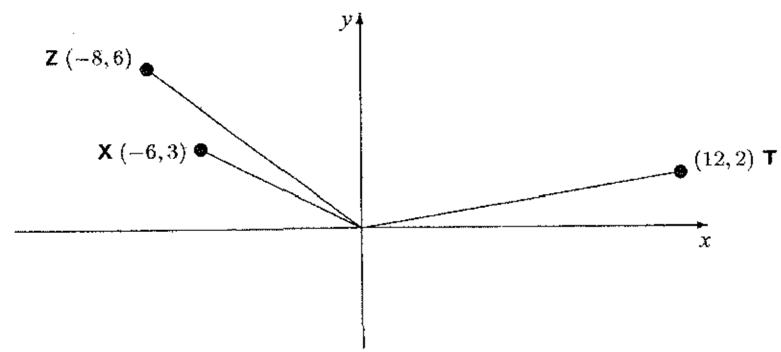

Figure 11.1 Transition from Cartesian to polar coordinates

Table 11.4 Transition from Cartesian to polar coordinates

	x	y	r	ϑ
Z	−8	6	10.00	2.50
X	−6	3	6.71	2.68
T	12	2	12.17	1.03

particular counterexamples is a poor way of defending a theory potentially under attack on an unlimited number of fronts. In Chapter 3, § 3, above I quoted the contrast Popper used to make between philosophical criticism, which 'aims at showing the invalidity of some arguments which have been offered in justification of the claim that a certain theory is true' and scientific criticism, which is 'an attack upon the theory itself – on its content or its consequences' (1967c, p. 24). We have an example here of philosophical criticism. Even when most kindly looked on, it fails to show that the sceptical thesis of my (1975) is false.

5.3 *'Truthlikeness is harmlessly relative after all'*

Zwart agrees with Niiniluoto that a change of primitives, such as that involved in the move from the *h-r-w* language to the *h-m-a* language, may indicate a change of cognitive problem or cognitive interest. In explanation, he offers the suggestion that 'in truthlikeness definitions the non-logical vocabulary has two totally different functions' (*op. cit.*, § 5.4.3; here and elsewhere in this paragraph emphases in Zwart's text have been suppressed in quotation). One function, called 'the descriptive function' at § 5.5.1.2c, is the obvious one of making possible the formulation of theories (Zwart describes it as 'the identification of the various possible worlds or constituents'). The other function 'consists of providing the set of traits according to which the constituents are ordered' (*loc. cit.*). This is not an entirely new idea. Much the same double duty is attributed by Wilson (*op. cit.*, p. 83), to

> a traditional picture of universals, such as Russell's, [in which] 'concepts' play two roles simultaneously: (i) they mark distinctions that actually differentiate the objects of the external world; (ii) they mark different conceptions or attitudes that *we* entertain as we attempt to decipher the structure of the distinctions under (i).

Zwart goes on (*loc. cit.*):

> The hidden variable of the language dependency debate, therefore, is the set of traits that underpins the order of the possible worlds. . . . this analysis avoids metaphysical . . . essentialism, which is typical of the privileged language argument [§ 3.0 above]. It is our cognitive interest that establishes the traits of our concern, but . . . they need not be more fundamental aspects of reality than other properties.

Zwart proposes as an example the pair of predicates My ('y is male') and Cy ('y is colour-blind'), and observes that if the domain is limited to persons with colour-blind progeny, then My is extensionally equivalent to Yy ('y has a Y chromosome') and Cy is equivalent to $Yy \leftrightarrow Uy$, where Uy is the predicate, 'y has exactly one colour-blindness allele' (*op. cit.*, § 5.4.2; again the notation is adjusted). This leads him to propose that genotypical questions (about sex or colour-blindness) belong to different cognitive problem situations from phenotypical questions (about possession of certain chromosomes and alleles), 'even if the terms of the first are definable in terms of the second' (p. 184). The cognitive problems being different, it is harmless (even welcome) that similarity orderings differ too.

Response There can be little doubt that when we are faced with a real problem, rather than a cut and dried investigation of the kind here considered, we do not restrict ourselves to using the vocabulary in which the problem is initially formulated. I know of no satisfactory answer to either 'Why is the sky blue when the sun shines?' or 'Why is the sky dark at night?' that admits any entity to which the term 'the sky' refers. Our interests may expand when we stop talking about the sky and attend to the dispersion of light or the distribution of matter in the universe, but they are not fundamentally disrupted. That is to say, a change of language need not herald a change of problem, but only an attempt to solve it. On this matter I agree wholeheartedly with Wilson, who writes that 'the art of gaining control over a physical problem often reduces to finding new quantities far removed from the set of parameters from which we have begun, quantities that might not even be definable, in the usual logical sense, in terms of the original set' (*op. cit.*, p. 90). If radically new properties and quantities may be needed to solve an old problem, it is perverse to interpret a change of terminology to a definitionally equivalent one as an indication that the problem being investigated, or our interest, has altered.

