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BETH'S THEOREM AND REDUCTIONISM

BY

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MATHEMATICALLY and logically minded philosophers have assessed the philosophical consequences of several important theorems in metalogic.* This is no surprise, given logic's schematic concern with the fundamentals of metaphysics and meaning: with notions such as object, identity, existence, extension, denotation, satisfaction, truth, logical consequence, concepts and their definitions, the expressive power of language, the deductive power of systems of inference, and the quest for certainty in axiomatic foundations.

Essentially logical deathblows have (according to some) been dealt by *Russell's paradox* to the logicist conception of class; by *Gödel's incompleteness theorems* to the logicist conception of number, Hilbert's finitary justification of mathematics, and the mechanist view of mind; by *Craig's theorem on axiomatizability* to the realist view of theoretical entities; by the *downward Löwenheim-Skolem theorem* to the effability of the uncountable; by *Tarski's theorem on truth* to the prospect of an all- (and self-) encompassing language; by the *compactness theorem* to the expressive success even of complete theories. Recent work in "soft model theory" has extended even further our understanding of inherent limitations on the expressive and deductive powers of mathematically well-defined languages. A central theorem in this generalized setting whose philosophical consequences have enjoyed the pioneering attention of but a few philosophers is that of Beth [1953]. *Beth's definability theorem* for a language L states that any predicate Q implicitly defined in terms of $P_1, \ldots, P_n$ relative to a theory *T* in L can be explicitly defined, relative to *T*, in terms of $P_1, \ldots, P_n$.

In §1 I shall explain the content of this theorem in some detail and explain how the theorem is relevant to the problem of *scientific reductionism*. §2

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provides a broader and more up-to-date account of Bethhood in a wide class of languages relevant to the scientific enterprise in general; and draws attention to the major features of the theorem that are relevant to the task of philosophical interpretation. This sets the stage for my main task in §4, which is *to counter the reductionists' invocation of Beth*. But I shall do this only after §3, where I criticize as inadequate the obstacles that anti-reductionists have sought to place in the reductionists' argumentative path. Having cleared the path in §3, I shall in §4 erect a wall right across it. I shall show why one can be an explanatory anti-reductionist while yet being a physical determinationist. I shall give logical reasons for regarding higher levels of explanation as autonomous, regardless of how thorough (in their own terms) one believes explanations at lower levels to be.

§1 Beth's theorem states that if a new concept Q can be defined *implicitly* by means of a theory *T* using concepts $P_1, \ldots, P_n$, then it can be defined *explicitly* in terms of $P_1, \ldots, P_n$ relative to *T*. Explanation of these notions is required. First, *implicit definability:*

> Suppose one understands the concepts $P_1, \ldots, P_n$. One develops a theory using them, and one can identify what count as P_i's in the domain to which the theory addresses itself. Suppose further that a new concept Q is imported, and a grasp of it conveyed by means of a set *T* of statements involving *both* Q *and* $P_1, \ldots, P_n$. Suppose finally—on the assumption that *T* is a true account of what is the case in the domain, and that you have settled what, in the domain, count as P_i's—that there turns out to be *but one* way of understanding what Q applies to. Then we say that we have *implicitly* defined Q in terms of $P_1, \ldots, P_n$ relative to *T*.

Secondly, *explicit definability:*

> Suppose there is a complex concept R built up from $P_1, \ldots, P_n$ but *not* involving Q and that it follows as a logical consequence of *T* that R and Q apply to exactly the same things. Then we say that R provides an *explicit* definition of Q relative to the theory *T*.

Beth's theorem, to repeat, states that if Q is *implicitly* defined in terms of $P_1, \ldots, P_n$ relative to *T* then there is some such R that *explicitly* defines Q in terms of $P_1, \ldots, P_n$ relative to *T*.

Why is it important in connect with the problem of scientific reductionism? To understand why, we have to separate two problems with which the philosopher of science is engaged:

> the problem of whether there is an ultimate level of reality (such as that described by fundamental particle physics) which—in a sense to be clarified—*determines* all other levels;

> the problem of whether higher level theories, such as biology, psychology and sociology, could, in principle, be *reduced* to a chosen lower level theory such as fundamental particle physics.

