Center for Philosophy of Science, University of Minnesota
Working paper MCPS–90–01, June 1990 (References updated January 2004) #146

Corroboration and Verisimilitude: Against Lakatos's "Sheer Leap of Faith"

Paul E. Meehl
Department of Psychology
and
Center for Philosophy of Science
University of Minnesota

In the last paragraph of Lakatos's (1978, posthumously published) paper on anomalies and crucial experiments he says, with regard to progressive problem shifts since Elizabethan science, "We can only (nonrationally) believe, or, rather hope, that we have been [heading toward the truth]. Unless hope is a 'solution,' there is no solution to Hume's problem" (p. 223). What is there formulated diachronically in the Lakatosian framework of theoretical history, has an achronic counterpart in asking whether we have any reason for expecting verisimilitude to be, at least stochastically, related to corroboration. I find no printed passage on that point, but I once heard Imre say, in a discussion with Feyerabend and members of the Minnesota Center, "Belief that there must be some relationship between verisimilitude and corroboration is a *sheer leap of faith*!" I argue that in this degree of epistemological pessimism, super-rationalist Lakatos was, happily, mistaken.

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Presupposing Nomologicals—Hume's Problem Bypassed

Since his last sentence in the paper cited mentions Hume, I should say that the present paper is "methodological" rather than "epistemological," in that I address a problem that concerns the thoughtful working scientist who has no interest in Hume's problem of justifying induction in general. Of course, one who has a general philosophical bent avocationally, as I do (see Appendix II), can get interested in discussing Hume's problem by taking off the scientist hat and putting on the philospher hat; but I have never conversed with a scientist in any field (physical, biological, or social) who had any *scientific* interest in Hume's problem. As Center director Giere pointed out in one of our meetings, when Newton's methodological views were being discussed, Newton worried about metatheoretical questions, about how experimental findings could demonstrate a theoretical conjecture. But so far as we know, he never worried about whether an experiment would come out the same way next week if he conducted it in the same way. So I am bypassing the philosopher's classic Humean problem of induction, and with a clear conscience, partly because Lakatos himself, in developing his amended and reconstructed form of Popper's metatheory, alleges that one cannot avoid a "whiff of inductivism." If one cannot help taking a whiff of inductivism, why not draw a deep breath?

Since that's not my interest here, and I don't believe the development depends on precisely how you do it, I shall take something like the existence of nomologicals, the "orderliness of nature," for granted, as do all working scientists in their theorizing and experimenting. (The literally true theory stating those laws I label 'T_{OJ}' for the theory held by Omniscient Jones, as the late Wilfred Sellars was fond of putting it.) If I must state my presuppositions, I will go back to John Maynard Keynes (1921) and say that I posit that the uni—verse is not infinitely complex (the Postulate of Limited Independent Variety), and that its basic constituents, once we get past the Big Bang, have persistent dispositions (the Postulate of Permanent Kinds). Whether these two postulates plus probability theory suffice (say, by taking nomic necessity to relate *universals*, as in Armstrong [1983], Dretske [1977], Swoyer [1982], and Tooley [1977]) I do not discuss, although that is my hope and conjecture. In any case, I simply assume arguendo that there are natural laws, in principle formulable as postulates. Almost everyone has long agreed with Russell (1948, p. xv) that "scientific inference requires, for its validity, principles which experience cannot render even probable." Finally, while my personal metaphysic is that of scientific realism, I doubt that matters here, since I think even a simon–pure instrumentalist could go along with most of what I say (see Appendix II). But I need that "whiff of realism" that distinguishes verisimilitude as an ontological predicate from corroboration as an epistemological ("evidentiary") predicate. I shall therefore allow myself to speak of empirical theories as being true or false, most of them having degrees of truth, without compunction. While it is generally admitted that neither the ontological concept of verisimilitude nor the methodological concept of corroboration has been adequately explicated by their advocates, I am not going to worry about that, because I am going to offer some alternative crude explications, hoping that my main thesis does not hinge upon which of them we adopt.

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Metatheoretical Idealization: Calculus and Embedding Text

From the "received view" of the logical empiricists I take the distinction between *calculus* and *embedding* text, realizing that scientists normally do not bother to parse it cleanly. That doesn't mean that the metatheorist may not properly parse it for analytic purposes. Thus, even with such an explicit "anti-deductivist" and "anti-postulator" as the arch-behaviorist, super-operationist B. F. Skinner, it is easy, in exegesis of a Skinner text, to discern which statements he considers the empirical primaries of his behavioral system, and which ones he derives. I invite any reader who doubts this to have a look at the classic work (Skinner, 1938) and see whether he experiences any appreciable difficulty, as he reads, discerning which is which. Thus, for instance, the quantitative properties of cumulative records under periodic and fixed ratio schedules are derived from basic probability arguments superimposed upon the fundamental notion of the dissimilarity of two exteroceptive or proprioceptive stimuli S^{D} and S^{Δ}.

I think it important to make Carnap's (1939) distinction, which there is a tendency to ignore, between the "general" and the "special" calculus. The general calculus consists of the formation and transformation rules of logic and mathematics, together with the mathematician's postulates and their deductive consequences. With possible exceptions in quantum mathematics (e.g., problems with three-valued logic), this formal machinery is shared by all of the empirical sciences that employ mathematics as a tool. Given this formalism, whose theorems are presupposed by the working scientist, simply taken over from the mathematician (as when a physicist relies on a table of integrals), Carnap's "special calculus" consists of *statements in the formalism* which are, however, not *theorems of the formalism*, and which, therefore, will be found asserted in the calculus of one empirical subject matter, but not in others. These latter, the mathematical (nontextual) component of what we could call the *theoretical premises* of the discipline, show their domain specificity, absent any embedding text, by the fact that they are not theorems of general mathematics. *Example*: In Skinner's (now abandoned) concept of the "reflex reserve," he conjectured that the rate of responding as shown by the momentary slope of a cumulative record in the Skinner box was proportional to how many responses remained in the reserve during an extinction process, the proportionality constant being a function of hunger drive. So one writes $\frac{dy}{dt} = \mathrm{K}(\mathrm{R} - y)$ which leads, when integrated, to the equation $y = \mathrm{R}(1 - e^{-\mathrm{K}t})$. Now neither of these is a theorem of general mathematics. What *is* a theorem of general mathematics is that the second equation follows from the first, given that $y = 0$ when $t = 0$. Presented with either of these relations, one does not have any way of knowing whether we are discussing animal learning or the growth of a corn stalk. (Applied mathematicians often refer to this as a "simple positive growth function," because many growth processes in the biological sciences are well fitted by it.) Without any embedding text, we do not know what science is involved. We would, without embedding text, infer that the equation occurs in an empirical science, because the equation is not itself a theorem belonging to Carnap's general calculus. There are several reasons for emphasizing this old distinction, and one is that it helps show why Church's Theorem is a "non-problem for the working scientist." When we ask whether a well formed formula in the theoretical language is decidable, given a certain mathematical postulate set belonging to some empirical discipline, we are not asking the logicians' question as posed by Church. We are asking whether, given the postulates of the special calculus characteristic of that science, this *wff* is a theorem or countertheorem. Aside from that, any *wff* that would arise, even in the general calculus presupposed by an empirical scientist, would surely fall under one of the ten cases for which a solution to the *Entscheidungsproblem* has been found by the logicians (Church, 1956, pp. 247–257).

I also find it convenient to make a distinction, not regularly made by philosophers, between two components of the embedding text that applies a calculus to an empirical domain. One part of the embedding text connects terms of the observation language (and statements formed from them) with a subset of terms in the theoretical language, rendering the latter terms, to employ the old terminology, "operationally" or "ostensively" defined, statements formed out of them being treated as Carnap's (1952a; but *cf*. Maxwell, 1961; Sellars, 1961) "meaning postulates," Reichenbach's (1938) *Zuordnungsdefinitionen*, or as "bridge laws." If the latter, one has the question of whether bridge laws are to be construed as conventions, or as, in some sense, "real definitions" or "explications of meaning," or (my preference) as empirical laws. If they are empirical laws, are they among the theory's postulates, or are they themselves theorems derivable from the postulates? Here I agree with Feyerabend (personal communication, February, 1960) that in the advanced state of a science, they should *all* be theorems, although I, unlike him, make one exception, which I am convinced (with Dubois–Reymond) will never be a theorem, namely the mind–body tie. Whether conceived as bridge-laws or definitions, this part of the embedding text I call the *operational text*.