Here are two more examples, one from everyday life, the other from the quantitative theatre, in which there is a change in the salient sentences and functions to be evaluated, but no discernible change in the cognitive problem that is being addressed.

a *Follow my leader*: Two strategies suggest themselves if there is a particular path that we wish everyone in a party to follow, or a particular series of actions that we wish everyone to undertake: one strategy is to supply each individual with a detailed list of instructions; the other is to supply one person (the leader) with the instructions, and to tell the other individuals to repeat what the leader does. In the one case we arrange for the set $\{p_i\}$ to come out true, in the other case the logically equivalent set $\{p_0\} \cup \{p_0 \leftrightarrow p_i \mid 0 < i\}$. There are not two different problems here, but two different strategies for solving one problem. Note that the follow-my-leader strategy may be disastrous if the leader makes a mistake.

b *Measuring a rectangular surface*: The agent wishes to paint a rectangular surface such as the floor of a large hall, and to tile its edge. He therefore needs to know the hall's perimeter and its area. These are not easily measured, and instead he arranges to have the length and width of the hall measured as accurately as possible by two surveyors **X** and **Z**. The results are given in the first two columns of Table 11.5. Recall that the perimeter of the hall is twice the length plus twice the width, whilst the area is their product. (The length and width are the two solutions of a quadratic equation particular to the perimeter and the area.) Which surveyor does a better job? It is stretching the imagination too much to maintain, as Zwart presumably must maintain, that for the cognitive problem of finding values for the length and the width, **X** is to be preferred, but for the quite different cognitive problem that really concerns the agent, **Z** is to be preferred, and that these two preferences are in harmony.

Table 11.5 Measuring a rectangular surface

	Length	Width	Perimeter	Area
Z	31 m	27 m	116 m	837 m^2
X	33 m	26 m	118 m	858 m^2
T	34 m	23 m	114 m	782 m^2

5.4 'A very down-to-earth phenomenon'

Zwart's position is that 'the substitution argument is an instance of a very down-to-earth phenomenon: the similarity order of objects... depends on the choice of the properties according to which the objects are ordered' (*op. cit.*, p. 185). That 'there is no absolute similarity' (p. 188, emphasis suppressed) is indeed a familiar refrain. Popper (1959), Appendix *x, (1), writes 'Two things which are similar are always similar *in certain respects*' and 'similarity always presuppose[s] a point of view'. Similar sentiments have been expressed by Hilpinen (*op. cit.*), § III, who writes that 'the degree of similarity between possible worlds (or possible situations) depends on the basis of comparison', by Niiniluoto (1987a), p. 129, who says much the same, by Mott (*op. cit.*), p. 250, and by many others. Forster (2004), pp. 10–12, compares truthlikeness to intelligence, something that can be measured only on a multidimensional scale. Note that if the properties forming 'the basis of comparison' are bivalent predicates, as they are implicitly taken to be in § 1.0 and § 1.1 above, then it is better to say that similar things are identical (rather than similar) 'in certain respects'. But as Goodman (1970/1972), pp. 443f., Watanabé (1965), § 3, and others have noted, from a formal standpoint any two distinct objects, however unlike intuitively, are identical in the same number of bivalent respects; that is, they share as many bivalent predicates as do any other two distinct objects. This result is clearly related to the theorem mentioned in Chapter 4, Addendum 1.

Response The incontestable fact that objects similar with respect to one property may be dissimilar with respect to logically independent properties – people of like age may be unlike in hair colour, height, wealth, and so on – cannot provide any encouragement for the fiction that similarity with respect to some properties is compatible with dissimilarity with respect to properties with which they are interdefinable. Zwart, like other writers (such as Smith 1998b, as mentioned in § 3.2 above) can fairly be accused of sliding silently from the first fact to the second fiction. In the present discussion the matter is made a little more complicated, but not essentially changed, by the fact that in all the examples a pair of logically independent properties are considered together, and set at odds with a pair of other properties, also mutually independent, with which as a pair (but not individually) they are interdefinable.

In Zwart's genetical example it is undisputed that the predicates My and Cy are logically independent of each other, as are the predicates Yy (which is equivalent to My) and Uy. Indeed, each of the three predicates My, Cy, Uy is logically independent of each of the other two taken individually. But, given modern genetics, none of them is independent of the other two taken together (in suitable domains). The theory imposes logical equivalence (and not mere 'extensional equivalence', as Zwart *op. cit.* persistently describes it) on My and Yy and on Cy and $Yy \leftrightarrow Uy$. In other words, the theory identifies the phenotypical questions and the genotypical questions, and voids of defence the suggestion that they are associated with impressively different cognitive problems. Of course, in the absence of any theory of genetics, questions about sex and eyesight may seem radically independent of questions about cell components. Yet a geneticist interested in maleness and colour-blindness and in any claims that they are located in specific individuals is obliged to be interested, objectively if not consciously, also in the properties Yy and Uy. Nothing remotely like this can be said about properties that are logically independent of My and Cy (such as the presence and absence of other alleles).