The first problem is known as the problem of physical determinationism; the second one is known as the problem of reductionism. Affirmative answers to each I shall call, respectively, the *thesis of physical determinationism* and the *thesis of (explanatory) reductionism*. In so doing I follow Hellman and Thompson [1975]. Reductionism is meant in the classic sense deriving from the work of earlier philosophers of science such as Nagel [1961]. Briefly, a theory T′ *reduces to* a theory T just in case the predicates of T′ can be defined by means of formulae in the language of T so as to permit within the theory T the derivation (upon substitutions using those definitions) of all the laws of T′. Note that the defining formulae are not maintained to be synonymous with, or analytical of, the "primitive" predicates of the higher level that they are intended to replace. All that is maintained is their (lawlike) *coextensiveness* with the latter.

Hellman and Thompson take the credit for explicating the thesis of physical determinationism in model theoretic terms. Briefly, the class of L-expressible facts determines the class of L′-expressible facts in all structures of a given class C just in case any two structures in C that are L-equivalent are L′-equivalent. (When the facts are sentential truths, equivalence is to be construed as elementary equivalence; when the facts are extensions of predicates, equivalence is to be construed as isomorphism.) The most interesting and natural choice of C, of course, is the class of all models of the union of the theories T and T′; or—perhaps more realistically—of whatever (L,L′)-theory one holds. (The latter can go beyond a mere union of T and T′ by containing bridge laws involving terms from both L and L′.) Note that determinationism, as thus defined, has absolutely nothing to do with determinism as a metaphysical thesis about the fixity of the future. Beth's theorem on definability is the result in mathematical logic that bears most intimately on the connection between these two problems. As explained above, it has the form of an implication

if A then B;

in which, as we shall now see, A could be interpreted as the thesis of physical determinationism and B could be interpreted as the claim that all theories are reducible to physics.

(It is worth noting that all I have to say would apply, in principle, to any other "higher order" science whose relationship to physics is similarly in question. I share, for purposes of exposition, the prevailing assumption among modern scientists that if any theory has a claim to be describing the determining level, it is first and foremost physics. Curiously enough, the logical investigations to be described would apply even if the determination were—outrageously—the other way round: even if, say, a theory of social individuals and group minds were taken as describing the determining level, on the bizarre metaphysical conviction that the "level of reality" whereof it spoke—namely collective unconsciousness, interanimation etc.—determined what was the case even at the level of fundamental particles.)

I claimed above that the antecedent of Beth's theorem is tantamount to the thesis of physical determinationism; while its consequent is tantamount to the thesis of reductionism. How is this so?

To see this, note first that in the antecedent A one is speaking of *implicitly* defining a concept Q in terms of other concepts $P_1, \ldots, P_n$ relative to a theory *T*. It is useful to think of *T* as the *full story* concerning *both* levels in question; *T* will therefore always include at least the *union* of the theories T and T′ mentioned in the foregoing, along with further bridge laws combining both higher level and lower level vocabulary, should such laws not appear in either T or T′. Think of Q as a "higher level" concept—a psychological predicate, say—and take the concepts $P_1, \ldots, P_n$ as physical or physiological predicates of the "lower level." Now the theory *T* involves all of them in various of its laws, and in such a way as to "pin down" the interpretation of Q once given interpretations of the P_i's. No doubt certain bridge laws—laws that involve Q and at least some of the P_i—will contribute crucially to *T*'s success in doing this. (It is worth noting that an overall theory *T* can contain bridge laws *without* yet succeeding in defining extensions of higher level predicates implicitly in terms of lower level predicates. Bridge laws are necessary, but not sufficient to secure that.) The antecedent of Beth's theorem, on this account, is tantamount to the thesis of physical determination. For, each of the predicates Q of the "higher level" will, according to that thesis, be "pinned down" (given the overall theory *T*) by the "lower level" predicates P_i. There will be only finitely many of these predicates Q to deal with. Thus the thesis of physical determination translates (taking each Q on its own) into so many repetitions of the antecedent of Beth's theorem—one repetition for each predicate Q concerned.

Secondly, note that the consequent B of Beth's theorem, according to which each such Q is *explicitly* definable (relative to *T*) in terms only of the "lower level" $P_1, \ldots, P_n$, provides for precisely such definitions as are needed in order to *reduce* any theory involving the Q's to a theory involving only the P_i's. Thus the consequent of Beth's theorem is tantamount to the thesis of reductionism.