Some hold that the Ramsey Sentence offers a complete explication of how theoretical terms are defined via the nomological network, whereby a proper subset of them are directly coordinated to observational terms, and the rest derive their meaning "implicitly," by a combination of their role in the network and a kind of "upward seepage" from the facts. The first set is the only portion of the embedding text that is, strictly speaking, part of the substantive theory, the rest being what the old positivists would have called noncognitive in character, that is, the scientist's imagery (Kekulé and the hoop snake!), useful in the context of discovery or in pedagogical settings. However, *prima facie*, given the machinery of the Ramsey Sentence as a way of eliminating theoretical terms without eliminating the theory, there is another portion of the embedding text that most scientists consider to be part of their explanatory machinery, and which is obviously not directly coordinated. I call this the *interpretive text*. This can include terms that in macro-concepts are observables (such as velocity), but that may be unobservable as applied in the context (e.g., the kinetic theory of heat, in which the v refers to the velocity of a single molecule, which is unobservable). Presumably a firm believer in complete Ramsification as an adequate account of the empirical content of scientific theories will hold that all of these theoretical terms of the embedding text can be Ramsified out, without loss of cognitive meaning. I am not myself persuaded of this, but have had little success in convincing my philosopher colleagues that it is a problem, or even an interesting question. The last time I talked with Carnap before his death, I suggested, in discussion of the Ramsey Sentence, that it is easy for us to feel cognitively satisfied with Ramsification of something like the kinetic theory's explanation of the gas laws, or Graham's law of diffusion, because the *notation* of the formalism includes letters like *m* and *v*; so that when one sees the term $\frac{1}{2}\left(\overline{mv^2}\right)$, one presumes that this designates the kinetic energy of a mass *m*. But if one were presented with the mathematics of Maxwell, Kelvin, and Co., using a totally unfamiliar notation, only associating certain symbols (such as the *p* of pressure per unit area on the piston face) with a summable set of momenta, would one fully grasp the sense in which the kinetic theory constitutes an intellectually satisfying "explanation" of the molar gas laws and the law of diffusion? I didn't get very far with that move, because Carnap (predictably being the logician more than the philosopher) looked at me benignly and said, "But, Meehl, what else would you want, since everything is deducible?"

Aside from the terms of the interpretive text that are tied to a particular scientific domain, and are the giveaway as to what domain we are in (e.g., "particle," "libido," "prime rate," "status," "protein," "valence"), there is an interesting set of terms that cut across empirical domains, what we might call *generic scientific*, or *cross-domain metaphysics*. I have in mind such basic causal and compositional terms as 'accelerate,' 'adjoin,' 'annihilate,' 'approach,' 'be composed of,' 'combine,' 'conflict with,' 'countervail,' 'couple,' 'distance,' 'evolve,' 'extend,' 'facilitate,' 'fuse,' 'inhibit,' 'inner,' 'interact with,' 'interval,' 'link,' 'oppose,' 'persist,' `potentiate,' 'prevent,' 'produce,' 'region,' 'resist,' 'retard,' 'separate,' 'subsequent to,' 'transform,' 'transmute,' 'turn into,' 'vanish,' 'vicinity of,' and the like. These terms appear in all empirical sciences, some more than others. The interesting thing about them is that if we conjoin them with the formalism, being careful to delete embedding text that is domain specific (e.g., 'habit,' 'profit,' 'metabolize,' 'mass,' 'valence,' 'igneous'), a reader has no way of knowing whether we are doing psychology, economics, physiology, physics, chemistry, or geology. An electron-positron collision results in mutual *annihilation* and *turns into* a photon. In Freud's pre-1926 theory, damned up libido is *converted into* anxiety. After that date (Freud, 1926/1959) the *inner* anxiety signal *produces* a defensive operation that *inhibits* the expression of the libidinal impulse. None of these terms, despite their nonspecificity to an empirical domain, is metalinguistic. They are all terms of the scientific object language. I do not assert that these terms, or some part of them, are incapable of being Ramsified out without loss of scientific meaning. I only say that it is not crystal clear whether they all can be Ramsified out, and that it is an interesting question that logicians should address. In what follows I try to make the treatment neutral with regard to the answer to this question, but I will have to advert to it, because of the problem of *how many alternative postulate sets* are adequate for an empirical fact domain. To explain the facts of an empirical domain involves the question of whether all of the nonoperational embedding text can be Ramsified out. I do have trouble about Ramsifying a concept like Freud's libido, but even more, some fairly simple physical ones such as distance, or small time intervals, because sometimes in the life sciences, and sometimes in the physical and chemical sciences, one does not directly ("operationally") link two points in time that are associated with components of a process participated in by theoretical entities, yet the relative size of spatial and temporal intervals plays a role in the theory. I shall say more about this problem later.

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Input-Output Theories Used for Illustration

The only kind of empirical theory I consider here is what may be called an "input-output" theory. Variables that we observe (and sometimes manipulate experimentally) are conjectured to influence the states of inferred theoretical variables, which in turn produce observable outputs. The paradigm nomological of such a theory has the causal arrow form $x \rightarrow \theta \rightarrow y$, where x [input] $\rightarrow \theta$ [theoretical] $\rightarrow y$ [output], or with the functional form of causal dependence being specified, $\theta = f(x)$ and $y = g(\theta)$, so the output varies with input according to a relation $y = F(x) = g(\theta) = g[f(x)]$. How severe this restriction on kind of theory is upon my argument against Lakatos I do not consider. It is a natural choice for me, as in psychology such theories are a common theoretical form. But obviously many theories in genetics, physiology, economics, sociology, and political science share this kind of overall structure. See, for example, Atkinson, Herrnstein, Lindzey, and Luce (1988, vol. 1: pp. 40, 52–53, chapter 2 *passim*, p. 428, chapter 13, pp. 721–723, and *passim*; vol. 2: pp. 55, 144, 180, 183–184, chapter 3 *passim*, pp. 354, 547–548, 618, chapter 10 *passim*, chapter 12, pp. 821–822, and *passim*); Glymour (1980, Chapter 7, "Theory testing in the unnatural sciences," 1983); Glymour, Scheines, Spirtes, and Kelly (1987); Herrnstein (1979); Hull (1943); Koch (1959–1963 *passim*, 1959a, 1959b, 1962); Loehlin (1987); MacCorquodale & Meehl (1953, 1954); Nevin, Mandell, and Atak (1983). One thinks of the economist's causal chain initiated by fiscal policy manipulation, where lower taxes and increased transfer payments produce a sequence: More disposable income → increased aggregate demand → decreased retail inventories → increased wholesale orders → increased factory production and capital investment. Biologists study input–output chains such as: Rainfall → earthworm availability → bird population → feral cat population. Or, to take an example of home-baked applied physics, I observe that the (uncompensated) grandfather clock runs fast in winter and slow in summer. Knowing nothing of clock mechanisms, I can at least time the pendulum bob, and find that its period is roughly proportional to the square root of temperature. Recalling a bit of high school physics, I combine Galileo's law with the thermal expansion law to explain the clock's behavior. Absent such an interpretive text, not knowing any physics, I might nevertheless come up with a conjecture that the observed $T = K(t°)^{1/2}$ should be decomposed into $T = k_1x^{1/2}$ and $x = k_2t°$, "suggested"—but not entailed—by my partial glimpse of the innards, the moving bob.

I am going to consider only input-output theories strong enough to predict functional relations between the observables, which they do by conjecturing functional relations between input and theoretical variables and between the latter and output variables. They are not required to be strong enough to derive the parameters of these functions, or even to derive cross-situational relations among the parameters. Such a theory may specify that a measure of rat learning in the maze will be logarithmic, but not say what the constants will be, nor even attempt to say whether a constant found by fitting a logarithmic function in a given maze can be "plugged into" an expression for the course of learning in a different maze, e.g., extrapolation from a single unit to a multiple unit *T*-maze.

Simple example: A psychologist conjectures that stimulus intensity influences a central brain state called "arousal," a conjecture for which he perhaps has some independent support from the neuroanatomy of the reticular activating system (RAS). He conjectures that the dependency of arousal on stimulus intensity can be adequately fitted by a linear equation. The output variable is tachistoscopic recognition time, and he conjectures that the relation between the arousal variable and the output variable is an inverse proportionality. From the two conjectured functional dependencies $\theta = ax$ and $y = \frac{b}{\theta}$, he deduces that the graph of recognition time against stimulus intensity will be a hyperbola, $xy = \mathrm{K}$. We of course allow for multivariate dependencies, so that, for instance, if an input variable controls the state of two theoretical variables u,v which jointly determine the value of an output y, we have $\frac{dy}{dx} = \left(\frac{\partial y}{\partial u}\right)\left(\frac{\partial u}{\partial x}\right) + \left(\frac{\partial y}{\partial v}\right)\left(\frac{\partial v}{\partial x}\right)$.

Next we consider a class of *wff* s formulated in the observation language. I do not enter here into the perennial dispute about the theory infection of observables; and I let the reader choose the observational level and reliance on instruments (e.g., an ammeter reading, a spoken word, a phenomenal green patch, or a chair characterized in Carnap's thing language). We idealize the set of observational *wff*s as "covering the waterfront" of experimental designs in a specified scientific domain. We need not worry about how large that is. If a particular experimental design is characterized by parametric regions, and the input-output relation by function forms (so that we are not getting into the pure mathematician's problem of the cardinality of the continuum as representing possible numerical values of inputs and outputs), this experimental class is, while large, nevertheless finite. There are many different things you can do by manipulating the parameters of stimulus conditions of rat behavior in the Skinner box, but they are not infinite in number. (Some of my colleagues in operant behaviorism tell me that the recent issues of the main operant behaviorism journal tend to be rather boring because, with some exceptions, what researchers are doing is ringing the changes on various complex schedules of reinforcement of the sort that no rat would ever find in its nonuniversity ecology. As my Skinnerian colleague Roy Pickens once said, "It's getting to the point that I call the 'Blue Maze' phase, where the psychologist says, 'Well, we have tried *T*-mazes and *Y*-mazes, single unit and multiple unit mazes, elevated mazes, and mazes where the rat has to swim; but come to think of it, we never yet ran any rats in a blue maze. Let's do that.")