The original argument in its various versions concerned vocabulary that is explicitly interdefinable with the original vocabulary, not just extensionally equivalent to it. I admit that some of the examples given in this chapter, for example 'appropriately dressed' and 'wearing warm clothing if & only if there is an "r" in the month' in § 3.1, are not logically equivalent or synonymous within any interesting theory. But they are only illustrative examples.

6 Conclusion

The poverty of what Oddie likes to call the 'likeness program for truthlikeness' (1986b) cannot any longer be disguised. The project to distil comparisons and even measures of truthlikeness from syntactic structure must be abandoned, despite there not being a great deal to put in its place. Yet the prospects are not quite blank. As noted in § 0, there exist ways of explaining truthlikeness and approximation to truth that are unaffected by the bane of language dependence.

The threat of subjectivism and relativism still lurks nonetheless. For surely similarity does depend on interests, in the sense that two items may be similar in one respect and dissimilar in a logically independent respect. The outstanding problem then seems to be the problem of how different respects are to be objectively weighed or aggregated, how overall similarity is to be assessed. Williamson (1981, Chapter III, p. 55) quite reasonably asks in what respect respects, as here understood, deserve such respect; that is, how it is that similarity-in-a-respect earns an objectivity medal, whilst overall similarity or resemblance is reduced to the ranks of the indeterminate or idiosyncratic.

> But it is doubtful whether any respect is too simple to be decomposed into others. Resemblances in respect of shape include resemblances in respect of symmetry and resemblance in respect of the number of sides; resemblance in respect of symmetry includes resemblance in respect of the number of symmetries and resemblance in respect of the symmetries between the symmetries; resemblance in respect of the number of sides includes resemblance in respect of the number of different prime factors of the number of sides and resemblance in respect of the number of different powers to which those factors were raised – and so on, indefinitely.... It would seem better to make *no* resemblances objective...

Williamson is not here advocating submission to full-scale subjectivism. On the contrary, his point is that if subjectivism can be avoided at the level of individual respects, it should be avoidable also at the aggregative level. I wish that I could agree with him. But what the argument shows, it seems to me, is something decidedly different, namely that resemblance, so understood, is too luxurious a relation for our purposes. That $b = 2^3 \times 5^4 \times 7^2 = 245000$ resembles $a = 2^5 \times 3^1 \times 5^5 = 300000$ in magnitude more than $c = 2^3 \times 11^4 = 117128$ does would normally be supposed to mean only that b lies between a and c in numerical order. If it is supposed to say also something about how much the number of prime factors of a (three) resembles the number of prime factors in each of b and c (three and two respectively), or about how much the number of exponents in the factorization of a (two) resembles the numbers of exponents in the factorizations of b and c (three and two respectively), then it is not the relation that we need. The number b is objectively closer to the number a than c is. Yet once we attempt to aggregate two or more such comparisons we are left floundering, as we have seen. More generally, not all resemblances are aggregations of other resemblances; they may (as in the case of bivalent predicates mentioned in § 5.4 above) look more like aggregations of identities and differences. To such aggregations Williamson's argument does not apply.

Lewis diagnoses indeterminacy and vagueness, rather than subjectivism, at all levels. He writes (1973, p. 91): 'Overall similarity consists of innumerable similarities and differences in innumerable respects of comparison, balanced against each other according to the relative importances we attach to those respects of comparison. Insofar as these relative importances differ from one person to another, or differ from one occasion to another,... so far is comparative similarity indeterminate.' Later he underwrites his objectivist position with the claim that '[t]here is a rough consensus about the importance of respects of comparison, and hence about comparative similarity' (*op. cit.*, pp. 93f.). As Mill remarked, '[r]esemblance between two phenomena is more intelligible in itself than any explanation could make it' (1843, Book I, Chapter V, § 6; quoted by Williamson *op. cit.*, p. 52).

The present chapter has tried to make it obvious that even a rough consensus should be an object of deep suspicion. How important one respect is judged to be inevitably depends on which other respects it is combined with. Whether the weather is minnesotan m or not may seem quite unimportant when considered singly, but if heat h is another characteristic considered, then $m(= h \leftrightarrow r)$ becomes as important as raininess r is. Yet we cannot allow both r and m to contribute to the overall aggregates, if h contributes. I suggest that, despite what may seem obvious, overall similarity or superiority cannot coherently be understood as an objective aggregate of more specialized similarities and agreements. It is a holistic property, like beauty perhaps. What makes an object or a person beautiful is as much concerned with proportion and balance as it is concerned with beautiful features.

This allows me to share Lewis's view that our judgements of truthlikeness and approximation to the truth are ineluctably vague. It does not follow that they are not objective, any more than vagueness elsewhere is a seedbed of subjectivity. Vagueness stems from ignorance about what is objectively the case (Miller 1980c; Williamson 1994, Chapter 7), and we can make an effort to learn more. But the degree of vagueness here is sufficient to make our judgements untestable, and to the extent that testability remains one of our original desiderata (see Chapter 10, § 2, above), it has to be conceded that only a fragmentary theory of verisimilitude and approximation to the truth is at present available.