Beth's theorem is therefore of special significance for the "higher level" scientist—the biologist or psychologist, say—who wishes to maintain *logically* privileged autonomy for his discipline. If the interpretation that I have intimated of Beth's Theorem can be sustained, then such a scientist would have to accept the consequence that his discipline is, *in principle,* reducible to physics. (Of course, whether or not it is reducible *in practice* is quite another question. Perhaps the *practical* impossibility of such reduction—despite its logical possibility—is sufficient to ensure him his theoretical autonomy. I shall say more on this in §4 below.)

Beth's theorem, then, according to the determinationist, *implies* reductionism. It appears to render uninhabitable the intellectual niche sought by the physical determinationist who, prompted perhaps by considerations of holism and emergent properties, is a would-be anti-reductionist. It threatens to demolish the philosophical position most likely to be adopted by biologists and psychologists anxious to preserve their disciplines from the reductive encroachment of molecular biology, neurogenetics etc. In vain would an evolutionary biologist such as Mayr (in his [1982]) be able to protest that

> The claim that genetics has been reduced to chemistry after the discovery of the structure of DNA, RNA, and certain enzymes cannot be justified. To be sure, the chemical nature of a number of black boxes in the classical genetic theory was filled in, but this did not affect in any way the nature of the theory of transmission genetics. As gratifying as it is to be able to supplement the classical genetic theory by a chemical analysis, this does not in the least reduce genetics to chemistry. The essential concepts of genetics, like gene, genotype, mutation, diploidy, heterozygosity, segregation, recombination, and so on, are not chemical concepts at all and one would look for them in vain in a textbook on chemistry.

For Mayr has come nowhere near establishing the *logical* impossibility of achieving a chemical definitional reduction of the notions of genetics. Just because they are not to be found in any extant textbook on chemistry does not show that it is not, in principle, possible to devise such a reduction. Beth's theorem would ensure that the definitional reducing formulae exist. They might be fiendishly complex: but they would be there.

§2 So far I have been discussing Beth's theorem for a classical first order language. Its quantifiers are just the existential and the universal. It has only the standard connectives. Its sentences are finitely long.

We know a great deal about this language. It has a complete proof system. Its relation of logical consequence is compact. If a theory in it has a model of any infinite cardinality, then it has models of all infinite cardinalities. It is also unusual for an interesting theory with a countably infinite model to have no other (that is, non-isomorphic) countably infinite models. (I say "interesting" because by a result of Ehrenfeucht [1972] there are continuum many countably categorical theories; but I suspect that few of these will be of mathematical or physical interest.)

Is this the best language for science? For one who thinks not, there are several ways in which he can improve or extend the workings of the language. First, one could add new quantifiers. Examples are "There are at least infinitely many x," "There are uncountably many x," "There are at least as many x as there are things in the domain," and "There are at least ω_α many x." If L is extended by adding a quantifier Q the result is known as L(Q). Secondly, one could add modal operators such as "It is necessary that" and "It is possible that." Thirdly, one could allow sentences that are infinitely long. There are two ways to do this. One could allow disjunctions and conjunctions to have ordinal length less than some uncountable κ; and allow quantifier prefixes to have length less than λ. The resulting language is known as $L_{\kappa\lambda}$.