Setting up a particular experimental design, we select a mathematical function that describes the input-output relationship. While the options may be theoretically motivated, I shall treat the choice as a curve fitting problem, and, for now, assume away any persisting replication difficulties. Consider the (huge but finite) class of all such functions characterizing experimental outcomes. We now ask whether our list of bridge laws (or, if you prefer, meaning postulates) is such that corresponding to each observational *wff* that has been admitted into the corpus there is operationally associated a corresponding *wff* in the theoretical language. If so, we inquire whether every such operationally coordinated theoretical sentence is a theorem of the postulate set. If so, we call the theory *empirically complete*. (I do not consider completeness or categoricalness in the strict sense of the contemporary logician, but only in the looser sense of Russell and Whitehead, who said that the postulates of *Principia Mathematica* would be "complete" if they showed themselves capable of giving rise to all of the theorems that the mathematician already believes.) In our case, we are not requiring (although I do not know whether doing so would have any undesirable consequences) that *every wff in the theoretical language* should be either a theorem or countertheorem. We only require that every theoretical *wff* that is coordinated to an observational *wff* (i.e., every theoretical *wff* required for an observational *wff* to be theoretically derived) is a theorem.

The main point argued in this paper is that one can reasonably expect (given the basic inductive metapostulates, despite Hume) that *there ought to be a stochastic linkage—and not a weak one—between a theory's verisimilitude and its corroboration defined as its experimental success record*. In the course of showing this I shall make a secondary point that (contrary to a truism of the pure logician not much interested in empirical science) it is not true—in any interesting, scientifically relevant sense—that "there is always an infinite number of alternative postulate sets that will explain any set of facts." To show this I must be allowed certain methodological constraints upon admissible theories which are commonly if not universally imposed by working scientists.

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Inadmissible Theories: Isolates, Formal Undoing, Hidden Variables

The resources of the propositional calculus are not adequate to the task of explicating the structure of scientific theories, because the latter always involve "getting inside the propositions," especially in the case of theories that are strong enough to state mathematical functions relating theoretical and observable variables. (Predicate logic is not a good formulation for analyzing empirical scientific theories, as Suppes points out. It is pretty adequate for taxonomic theories, but unhelpful; and for functional-dynamic, compositional-structural, or developmental theories, it is either clumsy or totally inept.) There are of course trivial solutions to the problem of postulate sets adequate to factual statements taken as the to-be-derived theorems, the easiest of which is to write down all of the observed generalizations as postulates themselves! When the methodologically curious empirical scientist asks the philosopher whether there is an infinite number of postulate sets to explain a finite set of theorems, he is not interested in examples of this kind. The "nomological network" metaphor is a handy way of talking because it highlights the fact that a scientific theory is not merely a heap of disconnected propositions. This is why the propositional calculus won't do as a tool for introducing such concepts as `testability' or `empirical' (which got Ayer into hot water repeatedly with Church). You have to get into the innards of the statements to understand why the theory *explains* anything.

The nodes of the nomological network are the theoretical entities, and the strands are the compositional, causal, and developmental laws that relate them. The kind of theory we are concerned with is such a network, which is not very restrictive because, so far as I can tell, all interesting scientific theories are networks rather than heaps. To speak less metaphorically, one of the characteristics of interesting "strong" scientific theories is that there are few, if any, two-term functional relations in which one has a conjectured functional dependency $\theta = f(x)$ and $y = g(\theta)$, with none of the three variables—two observables (x, y) and one theoretical (θ)—being functionally connected with anything else in the system. Such a one-term mediated theoretical dependency, not cross-connected with any other input, theoretical, or output variable z, ϕ, w, I call an *isolate*. So a network is a theory in which there are no isolates. We may either forbid isolates (which I think sometimes happens in science as a matter of policy, although obviously not a matter of logic); or, more tolerantly, we may simply say that we do not concern ourselves here with theories that contain an isolate, or, perhaps, roughly, that contain "too many" of them. What we definitely exclude is a theory which consists solely of a collection of isolates, in which each theoretical term θ_i is linked by an input relation to only a single input variable x_i, has an output link to only a single output variable y_i, and no "horizontal" functional linkages to any other theoretical variables. I am tempted to say that an isolate, even in a theory that is otherwise a network, would be routinely excluded by critical scientists because it does not assert anything, and hence does not "explain" anything that is not asserted ("unexplained") by the *xy* relationship itself. I would like to say this as an exclusionary principle, but it is not quite correct, because there is a (formal) cognitive difference between writing the equations $\theta = \mathrm{a}x^2$, $y = \mathrm{b}\theta^{1/2}$, and the equations $\theta = \mathrm{a}x$, $y = \mathrm{b}\theta$, although they both entail the sole observable dependency $y = \mathrm{K}x$. A nondescript theoretical variable, about which nothing is said by way of interpretive text, is not strictly a cognitive nothing, although one knows that the history of science includes arguments about whether somebody was "really saying anything beyond the facts" when that is about all he had, plus what Herbert Feigl used to call "a promissory note." Since I remain devoted to Carnap's Principle of Tolerance, I would prefer merely to sort theories into those that do and do not contain isolates rather than to enact a legislative prohibition against having any isolates at all. Of course one must distinguish between how we criticize a theory at an early stage in its development, allowing the theoretician to inform us that he has some ideas up his sleeve about saying something later as to the inner nature of the theoretical variable θ but he hasn't worked out the experimental consequences yet, and whether we categorize the theory as "admissible" for metatheoretical purposes.

Another thing we might forbid (here I think we would get considerable agreement, at least among psychologists) or, failing that, insist upon labeling pejoratively, is an operation performed upon the value of a theoretical variable θ preceding plugging its value into an output link function $y = g(\theta)$, which operation simply undoes an operation $\theta = f(x)$ performed in getting to the theoretical variable from the input. For a given input-output function, there could be an infinite number of such cases if the right hand knoweth not what the left hand doeth. For example, in the simple case of input-output relation $y = \mathrm{b}x$, one might arbitrarily write $\theta = \mathrm{c}x^2$ and $y = \mathrm{d}\theta^{1/2}$, or $\theta = x^3$ and $y = \mathrm{d}\theta^{1/3}$, and so on ad infinitum. I can't imagine any scientist, absent interpretive text that motivates both the *raising to the power* and the *extraction of the corresponding root*, writing linked equations of this kind. If one did so, it would surely be criticized by colleagues. I shall refer to such a pair of pointless, give-and-take-away operations as a *formal undoing*, taking a handy word from Freud. Aside from whether we would criticize it methodologically, it is obviously conceivable ontologically that such relationships should sometimes exist. Allowed or not, it is psychologically unlikely to be invented, except for dependencies of a very simple kind like the square and root combination.

By considering the derivatives, we see how improbable it would be for the theorist to concoct a formal undoing inadvertently. In Omniscient Jones's theory T_{OJ} the input and output functions are $\theta = f(x)$ and $y = g(\theta)$ respectively. Then $y = g[f(x)]$ and Chain Rule gives

$$\left(\frac{dy}{dx}\right) = \left(\frac{d\theta}{dx}\right)\left(\frac{dy}{d\theta}\right) = \left(\frac{df}{dx}\right)\left(\frac{dg}{d\theta}\right)$$

Labelling the distorted input and output functions by a tilde, the erroneously constituted output/input derivative is

$$\left(\frac{dy}{dx}\right)_{\mathrm{err}} = \left(\frac{d\tilde{f}}{dx}\right)\left(\frac{d\tilde{g}}{d\theta}\right)$$

which on the left must be the same as the correct one, i.e., the condition is that

$$\left(\frac{df}{dx}\right)\left(\frac{dg}{d\theta}\right) = \left(\frac{d\tilde{f}}{dx}\right)\left(\frac{d\tilde{g}}{d\theta}\right)$$

Hence the erroneous output link function $\tilde{g}$ must be chosen so that

$$\left(\frac{d\tilde{g}}{d\theta}\right) = \left(\frac{df}{dx}\right)\left(\frac{dg}{d\theta}\right)\left(\frac{dx}{d\tilde{f}}\right) = \left(\frac{df}{dx}\right)\left(\frac{dg}{d\theta}\right)\left(\frac{df^{-1}}{d\theta}\right)$$

The "compensating" erroneous output function's derivative must be factorable into the derivatives of the true input and output functions and the derivative of the true output function's inverse. A simple case of this, as in the roots and powers example, will be immediately detectable as a formal undoing; and a case complex enough to go undetected would be hard to concoct unintentionally.

The simplest case of a network property avoiding an isolate and making such formal undoing difficult or impossible is what may be called an *output fork*. We have here a functional dependency of a θ upon the input variable x, and then we have two output variables y and z, each of which is functionally dependent upon the intervening variable θ in accordance with the functions $y = g(\theta)$ and $z = h(\theta)$. Even if we permit a formal undoing, we will be in trouble except for an unusual stroke of luck or perversity on the theorist's part. Conjecturing an erroneous substitute for the output link g, in order to fit the facts of the observable relation $y = \mathrm{R}(x)$, we must alter the input link $\theta = f(x)$ in just the right way. But if we do that, then the input-output linkage between x and the third variable z will no longer be correct. So we have to fiddle just right in assigning the (erroneous) substitute for the output link $z = h(\theta)$. If our network includes horizontal (inter-θ) linkages, or multiple input determiners, when we write the various equations involving the partial derivatives for mediating variables, we will get into plenty of hot water. It is as if the assignment of one incorrect functional relation acted, so to speak, as a carcinogenic mutation giving rise to a kind of "syntactical cancer" metastasizing throughout the nomological network.