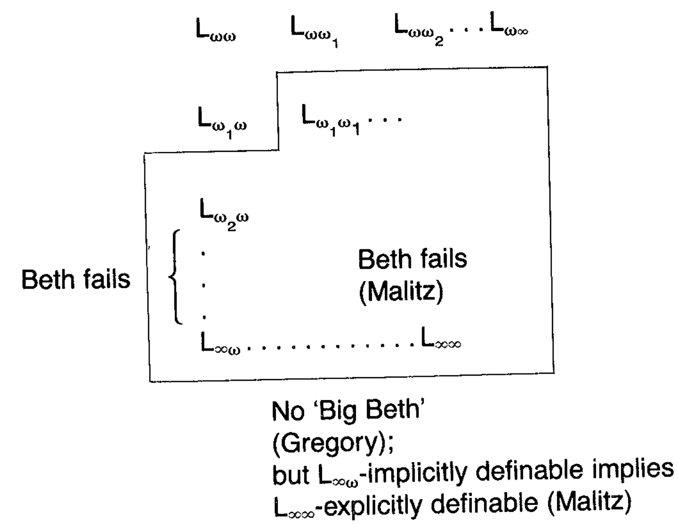
These extensions of the language could be motivated in different ways. First, one might wish to argue that the need to characterize the exact structure of space-time calls for the use of infinitary quantifiers such as "There are uncountably many x such that." Secondly, one might wish to argue that science discloses metaphysical essences of naturally occurring substances, and should be able to distinguish accidental from essential properties by using modal operators. Thirdly, one might wish to argue that our own finitary capacities should not prevent us from adopting an infinitary language. For it may be the ideal means of expressing facts which, by their very infinitary nature, call for infinitary "encapsulation" in a language whose sentences are supposed to "correspond with" those facts. These are just hints as to how certain arguments might be developed, and it is not my concern to pursue them here. There are enough to alert us to the need to approach the problem of reductionism in science with a broader awareness of Bethhood across all the suggested languages. To such a survey I now turn.

For languages with generalized quantifiers, results of Friedman [1973] and Yasuhara [1966] give the following picture:

> If ω is regular, Beth fails for L ("There are at least ω_α many") without identity;
> Beth fails for L ("There are domain-many");
> Beth fails for all L ("There are ω_α many")

For the standardly quantified modal system S5, Fine [1979] showed that Beth fails.

For infinitary languages, results of Gregory [1974] and Malitz [1971] give the following picture:



Bethhood thus appears to be the exception rather than the rule. We must therefore be sensitive to the choice of L when drawing philosophical consequences from the statement of Beth:

In the language L:
if [$T(P_1 \ldots P_n,Q)$ implicitly defines Q]
then [$T(P_1 \ldots P_n,Q)$ explicitly defines Q]

I wish now to draw attention to some other features besides choice of L which must be taken into account. To do so, I shall forage in the definitions of implicit and explicit definability just far enough. First, the statement of implicit definability in the square brackets above becomes:

$$T(P_1 \ldots P_n,Q),\ T(P_1 \ldots P_n,Q') \models \forall\bar{x}(Q\bar{x} \equiv Q'\bar{x})$$

where the substitution convention is obvious. This says that if we regard Q as uninterpreted, and consider replacing it throughout by another uninterpreted predicate Q′, both predicates are forced, by what the theory *T* says about them in relation to $P_1, \ldots, P_n$, to be coextensive in any model of the theory union to the left of the turnstile above. Put another way, the extension of Q is uniquely determined (by some Skolem function f) by the extensions of $P_1, \ldots, P_n$:

For all models M of $T(P_1 \ldots P_n,Q)$:
$ext_M(Q) = f(ext_M(P_1), \ldots, ext_M(P_n))$

It is with this form of the statement of implicit definability that I shall subsequently be engaged, focusing on the importance of the underlined quantifier prefix 'for all models M'.

Secondly, the statement of explicit definability in the square brackets above becomes:

There is some formula $R[P_1 \ldots P_n](x)$ such that:
$T(P_1 \ldots P_n,Q) \models \forall\bar{x}(R[P_1 \ldots P_n](\bar{x}) \equiv Q(\bar{x}))$

It is with this form of the statement of explicit definability that I shall be subsequently engaged, focusing on the importance of the underlined quantifier prefix "there is some formula $R[P_1 \ldots P_n]$."

We have so far noted three important points of emphasis in the statement of Beth's theorem: the language L, the range of models M, and the existence of a defining formula R. My fourth and final point of emphasis is that throughout the statement of the theorem there is a pervasive dependence on the actual *theory T* that marshals the definiendum Q and the predicates $P_1, \ldots, P_n$ available as raw material for the definiens R. Both kinds of definition—implicit and explicit—are relative to, or *modulo T.*

Having assembled my quartet of considerations for future application, I turn now to what other philosophers have made of Beth's theorem in relation to the problem of reductionism.

§3 As in all philosophical disputes, there are voices for and against: in this case, one voice for and one voice against. They sounded in the literature independently of each other; they have not engaged each other's arguments.

Bealer [1978] applies Beth's theorem with reductionist effect. He examines leading statements of the functionalist theory of mind to tease out formally statable conditions of adequacy on such a theory. He then shows that those conditions are tantamount to the claim that mentalistic notions are implicitly defined by other (physical and behavioural) notions. Finally he brings in Beth's theorem to show that "there exist provably adequate functional definitions of mental predicates if and only if there also exist provably adequate ordinary explicit definitions" (*loc.cit.* p. 359).

Hellman and Thompson (op.cit.) take another line. They are concerned more generally with the determination of any higher level by a lower level, so the locus of Bealer's application is certainly within their purview. But they are less willing to regard the starting point of scientific materialism as tantamount to the antecedent of Beth's theorem—that is, as tantamount to the claim that the higher level notions are implicitly defined in terms of physical ones. Their argument rests on a single observation, which might strike some as *ad hoc*. It is that in the statement of implicit definability (the version I settled on in §2 above) the quantification must be construed literally as over *all* models of the theory *T*. This is extremely important from the (meta)logical point of view, in order to secure the implicit definability of Q. Now among those models will be ones in which the natural numbers form a non-standard progression. Indeed, by a result of Ehrenfeucht and Mostowski [1956], there will be continuum many such models, even if we consider only countable ones. But according to Hellman and Thompson the scientific materialist loses nothing by way of either content or justification for his claim that the physical facts determine all the facts, *if he forswears the use or consideration of models in which the natural numbers form a non-standard progression*. The view advanced is rather that the scientific materialist is, like any other theorist, interested in coming as close as possible to the truth, the whole truth and nothing but the truth: in witness whereof he will admit only the standard natural numbers into his model of the world.

The intention is admirable, and indeed I have considerable sympathy for such a view. I believe that there *are* good reasons for refusing to admit non-standard models, but they are not available in the realist framework within which Hellman and Thompson confine themselves. Countable categoricity of natural number theory is a terminus on a long line of anti-realist argument which they are nowhere near broaching. It would be a digression here to develop the argument in question; I wish only to note its existence, and make the point that there are easier ways for Hellman and Thompson to block the application of Beth's theorem. They succeed by collapsing the scrum and smothering the ball. They subscribe to physical determinationism, but only in the form

For all *standard* models M of $T(P_1 \ldots P_n,Q)$:
$ext_M(Q) = f(ext_M(P_1), \ldots, ext_M(P_n))$

This is strictly weaker than the proper antecedent of Beth's theorem, so the backs are starved of possession. But I intend to show that one can allow the clean heel, even against the anti-realist head, keep the ball in play and still tackle the backs before they reach the reductionist goal line.

§4 Of the four points isolated in §2, three remain: choice of language L; existence of defining formula R; and relativity to theory *T*. What I want to say about the choice of L is already implicit in my discussion above. To wit, the reductionist who wishes to justify his reductionism by appeal to Beth's theorem had better be sure that Beth's theorem holds for the language of science that he adopts. The Quinean will have no quibble with the standard "grade A" idiom for which Beth obtained his original result. But others might wish to follow Bressan [1972] in his choice of a modal language for scientific theorizing; or be drawn, by the agreeable fact that in $L_{\omega_1\omega}$ all countable structures can be categorically characterized, to the view that the ultimate repository of scientific truth may be infinitary; or be driven, by the felt need to capture the continuity of space-time, to the use of generalized quantifiers. Indeed—Heaven forbid—someone might wish to combine all three strategies in order to be able to say exactly what the world is like, even though this means that one will be unable to deduce all the consequences of its being so. That would be a hard nut even for soft model theorists. The anti-reductionist who opts for a language for which Beth fails has caught the reductionist with his hands in the scrum.

But let us suppose that a conservative choice of language guarantees our would-be reductionist the relevant version of Beth's theorem. Let us suppose further that the thesis of physical determinationism is conceded to be equivalent to the antecedent of that theorem: that one allows the claim that one's overall theory *T* fixes the extensions of higher level terms once extensions for lower level terms are given. Thus the opposition is allowed a clean heel, and the ball moves away down the line. How under these assumptions can one nevertheless prevent the reductionist touchdown?