A multiple-tined output fork may suffice to overdetermine postulates in the formalism although the input variable is discontinuous, or even nonquantitative, so no input function $u = f(x)$ with derivatives exists. I have not worked out general conditions for this case, which appears fairly often in fields I work in (psychopathology, personology, behavior genetics, psychometrics, outcomes prediction). *Example*: L. L. Thurstone (1959), the psychologist who invented multiple factor analysis as a generalization of Spearman's tetrads, conducted research for the Navy on food preferences, a problem of great importance for military morale and combat effectiveness. If one computes (method of paired comparisons) the percentages of choices where roast beef is preferred over fried chicken, and chicken over sweetbreads, then a crude summing of these two increments $\Delta p_{ij} + \Delta p_{jk}$ will badly mispredict the pairwise preference Δp_{ik} for roast beef over sweetbreads. If, however, one postulates a latent "hedonic continuum" on which the central value for each food is located, with Gaussian distributions of fluctuations around those central values, then the Gaussian integral of the difference $(\overline{x}_i - \overline{x}_k)$ gives an excellent prediction of the beef-over-sweetbreads proportion. So we have to go from two pairwise preference proportions via the Gaussian integrals to two axis cuts, then to a third standardized difference score, and back to the desired proportion p_{ik} via its Gaussian integral from the cut upwards. Finding this predicts p_{ik} successfully, one may then go on to consider various neurophysiological states or processes constitutive of the hedonic dimension; but the implicitly defined construct *hedonic value* is, in the formalism, uniquely determined by the total set of $\binom{n}{2}$ pairwise preferential proportions. The observed numerical values fit a set of latent Gaussian fluctuation distributions and, I believe, *nothing else*.

Another example is from behavior genetics. Suppose one identifies a set of, say, four stochastic indicators of a conjectured schizogene (Mendelian dominant predisposing to, but not necessitating, schizophrenia) by showing their markedly *elevated frequency* and *pairwise compresence* in schizophrenes, schizophrenes in remission, and MZ twins of schizophrenic probands, but not in remitted manic-depressives or their relatives. Once such high-validity (but fallible) indicators of schizotypy have been identified, multiple taxometric bootstraps analyses (Meehl & Golden, 1982; Meehl, 1973, 1990b) provide estimates of the sensitivity and specificity of each indicator among the parents of probands, so one infers that the latent distribution situation for parents is taxonic. This leads to several theorems (Golden & Meehl, 1978) concerning quantitative patterns among the parents. For example, if the valid positive rate of a schizotypic sign is p_s and the false positive rate p_n, then in a 4-fold mother/father table the numerator of the ϕ-coefficient $p_{ij} - p_i p_j = -\frac{1}{4}(p_s - p_n)^2$. This relation is not derivable from a polygenic theory, and even if we allow the latter to be adjusted *ad hoc*, such an adjustment unavoidably involves an auxiliary conjecture of high negative assortative mating for schizotypy, already refuted by several diagnostic and psychometric spouse studies. *Per contra*, if the numerical results do *not* fit these pattern theorems, the dominant gene theory stands clearly refuted. Since a large, varied, and consistent body of research shows beyond reasonable doubt that schizotypy is inherited, major locus (as *sine qua non*, not as sufficient) and polygenic exhaust the etiological possibilities. Within the general genetic constraints and the several statistical facts, we will literally be "forced" to accept one of these models, rejecting the other.

A third situation which one will want to identify and treat suspiciously, or highhandedly forbid, is *hidden variables*. These we would not only be inclined to label pejoratively, but (except in the pure context of discovery, allowing the theorist to suspend criticism and free associate looking for possibilities) we would probably forbid them, absent an interpretive text that motivates them. I mean by hidden variables not quite what, as I understand it, has been the case in quantum mechanics, because there ingenious experimentation relying on Bell's inequality "digs them out" and makes them nonhidden. Rather I have in mind cases like the following: A psychologist introduces a mediating variable θ about which he may or may not provide some interpretive text and input-output linkages, say with an output fork or a horizontal linkage that preserves θ from being an isolate. So far so good, nothing objectionable methodologically. But suppose he now writes a compos—ite relation $\theta = \mathrm{a}\theta_1 + \mathrm{b}\theta_2$, fitting this with the input relation by writing appropriate linear weights on the component θs, with appropriate input functions $\theta_1 = f_1(x)$, $\theta_2 = f_2(x)$. Again, we have a potentially infinite number of ways to do this, so long as we keep the coefficients straight. We could also do it with three components θ_1, θ_2, θ_3, or any other number of them. Without any weights assigned to them, we could simply redefine θ as $\theta = \theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5$ and then say that these five θs are functions of the input variable x such that they give rise to the composite input function $\theta = f(x)$ we have initially postulated in the special calculus. I can't imagine anybody in his right mind doing such a thing, absent some kind of interpretive text which would make the decomposition an "explanatory model" having surplus meaning over and above the formalism. So I am going to say that we forbid hidden variables which the formalism combines to yield a linked theoretical variable but in which there are no separable input or output causal links to the components, and no interpretive text that provides a surplus meaning to the formalism. Here again, a true believer in total Ramsification would not need to add that second condition, since on his view no surplus cognitive content is provided by the interpretive text, all of the "empirical meaning" being provided by the network's structure plus upward seepage from the observational predicates.

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Ordering Theories by Verisimilitude

Assume those general conditions on the admissiblity of an input-output theory, or, more tolerantly, the meta-semantics defining what I shall call a *strong explicit network* (= mathematical functions with no isolates, hidden variables, or formal undoings, and with a subset of the derivable theorems operationally coordinated to *wff*s in the experimental observation language). We will now proceed in an unusual way, ontologically rather than from the epistemic position of the scientist trying to figure things out; we adopt the position of Omniscient Jones, and *start* with Omniscient Jones's theory, labeled T_{OJ}. We then proceed to degrade T_{OJ} by altering the functional relationships that it postulates. We have a little semantic problem saying what it means to "alter the functional relationship" if one says, in Ramsey Sentence spirit, that the meaning of theoretical terms is given by the net; consequently, to identify a theoretical term is to locate it in the net, as a result of which one has the old problem of whether all, or only some "privileged" nomo-logicals, are definitory (or, better perhaps, "explications") of theoretical meaning. I don't think anything important for my purposes hinges upon that, and I don't wish to get into a semantic argument, such as that between Millikan and others as to whether one should say "positive" and "negative" electron or "positron" and "electron." The problem is to explain the notion of altering the functional relations conjectured bet-ween a pair of theoretical terms, i.e., how do we decide whether we are still dealing with "the same theo-retical term" when we alter the structural features of the Ramsey Sentence? Let us consider a theoretical term as "the same term" so long as the postulated causal or compositional linkages with other theoretical terms (and observational terms, if any are directly linked) are maintained, even though the mathematical function in one or more of them is changed. Thus, when relating an intervening variable to an input variable, substituting a square root for a linear function, or a logarithm for a decay function, constitutes a modification in T_{OJ}. If an input-output function $y = f(x)$ is mediated in T_{OJ} by two or more intratheoretical linkages functions, then if we alter one of these functions, the input-output function will be altered; and since T_{OJ} fitted the experimental function, the altered one cannot do so. The only way we can alter T_{OJ} without sabotaging its fit to the facts is to alter two or more of the postulates in such a way as to perform a formal undoing operation. Similarly, if we delete a strand in the nomological net, making a variable that formerly was a function of some other theoretical (or observable) variable no longer dependent on it, or if we add a strand (a further dependency), this will necessarily alter at least one input-output function, and the resultant theory will not fit the facts. Finally, suppose we simply delete a postulate, rather than altering it; or we delete all the postulates that contain the particular term. Then we may or may not get a *mis*prediction, but we will either get a misprediction or *non*prediction of some experimental relation, thus a postulate set that no longer fulfills our original condition of empirical completeness. This last is obviously true of all actual theories in the life sciences, biological or social. There are, of course, thousands and thousands of *wff*s that are neither theorems nor countertheorems of the existing unformalized postulate set, and would remain so were it formalized and axiomatized so as to be writeable as a Ramsey Sentence.

Let me indicate, by subscripting on the modified theory's symbol, which postulates of Omniscient Jones's theory have been modified (from now on I shall use the term 'modified' to include the case of being deleted). Thus, T_i differs from the true theory T_{OJ} by having altered Postulate *i* in T_{OJ}; T_j alters only Postulate *j* in the true theory; and T_{ij} alters both Postulate *i* and Postulate *j* of O.J.'s theory. If a postulate, say Postulate *k*, is *not* subscripted, as in T_{ij}, this means that Postulate *k* has been left intact as it was in T_{OJ}. Clearly distinguishing verisimilitude from predictive or manipulative success—we must not pack the latter into our ontological concept, begging the question we are here examining—we can come to one intuitively strong definition of verisimilitude that permits an ordination of theories, despite the absence to date of an adequate quantitative—let alone metricized—explication of the verisimilitude concept. That persuasive one is this, where the inequality sign designates pairwise comparison of verisimilitude:

$$T_{\mathrm{OJ}} > T_i > T_{ij}$$

It is intuitively compelling that T_{ij} has suffered a greater departure from verisimilitude than T_i. A postulate set erroneous in two respects is less verisimilar than a postulate set erroneous with respect to only one *of the two*.

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Proof that Verisimilitude Correlates with Factual Fit

Every derivation chain in T_{OJ} that contains Postulate *i* will now give an incorrect result in the lab. Employing a crude "box score" of experimental successes and failures as our measure of corroboration, T_i will necessarily have a worse track record than T_{OJ}. Now we proceed to reduce the verisimilitude of T_i by modifying Postulate *j*, giving us a theory T_{ij} still further removed from the true one. By our non-metrical ordination, T_{ij} ranks worse in verisimilitude than T_i, and it ranks worse in verisimilitude than T_j. How does it do with the experimental facts? All of the derivation chains that contain Postulate *i* alone will now go awry in the lab; ditto the derivation chains that contain *j* alone. Unless we were so unlucky as to have committed and not noted a formal undoing (unlucky for a scientific realist, lucky for an instrumentalist!) the box score of T_{ij} will be worse than that of T_i. Denote by *n* the number of derivation chains terminating in those theoretical *wff*s required to correspond to all experimental *wff*s, and by a subscript *i, j, k*… which postulate we are examining; so that n_i is the number of derivation chains containing Postulate *i*, and n_{ij} is the number containing both Postulates *i* and *j*. Then the only way we could get an undesired reversal of rank between verisimilitude and corroboration box score is the condition

$$n_i > n_{i \vee j} = n_i + n_j - n_{ij}$$

$$n_{ij} > n_j$$

which is impossible. So the only way to get a reversal in this case is a formal undoing between P_i and P_j in one or more derivation chains. Obviously such a thing is mathematically possible, and could empirically happen (although quite rarely) unless we forbid formal undoings as a methodological rule. There is no way to compute the likelihood of such a thing happening, or the proportion of all postulate pairs in which it would happen in any real theory, because what determines its occurrence is the *empirical pervasity* of a given postulate. Given the total of all theoretical derivation chains needed that terminate in theorems that correspond operationally to the whole set of verified observational *wff*s, in how many of these chains does a particular postulate play a role? A "core postulate," such as the principle of reinforcement in Skinner, or Newton's second law in mechanics, or the law of economic equilibrium where supply and demand curves intersect, would be completely pervasive. Its distortion, departing from T_{OJ}, would result in every experiment coming out wrong. But if we had two postulates, each of which was core or quasi-core, n_{ij}

could easily exceed the sum $(n_{i\bar{j}} + n_{j\bar{i}})$, and consequently we would have a reversal of rank ordering between verisimilitude and box score.

What does the possibility of such reversals, due to an undetected (or detected but tolerated) formal undoing, imply as to the stochastic relation between corroboration and verisimilitude? Since we have no intentions to establish a metric for either concept, our concern is with a stochastic ordination. Is Lakatos right in saying that there is *no rational ground at all* for expecting ranked verisimilitude, strongly defined as T_{ij} worse than T_i or T_j, to be correlated with corroboration? In what follows I use the crude "box score" of empirical results, a simple tally of replicable experimental outcomes, over all experimental arrangements, expressed as a proportion of correctly predicted results. I bypass curve-fitting and significance test questions (Meehl, 1978a, 1990a, 1990c) to simplify the argument. So each theoretical *wff* that terminates a derivation chain matches an observational *wff*, considered classified as either true or false. Next we employ the old Spearman rank difference correlation coefficient ρ [= rho] (Hays, 1973; Siegel, 1956), which specifies nothing about the metric. In fact it "throws away" metrical information, although it is known to have a certain relationship to the Pearson *r* which is treated as metrical when the measures warrant it. Like the Pearson *r*, Spearman's ρ goes from zero (no relation) to one (perfect agreement of rank orderings). We consider all of the ordinations that can be made over the postulate set by starting with T_{OJ} and then writing successively less verisimilar theories T_{ij}, T_{ijk}, $T_{ijkl}\ldots$, etc. What is a representative or expected value in any one of those ordinations? (I don't know how to compute it over the batch, because they are not comparable, that is, they cannot be mixed. On our strong rule for verisimilitude ordination, we cannot compare T_{ijk} with T_{ilm}.) If over the whole batch of such combinations of modified postulates the statistical proportion of undetected (or allowed) formal undoings is *p*, with *N* ranks of theory verisimilitude (corresponding to an *N*-postulate theory) the expected value of Spearman's ρ is

$$\rho = 1 - \frac{12p}{N(N+1)}$$

Suppose T_{OJ} is a 10-postulate theory and the incidence of undetected formal undoings were as high as 1 in 10. Even in that awful scenario, the expected rank correlation between box score and verisimilitude is $\rho = .99$, a reassuring result.

A less intuitively compelling (some would say objectionable) ordination of modified theories permits us to rank T_i versus T_{jk}, where there is no overlap in the postulates altered but T_{jk} has more altered postulates than T_i (two rather than one). Because of the differences in pervasity or central—ity among postulates, one would properly hesitate to allow such a broad basis for ranking veri—similitude. But consider an extreme case. Suppose we omit consideration of ubiquitous postulates, those so central or core as to appear in every derivation chain. Comparing a theory T_i that modifies one non-ubiquitous postulate with theory $T_{jklm\ldots}$ that modifies *every other* postulate in T_{OJ}, $T_i > T_{jklm}$ seems quite compelling as a meaningful ranking. If none of the postulates being considered is ubiquitous, although they vary somewhat in their centrality or pervasiveness, it does seem that a theory differing from T_{OJ} in only one respect would, in some average sense, be thought of as more verisimilar than one that modifies, say, all but one of the non-core postulates of T_{OJ}.

Permitting this basis for rank ordering verisimilitude, can anything be said in general terms about the resulting box score for such comparisons? It turns out, surprisingly, that something can be said without quantifying the distribution of pervasities among postulates. We want to compare the experimental box score of T_i with that of T_{jk}. An (undesired) rank reversal between verisimilitude and corroboration occurs if the number of T_{OJ}'s derivation chains that fail in T_i exceeds the num—ber that fail in T_{jk}. This could surely happen logically, and, one presumes, psychologically in the scientist's theorizing. How often it happens depends on subsets of postulates' pervasity and the frequency of their being derivationally correlated, these relations being dependent jointly (in a complicated way, as drawing a few diagrams abundantly shows) on experimental pervasity and postulate *overlap* of theoretical terms. However, if the T_i/T_{jk} verisimilitude reverses corroboration rank order, it is impossible that either T_j/T_{ik} or T_k/T_{ij} do so, shown easily thus:

Let n_i be the number of derivation chains containing Postulate *i*, $n_{j \vee k}$ the number containing Postulates *j* or *k* (the wedge being the non-exclusive *or*), and n_{jk} the number containing both. Denote by $n(T_i)$, $n(T_{jk})$, $n(T_{ikl})$, etc., the number of derivation chains (valid in T_{OJ}) that fail in each of the parenthetical theories, their subscripts indicating which postulates of T_{OJ} have been altered (distorted or deleted). If formal undoings are forbidden, the number of failed chains in degraded theory T_{jk} will be the number containing Postulates P_j or P_k or both $= n_{j \vee k}$. If only one postulate P_i is altered, $n(T_i) = n_i$, the number of chains which involve it essentially. Ordering theories T_i and T_{jk} as to the looser definition of verisimilitude,

$$T_i > T_{jk}$$

and the number of failed derivation chains being

$$n_i = n(T_i)$$

$$n_{jk} = n(T_{jk})$$

the condition for a corroboration/verisimilitude reversal in ranking is

$$n(T_i) > n(T_{jk})$$

$$n_i > n_{j \vee k} \qquad \text{[No undoings]}$$

$$n_i > n_j + n_k - n_{jk} \tag{1}$$

Similarly, for theories T_j and T_{ik} we have the rank-reversal condition

$$n_j > n_{i \vee k} = n_i + n_k - n_{ik}$$

$$n_j > n_i + n_k - n_{ik} \tag{2}$$

Adding [1] and [2],

$$n_i + n_j > n_j + n_i + 2n_k - n_{jk} - n_{ik} \tag{3}$$

and subtracting $(n_i + n_j)$ we obtain

$$0 > 2n_k - n_{jk} - n_{ik} \tag{4}$$

$$n_{jk} + n_{ik} > 2n_k \tag{5}$$

But $n_{jk} \leq n_k$ and $n_{ik} \leq n_k$, so

$$n_{jk} + n_{ik} \leq 2n_k \tag{6}$$

which contradicts [5]. Similarly $n_k > n_{i \vee j}$ is shown to be impossible. Imagining this worst scenario as distributed over all of the ordinations (which now will collapse into a smaller number of ordinations since we can rank any altered set T_i, T_{jk}, T_{lmn} by counting the *number* of altered postulates, paying no attention to which ones they are), not more than one third of the ordinations can show rank reversals with respect to any triad of postulates. If we plug the value $\frac{1}{3}$ into Spearman's formula, we get

$$\rho = 1 - \frac{6\left(\frac{N}{3}\right)(1^2 + 1^2)}{N(N^2 - 1)}$$

which for a 10-postulate theory yields a correlation of $\rho = .996$ between box score and verisimilitude. Even if all the possible $\frac{1}{3}$ reversals were adjacent in the *T*-series, so the summed squares of rank differences were $(2^2 + 2^2)$, the correlation would be $\rho = .84$, a quite satisfactory value.