I have two ways to suggest, each one corresponding to one of my remaining two points isolated in §2: the existence of a defining formula R, and relativity to the theory *T*. These observations on the first of those points expand a hint in Peacocke [1979] pp. 122–23, n. 8. Those on the second are new.

First, note that in the preferred version of explicit definability given above, one can legitimately ask after the interpretation of the existential

quantifier. Is it to be interpreted constructively? If not, is it of any use, or—to see matters from the standpoint of the anti-reductionist—does it pose any threat? I would maintain that it is a minimal epistemological requirement that the quantifier be interpreted constructively. For it is highly unlikely that the true theory of the world will be axiomatizable, and even less likely that it will be decidable. Thus if the quantifier is understood non-constructively, we are highly unlikely ever to lay our hands on the defining formula R. For there are two ways we might seek it. One would be to test for the theory-equivalence of Q with $R_0,R_1,R_2, \ldots$ where the latter is an effective enumeration of all complex formulae with the same polyadicity as Q. If the theory is not decidable, we have no guarantee of a result. The other way would be to enumerate theorems of the (axiomatizable) theory *T*, and look for those of the form $\forall\bar{x}(R[P_1 \ldots P_n](\bar{x}) \equiv Q(\bar{x}))$. This wait-and-see method will not offer much comfort to the ardent reductionist. And were the theory *T* to be *un*axiomatizable, there would be no guarantee of success with any axiomatizable fragment of *T*, no matter how "eventual" the appropriate discovery might be allowed to be. The opposition has been forced to hoist a high kick, uncertain of possession no matter how much ground is covered.

These observations on the accessibility or availability of the defining formulae whose non-constructive existence is asserted by the consequent of Beth's theorem are admittedly more heuristic in their import than the logical character of the considerations adduced might lead one at first sight to believe. So be it; the possibility of reduction *in principle*, even if established with classical metalogical certitude, becomes a methodological non-starter when the degree of non-accessibility is revealed. To expand on this thought, consider for a moment the usual route to Beth's theorem in classical first order languages. One usually proves Craig's interpolation theorem first, and then applies Craig at an appropriate point in the proof of Beth so that the interpolant given by Craig serves up the definition R sought for Q. (For further details, see Tennant [1978] pp. 116–122.) Now take two sentences A and B as premiss and conclusion respectively of a valid argument. Any Craig interpolant I would be logically implied by A and would in turn logically imply B; and would moreover involve only such extralogical vocabulary as is involved in *both* A and B. Now Mundici [1981] has shown that there are formulae A and B each of length less than 1145 all of whose interpolants are of length greater than 2 to the power 2 to the power 2 . . . to the power 2 (seven times). Indeed, there is no recursive function that bounds the length of I as a function of the lengths of A and B. (See also Friedman [1976].) The conclusion to be drawn from the foregoing is that, even when the existential quantifier in the consequent of Beth's theorem *can* be interpreted constructively, there are formidable obstacles in the way of wielding Beth so as to yield the sought defining (hence: reducing) formulae.

My final and possibly most forceful objection, on behalf of the anti-reductionist determinationist, to the reductionist's invocation of Beth is the following. There is throughout, as already noted, an important dependence on the theory *T*. It is *relative to the theory T* that Q is *implicitly* definable in terms of the P_i. It is *relative* to the theory *T* that Q is *explicitly* definable in terms of the P_i. In the former case, without the theory *T* there simply is no fixing of extensions of predicates to do. And the reason why Q *is* fixed in extension when the P_i are is because they are marshalled together in the theory *T* which foists upon Q an intimate dependence upon the P_i. In the latter case, since this dependence is so intimate, it can be spelled out in an appropriate logical combination of the P_i. But still the theory *T* constrains all: it is only *modulo* the theory as a background of licit assumption that the explicit definer can demonstrate the lawlike co-extensiveness of Q and his compound R of the P_i.

What does all this imply for reductionism via Beth's theorem? My reply is a rhetorical question: what boots it thus to put in the boot of reduction? For:

the reducing (because: defining) formulae R served up for each higher level predicate Q whose theoretical autonomy is in question are adequate *as such* only on the very challengeable assumption that the theory *T* relative to which the defining (both implicit and explicit) takes place is stable and fixed once and for all.