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Are These Correlations Too Good to Be True?

When we examine a series of theories obtained by striking or altering postulates from the T_{OJ} theory, and consider the effect of these alterations on the box score of experimental tests, the Spearman rank correlation seems too good to be true, being in the .90s. Does the metatheory prove too much, so that it can't be correct? A critic says, "The history of science shows that it doesn't go that smoothly. Zeroing in on the right theory doesn't happen as fast as it should, according to this meta—theoretical reconstruction." That is a problem for me. But there are ten answers to it, and they are not inconsistent, so that collectively they may constitute a satisfactory reply.

1. The biggest difference between my rank-order statisticizing and the scientist's cognitive situation is, of course, that the box score "hit"-rates for the array of theories, with which their verisimilitude rankings are objectively correlated, are rates computed over the gigantic collective of experimental facts. This collective includes experiments that will never be performed before the sun burns out, since it has to cover tests of all observational *wff*s, to support my rank statistics argument. Of course a subset can provide an estimate of the exhaustive rank-correlation, and that subset's error of estimation depends not on the ratio $n_{\text{(performed)}}$:$n_{\text{(formulable)}}$ but only on the size of $n_{\text{(performed)}}$, as in public opinion polling. An animal psychologist may know the results in several thousand studies of rats in the Skinner box (he will have trouble processing that many without formal meta-analysis!), but even this huge collection represents a small subset of the *possible* experimental arrangements. In fact, the known set is a small minority of all the *actual* experiments that psychologists will perform before humanity destroys itself. So the theorist's appraisal, even if made optimally on the available facts, would be based on a small subset of the observation language *wff*s that were idealized in our rank correlation argument. Reflection on this crucial point leads one to further skepticism about the "infinitely many explanations" commonplace. When we examine the processes of fact collection and reasoning as they took place in a particular scientific episode, we see how chancy, murky, and derailed the line of development may be. But when we contemplate the collection of facts in the late, "settled" stage of a domain, when almost all of the ongoing empirical activity is of Kuhn's puzzle solving kind, we get a different picture of the fact-theory relationship. (The example from genetics, *infra* pages 24ff, shows this nicely.) Even if all the known facts were taken as unproblematic, empirical science is difficult because there are known theoretical options. The facts known at this time do not suffice to exclude all but one option; we also believe (rightly, *at this stage*) that there are conceivable theories that no one has concocted; and we know (for sure) that there are many facts presently undis—covered. My rank correlations obtain strictly only for the utopian situation where the first and third of these sources of underdetermination are absent.

2. The scientist cannot begin with T_{OJ} as we have done here. He concocts a theory that falls somewhere else in the gigantic collection of T_{OJ} modifications T_{ij}, T_{ijk}, T_{klm}, etc. If he has bad luck" in the set of experiments done by a given time, or has started with a theory $T_{ijklm\ldots}$ far removed from T_{OJ}, the correlation we have computed does not help him much.

3. If metatheory is defined as it is today, appeal to the history of science should be *statistical* rather than *anecdotal*. Absent cliometric research, especially on minitheories (which abound in all sciences, especially the biological sciences, and which do not flow from a unified Big Theory," but are required to be consistent with it), one simply does not know how "fast" or "sure" the process is of zeroing in on the correct theory as experiments accumulate. I think that the young generation of philosophers of science are not consistent unless they do cliometrics, since all sides admit that methodological principles are guidelines and not strict rules, the reason for this being the stochastic character of empirical testing (Meehl, 1990c).

4. Just as contemporary philosophers and historians don't proceed cliometrically, so the scientists they study did not proceed cliometrically in assessing theories. That isn't as bad in the physical sciences as it is in the social sciences (Meehl, 1990c), but we have no grounds for saying that meta-analysis is *inappropriate* for complicated problems in physics, chemistry, and the biological sciences without trying it. Given that doubt, it is likely that there have been shoddy misevaluations and failures to converge rapidly, especially when received doctrine was in trouble. *Point*: Just as *we* might do better examining the history of science if we proceeded cliometrically, perhaps science itself would have evolved closer to optimally *had the scientists proceeded more cliometrically*.

5. The Spearman calculations are based upon consideration of the whole set of altered theories, with various combinations of postulates altered; that is a huge number of sets, each of them likely to generate additional derivation chains. If the postulate set is consistent, we cannot eliminate a valid derivation chain based upon a subset of postulates by adding an additional one to the set, and we will in general, though not always, tend to increase valid derivation chains by such an addition. This depends upon

the inner structure of the postulates, i.e., the overlapping of the terms in the embedding text, and the overlapping of the variables in the formalism. There is no reason for assuming that in sampling from that domain (subject to sampling error) investigators have proceeded in an optimal way. The strongest way would be Popperian, in which we juxtapose competing theories with different altered postulates (a fork in the road). A random entry is less optimal than this. A biased entry, influenced by "nonrational" considerations (what apparatus is available, what sub-area of facts the researchers feels familiar with and has been reinforced for studying, how much tax money is available, the biases of peer review committees, etc.), will move us away from randomness. Thus a random selection is somewhere between an optimal Popperian choice of what to study and the empirically realized choice based upon financial, psychological, ideological, and other nonrational considerations operating on the scientist.

6. My entire development here is based upon the conjectures of function forms in the mathematics, which is required for being able to say that if the postulate is distorted, the experiment will fail. That is only true when the mathematics is reversible, otherwise we would be committing the formal fallacy of denying the antecedent. Many theories, especially in their early stages, are not strong enough to do this.

7. I assume replicability throughout. Even in the physical sciences, problems of comparability and replication continually arise, particularly in the early stages of studying a theory. As of this writing, for instance, the notion of a fifth force based upon the baryon number is of tremendous theoretical significance, being close to a scientific revolution if true, but we do not know whether to trust the experiments or not.

8. My development says nothing about the embedding text associated with a particular function form stated in the formalism. If, for example, a postulate conjectures a logarithmic function between an input variable x and a theoretical variable u, that is taken to be the substance of the theory. Very few theories are so lacking in embedding text of the interpretive kind. The only kind of embedding text that my reconstruction includes is the operational text. It does not include the interpretive text. It is obvious that a scientist could be right about a postulate involving a logarithm or a power function, and wrong in the embedding text that interprets the theoretical variable or the causal character of the linkage.

9. I have said nothing about the role of problematic auxiliaries. Even in the advanced sciences problematic auxiliaries is probably the biggest single factor, *even given perfect replication*, producing interpretive ambiguity. Whether that could be subsumed under my development by counting all of the auxiliaries as part of T_{OJ} remains to be seen.

10. My development begins with T_{OJ} and moves away from it by altering postulates one at a time. I have not distinguished the cases of "altering" a postulate by changing its functional form versus deleting that postulate. And what happens if one modifies T_{OJ} by adjoining a postulate to it which is false? It isn't clear what it means to modify a postulate versus replace it, although probably a useful distinction could be worked out by a better logician than I am. We might begin by letting 'modify' refer to an alteration in the mathematical dependencies among the same subset of theoretical variables, where 'the same' means that a certain variable u recurs in other postulates, and this postulate relates u to some input x or output y or other theoretical term θ. I am unclear about how the *ceteris paribus* clause would fit into this analysis.

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Have We Surreptitiously Fused Ontology and Epistemology?

It may seem that after having emphasized the sharp ontological/epistemological distinction between verisimilitude and corroboration (as indexed by crude factual fit), I then proceed to conflate them, ordering theories' verisimilitudes by a concept of pervasity that "counts facts." A critic might say, "You set yourself the difficult task of refuting Lakatos's 'sheer leap of faith' thesis by a metaproof that verisimilitude and corroboration must be correlated. But then you made it easy for yourself by packing an index of the latter into your explication of the former. While not quite a *petitio*, it smells of circularity." I could reply that despite the value of distinct conceptions for certain analytic purposes, one consensual thesis of post-positivist philosophy is that in a deep ineluctable way, epistemology and ontology are inextricable; but while I believe that, I won't take that quick way out abstractly, but instead will exemplify it concretely. We did not *define* postulate pervasity in terms of factual "hits" (experiments that pan out), but in terms of *wff*s derivable. It then follows that a theory erring in a highly pervasive postulate will (stochastically) have a poorer factual track record than one defective in a less pervasive postulate. Of course it is unavoidable (and unexceptionable) that we demarcate a *domain* of a theory's interest. Our topic is empirical theories, theories about the external world, about facts. We cannot even get the discussion going with—out stipulating a broad factual domain, the regions or aspects of the world that a given theory purports to be about. It makes no sense to discuss the verisimilitude of "Freud's theory of rat learning," or "Skinner's theory of carcinogenesis," because—epistemology aside—there are no such theories, the domain of entities has been misidentified. It is nonsense to ask how Clark–Hull and E. C. Tolman stack up in the *ontological* domain of quasars, elasticity, anaphylaxis, or covalent bonds. So I admit to nothing viciously circular in my derivation. Another way of putting it is that although post-positivist metatheory is the empirical theory of scientific theories, on my conception it aims to *explain* scientific success, to show *why* one should *rationally expect* science, proceeding in such-and-such ways, to do as well as it does. When we explain, we derive, we deduce from something to something else. In all developed empirical sciences, we use logic and mathematics in making explanatory derivations without apology. It goes without saying that in using "logic" we have a semantics, and in using mathematics we include probability theory. I have heard no good (or bad) reason offered why empirical metatheory should deprive itself of these standard tools in explaining its explananda. So *of course* we expect that a valid derivation of a metatheoretical thesis will—to employ a positivist term—be "tautological." (*Cf.* Reichenbach's comment on his justification of the straight rule, "*Although the inductive inference is not a tautology, the proof that it leads to the best posit is based on tautologies only*" [1938, p. 359, italics in original]).