We have already noted that the reducer *a la* Beth seeks only lawlike co-extensiveness. For depending on the order in which one cashes out each of finitely many higher order Q's in terms of the remaining Q's and then ultimately (after finitely many applications of the procedure) in terms only of the P_i, one can produce, for a given Q in the set, syntactically distinct reducers $R[P_1 \ldots P_n]$. Each of these distinct reducers for a given Q, however, will be provably co-extensive modulo the theory *T*. But the laws on which the lawlikeness of that co-extensiveness rests themselves contribute crucially to the very identity (better: *T*-relative equivalence class) of (any of) the defining formulae whose coextensiveness with the given Q is to be derived. This would be all very well if we had the One True Story, to use Putnam's phrase; if we had arrived at the Peircean endpoint of rational enquiry. For then, and then only, can we *use* the formulae R in order to dispense with the corresponding Q's. But science falls congenitally short of that goal. We are continually revising our scientific hypotheses in the light of new and newly interpreted evidence. We are continually "discovering" new tentative nomological links between terms at different levels in the many-layered union of the languages of all scientific disciplines. In seeking the One True Theory, bridge laws, reducing formulae and all, it would be methodologically wiser to have autonomous Q not subject definitionally to the vicissitudes of theory change. For what is a good Q, but

one suggestive of new links with other Q′ and better established P_i? Or one drawn from common sense theory expressed in everyday language, and now on the brink of having interesting and unexpected new links forged (perhaps via bridge laws) with the special terminology of a developing branch of science? The very impetus to theoretical enrichment will be lost if one takes the reductions available at any incomplete stage of scientific theorizing and henceforth eschews the higher order concepts supposedly "defined away." *The tentative nature of any theory T in actual scientific practice will leave one wielding an obsolescent definition.*

On this point—the most important, to my mind, by far—I conclude the case for the anti-reductionist who lets the backs get hold of the ball. The obsolence of reductive definition is an essential feature of the very process of scientific change. The would-be reducers must be constantly reminded that they will forever be playing a curtain raiser—and that it is, after all, only a game.

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*This paper was presented to the Seventh International Congress on Logic, Methodology and Philosophy of Science at Salzburg in 1983. It is the promised technical sequel to Tennant (1985). It is also, I hope, a timely corrective to the confusion on the part of some reviewers of Schilcher and Tennant (1984). Both Rose (1984) and Woodfield (1984) assume, incorrectly, that emphasis on the scope of biological explanation commits one to biological reductionism in the human sciences. The reader who has come this far will, I hope, now agree that this is definitely not the case.

I am grateful to George Bealer for clarifying in correspondence how he applies Beth's Theorem.

IS A GOD'S EYE VIEW AN IDEAL THEORY?[1]

BY

MICHAEL LISTON

WHAT THE FACTS are, what kinds of objects exist, and how it is with those objects do not depend in any important manner on epistemic agents in general and on humans in particular. Thus *F*-s might exist and certain portions of the world might be thus-and-so independently of whether or not these items could causally affect us in such a way that we could ever come to know or correctly describe them. Moreover, insofar as truth is a correspondence between linguistic items and the facts, to the extent that *what the facts indeed are* is epistemically unconstrained, so is truth independent of our ability to determine it. Even our epistemically best theory could fail to correctly describe the world in some way. Or so it would seem.

In this century, however, these realist theses have come under increasing fire. A common form of anti-realist complaint concerns the claim that there are, or could be, recognition-independent facts. The anti-realist typically suggests that such a claim either makes no sense or has no point. In keeping with this view the anti-realist tends to shift the focus of the dispute away from metaphysical and toward semantical issues. Anti-realists from Schlick to Dummett and Putnam have endorsed the claim that the dispute about realism properly concerns meaning, truth, and the relationship between language and the world. Two features seem characteristic of this anti-realist tendency. First, there is the rejection of absolute realistic truth-conditions in favour of relativized verification-conditions or assertibility conditions in the semantic treatment of at least some of the sentences of a language. In line with this, there is an accompanying rejection of bivalence and/or arguments for non-standard logics for the treatment of that set of sentences. Second, there is a relativization of ontological—i.e. existential—claims in similar ways: relative to verification procedures, theories, principles of

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