If these replies fail to dispel the aura of circularity, or inconsistent conflation of ontology and epistemology, I offer the following: Consider the set of theoretical *wff*s derivable from T_{OJ}, regardless of whether a theorem has an observational correlate or not. In any science, a proper subset of theoretical *wff*s are "operationally linked" to observation statements. A large part of scientific effort and ingenuity goes into devising clever experiments for accessing a theoretical theorem that had appeared to be a kind of "epistemological dangler," not capable of fairly direct observational test (e.g., Reines and Cowan detecting the neutrino 25 years after Pauli's theory-based—and ad hoc!—conjecture). Redefine the *theoretical pervasity* of a postulate as the number of theoretical *wff*s (operationalized or not) whose derivation chain contains P_i. So now we are clear of any smuggled-in allusion to the observational facts. Then define the *observational pervasity* of P_i as we did above, in terms of the set of factual *wff*s. What is the expected correlation between theoretical and observational pervasity? Consider a 10-postulate theory T_{OJ} whose postulates suffice to derive all 1000 true *wff*s of the observational domain. The hard core consists of five ubiquitous postulates $P_1 \ldots P_5$, each of which occurs in all 1000 derivation chains. More "peripheral" $P_6 \ldots P_{100}$ postulates have factual pervasities 900, 700, 400, 100, and 50 (i.e., Postulate 10 deals with only 5% of the experimental designs). Let the *excess* [= *e*] of non-observational over observational *wff*s vary from $e = .05$ to $e = .50$, by equal steps of $\Delta e = .05$. Thus if the theoretical excess of Postulate 8 is .30, and it occurs in 400 of the observational derivation chains, then it occurs in (1.30)(400) = 520 theoretical danglers, theorems lacking *any* "direct" observational test. I trust readers will agree that these quantitative values represent a very bad scenario, one hardly conceivable in any science. Lacking any rational basis for saying how theoretical excess will relate to theoretical pervasity (I haven't an intuitive hunch even about whether the relationship would be positive or negative), I have made random assignments of excess to pervasity, done repeatedly to generate 25 Pearson correlation coefficients. The correlations range from .96 to .99, with a mean = .972. (Even with perfect rank ordering, $r < 1.00$ due to skewness.) "But that's simply due to overlap of variables (x, $x + 1.ex$), such as happens when we correlate the positions of baseball teams halfway through a season and at its end." Of course. That is an algebraic consequence of the definitions of theoretical and observational pervasity. Given these numerical results assuming a preposterously bad scenario, I conclude that a fact-free definition of theoretical pervasity "works" as well as one in terms of observational *wff*s, hence no circularity problem.

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Crudity of the Two Indexes

Presupposing a conception of scientific theories that is realist with nomic necessity, it is reassuring to find that a stochastic linkage can be proved to obtain between verisimilitude (ordinating theories by their degradation from Omniscient Jones's theory) and factual track record (numerified by proportion of replicated observational *wff*s theoretically derivable). I need hardly say that neither of these crude numerical indexes can be considered adequate explications of the intuitive concepts *verisimilitude* and *empirical performance*. As to the former, scientists and metatheorists have more complex internal properties of theories in mind; as to the latter, there are other important performance features than a simple head-count of successful experiments. Even the experi—mental hit rate is defective as an index of factual fit, because it gives no positive value to near misses, contrary to scientific practice; and it fails to incorporate the factor of *theoretical intolerance*, how detailed the prediction was (nonphilosophical scientist speaking), or how risky (Popperian), how much of a damn strange coincidence (Salmon). I have made some tentative suggestions along those lines in another paper (Meehl, 1990c).

In that same paper I offer an explication of verisimilitude that is, I submit, more like the way scientists think about "nearness to the truth" than the explications provided thus far by logicians (e.g., Goldstick & O'Neill, 1988; Hilpinen, 1976; Kelly & Glymour, 1989; Newton-Smith, 1981; Niiniluoto, 1984, 1987; Oddie, 1986; Popper, 1962, Chap. 10 and *Addenda*, 1972, Chapters 2, 3, and 9, 1976, 1983; Tichý, 1978; Tuomela, 1978b). My suggestion is summarized in Figure 1. The similitude of theory T_2 to theory T_1 depends on how closely they correspond in features I–X. Then if T_1 is OJ's theory, the similitude of T_2 to T_1 [= T_{OJ}] is T_2's verisimilitude. Most of the later questions of resemblance are foreclosed by those higher (earlier) in the list being answered negatively, so the items are almost "lexicographically ordered" (Rawls, 1971) or, to use my profession's jargon, "Guttman scalable" (Guttman, 1944; Loevinger, 1947). Example: If a postulate asserts a causal influence between theoretical variables ($\theta_1 \xrightarrow{c} \theta_2$), and there is no such relation (falsity at Level II), we do not ask (Level III) about the signs of $\frac{d\theta_1}{d\theta_2}$, a meaningless question. Or if T_1 correctly states that θ_2 depends on θ_1 with (+) first derivative and (–) second derivative, but wrongly makes the function logarithmic when it is in reality a growth function (error at Level VI), one cannot meaningfully ask about the parameters (Levels VIII–X). The overall verisimilitude of a theory would be indexed by a composite of these levels for all of its postulates, presumably weighted by the postulates' pervasities.

Theory Specifications

I. Type of entity postulated (substance, structure, event, state, disposition, field)

II. Compositional, developmental, or efficient-causal connections between the entities in **I**

III. Signs of first derivatives of functional dynamic laws in **II**

IV. Signs of second derivatives of functional dynamic laws in **II**

V. Ordering relationships among the derivatives in **II**

VI. Signs of mixed second order partial derivatives (Fisher "interactions") in **II**

VII. Function forms (e. g., linear? logarithmic? exponential?) in **II**

VIII. Trans-situationality of parameters in b

IX. Quantitative relations among parameters in **VII**

X. Numerical values of parameters in **VII**

Figure 1. Progressively stronger specifications in comparing two theories (similitude). (Adapted from an earlier version published in Meehl, 1990c.)

The construction of composite indexes is one of the most obscure (and, except for economists, least discussed) topics in social science. We have a set of numerical indicators x_1, x_2,$\ldots x_m$. We wish to construct a function $X_C = F(x_1, x_2, \ldots x_m)$ that combines the x's "optimally," or, at least, "satisficedly."

How do we go about this index concoction? It all depends on the *theoretical framework* and the *aim*. Until we formulate these prior commitments, no help is forthcoming from the statistician or mathematician. If we are engaged in an effort to numerify a general intelligence factor *G* underlying individual differences in cognitive functioning, the statistical model will differ from that of an industrial psychologist whose pragmatic context is combining a set of test scores to predict job performance in a factory. The index problem faced by the welfare economist in defining a Consumer Price Index is again different. Different still is the sociologist's problem constructing an index of social class (Hollingshead & Redlich 1958, Appendix 2, pp. 387–397). Thus, for instance, the psychologist interested in tapping the heritable component of *G* will have in mind, however vaguely, some biological property of the brain; no such unidimensional physical variable is conjectured by the sociologist's index of SES.

Given a set of numerified components of verisimilitude ($x_1, x_2, \ldots x_m$), and of theory performance ($y_1, y_2, \ldots y_m$), and absent a rational basis for weighting the variables in each set, we might get some help from the industrial psychologist. Treating the verisimilitude *x*-variables as "input" (analogous to a mental test battery) and the track-record *y*-variables as "output" (analogous to multiple aspects of job performance), we could weight the two sets by the method of *canonical correlation*. We define an input composite $\widehat{x} = \beta_1 x_1 + \beta_2 x_2 + \ldots \beta_m x_m$ and an output composite $\widehat{y} = \gamma_1 x_1 + \gamma_2 x_2 + \ldots \gamma_n x_n$. We adopt the intuitively plausible convention to weight aspects of verisimilitude and aspects of track record so that the two will be as closely related as possible, i.e., that $r_{\widehat{x}\widehat{y}} \rightarrow \text{MAX}$. This is done by minimizing the quantity

$$S = \sum^{N} (\widehat{y} - \widehat{x})^2 \longrightarrow \text{MIN}$$

where *N* is the number of theories under study. The sum of squared "errors" expands to terms in β_i^2, γ_i^2, and cross-products β_i^2, γ_j^2. Differentiating *S* wrt the β's and γ's, setting these partial derivatives $= 0$ for a minimum,

$$\begin{array}{ll} \frac{\partial S}{\partial \beta_1} = 0 & \frac{\partial S}{\partial \gamma_1} = 0 \\ \frac{\partial S}{\partial \beta_2} = 0 & \frac{\partial S}{\partial \gamma_2} = 0 \\ \vdots & \vdots \\ \frac{\partial S}{\partial \beta m} = 0 & \frac{\partial S}{\partial \gamma_n} = 0 \end{array}$$

the β^2 terms drop to 1st degree in β, the γ^2 to 1st degree in γ, the cross-product terms β_i^2, γ_j^2 to first degree in the non-differentiated variable. So we have $(m + n)$ linear equations in $(m + n)$ unknowns, which we solve. The result of this process is an output composite that is more predictable from the (best-weighted) input composite than any *other* output-composite would be from *its best* input-composite. Whether this explicates our intuitions about verisimilitude I do not here consider. A more thorough consideration of the terrible problem of index numbers can be found in my forthcoming paper on cliometric metatheory.

What are the raw empirical data that go into these solutions? A first try proposal: In a specified scientific domain (e.g., biochemistry, mammalian learning, macro-economics) we sample all minitheories that are, say, 50 years old. In their original formulation, some have been consigned to oblivion; a few are enshrined in textbooks, wrongly called "facts" rather than "theories"; others fall between these extremes, having been amended and then accepted. We can compute the theory-property indexes and the empirical track-record indexes for each such minitheory, treating the status of each postulate as known. From these indexes we compute the canonical correlation. This procedure does not assume that *all* of the truth-values relied on are correct, but only that *most* of them are, and the rest *nearly so*. Statistical analogies: Intelligence tests were initially "validated" against the fallible criterion of cumulative teacher judgments, now supplant them; ditto for personality tests against clinical ratings and psychiatric classification; ditto for biochemical indicators against physicians' diagnoses in organic medicine (*cf.* discussion of the "bootstrap effect" in Cronbach & Meehl, 1955; Meehl & Golden, 1982). *It may be that metatheorists must learn to think like clinical and industrial psychologists.*

In suggesting canonical correlation do I conflate ontology and epistemology despite my contrary intention? I hope and believe not. If the 10 levels of similitude in Figure 1 *could* be shown to possess nonarbitrary weights, somehow conceptually intrinsic to them, those would be the "correct" weights in a composite index of truthlikeness. But I cannot imagine how such a metric would be arrived at. A rank-ordering, almost, yes. To theorize the wrong sorts of entities (Level I) is "worse" than to be a bit off in the numerical value of one of a single postulate's parameters (Level X). But this lexical quasi-ordering will be reflected, however imperfectly, in any index that assigns value 0 to all levels beyond the first level a postulate fails to pass. This relation assures a representation of *level dominance* (importance, priority, privilege, value) in the empirical statistics. If—beyond that "automatized" accounting of privilege—we lack a *nonarbitrary* basis for weighting, it seems reasonable to add a whiff of instrumentalism, assigning *conventional* weights to the components of verisimilitude in terms of the theory's empirical success. (I find myself comfortable with this because of that deep sense in which we correspondence theorists are, willy-nilly, *also* coherentists and instrumentalists, as I think almost everyone agrees today. A simon-pure correspondence theorist demands coherence, even at times excluding candidate protocols from the corpus on Neurath-like grounds. The protocols, *in the long* run and *collectively*, control our acceptance of the theory; in the *short run*, an accepted theory may control admission of *individual* protocols. And I, a correspondence theorist and scientific realist, am not above invoking the technological impres—siveness of the moon shot as a "good reason for believing physics." The *definition* of 'True' is correspondence, but the *criteria* are a mix of coherence and pragmatic success. *Cf.* Rescher, 1973.) Let me repeat, if there *were* a nonarbitrary weighting of these 10 conceptually distinct components that flowed rigorously from their definitions and their relations, one should use it. Lacking that, and recognizing that all 10 are entitled to "be counted" as partial aspects of truthlikeness, weighting by contribution to track record seems acceptable. Fortunately, the *statistical algorithm* sees to it that the components' pairwise correlations (largely forced by the lexical ordering) and their individual contributions to performance are considered jointly in an optimal configural way.

Finally, emphasizing that the whole business is stochastic, let me reassure readers worried about the weights that it hardly matters anyway. A theorem of psychometrics due to Wilks (1938; and *cf.* Bloch & Moses, 1988; Burt, 1950; Dawes, 1979, 1988, chapter 10; Dawes & Corrigan, 1974; Einhorn & Hogarth, 1975; Gulliksen, 1950; Laughlin, 1978; Meehl, 1992; Richarson, 1941; Tukey, 1948; Wainer, 1976, 1978) shows that under rather weak conditions (e.g., positive manifold) the expected value of the Pearson r between an optimal and a random weighting of a linear composite is, to a good approximation, $\bar{r} \simeq 1 - \frac{1}{m}$, where m is the number of variables. So with our 10 verisimilitude variables, God's composite and even a whimsical, half-baked human weighting can be expected to correlate .90 or better. Since the canonical method will surely achieve more than that (as shown by my anti-Lakatos proofs even for two crude indexes), we may rationally hope for a correspondence in the middle or high .90s.

"But you obtained correlations in the .90s using the two crude indexes. Where is there room for improvement? And there *must* be improvement, given two refurbished indexes. Something's wrong here, surely." Answer: The crude ones were based on exhausting the humongous set of observational *wff*s. Given a utopian condition, that we may only improve from, e.g., $r = .95$ (crude indexes) to $r = .98$ (refined measures) should not amaze us. But—emphasizing again that the entire enterprise is stochastic—one expects that *early on* in the empirical appraisal of a theory, the verisimilitude-corroboration correlation will be considerably higher using the superior indexes. The random sampling error of the Pearson r decreases nonlinearly with its parameter value, and Fisher's $z_r = \frac{1}{2}\, log_e \frac{1+r}{1-r}$ is close to Gaussian with a standard error $SE_z = (N-3)^{-1/2}$. When we have conducted only 10 experiments, the track record at that point correlates with verisimilitude by an amount that can be expected to deviate from the fact-exhaustive correlation some 3.7 times as much as when 100 experiments are in, 12 times for 1000, and 37.8 times for the over 10,000 experiments that psychologists have run on white rats. Suppose the true correlation between verisimilitude and experimental hit rate over the complete set of experimental designs were $r = .95$ for a class of theories. If, in testing each theory, only 10 experiments from the fact domain were randomly chosen, the 95% confidence interval (within which the sample correlation has probability $p = .95$ of being found) has limits (.80, .99). The table shows the range limits for additional sample sizes of 100, 1000, and 10,000 experiments. It also shows the confidence belts that would obtain if the true r were only .84 (due to worst-scenario reversals of order, p. 12). The up-down asymmetry in error reduction arises, of course, from the nonlinear z_r-transformation.

		95% confidence interval	
		$r = .85$	$r = .95$
	m	LO HI	LO HI
Number	10	.47 – .96	.80 – .99
of	100	.78 – .90	.93 – .97
Experiments	1000	.83 – .87	.94 – .96
	10,000	.84 – .86	.95 – .95

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Infinitely Many Alternatives: A Logician's Truism But Not a Scientist's Problem

The rather strong rank-correlation results do not accord with the logician's truism that "You can always concoct an infinite number of different postulate sets for the purpose of deducing any specified set of theorems." When one queries a philosopher about this truism, one is told (in an offhand or even impatient way), "It's trivial, it's merely because, given any finite set of consistent *wff*s, there are infinitely many different postulate sets sufficient to deduce them." Lacking requisite expertise, I do not challange this assertion, let alone deny it. But it does seem odd that such an important theorem, which has such baneful implications for rationalizing empirical science (e.g., often considered fatal for Bayesians), is not to be found in Church, Copi, Quine, and Co., and is not distinguished by a name as are the Goedel, Church, or Loewenheim-Skolem theorems.

It is, of course, "obvious" in the propositional calculus. But we cannot analyze real science with that calculus' resources; it does not help us to get insight into the "inner structure" of interesting explanatory theories in science, as I have objected above. What happens in empirical science is that the postulates and definitions of a theory overlap in the predicates, functors, ranges of variables, even definite descriptions and proper names (*cf.* geography, paleontology, astronomy) that occur in them, such that, applying the transformation rules of logic and mathematics, one deduces further statements as consequences. Scientific explanations *satisfy* intellectually precisely because of this kind of entailment. You cannot see how science works if your smallest unit is the proposition, you must get at the "innards" of propositions to see that. What makes empirical science a *network* rather than a *heap* is this overlap of the statements' inner components, like Aristotle's syllogisms. If I propound a "theory" to explain observational statements r, s by saying "I postulate that $p, q, p \supset r, q \supset s$; then, relying on the transformation rules of the propositional calculus, I infer r,s," is any scientist going to take it seriously? Of course not. He wants to know why $p \supset r$, i.e., he wants a stronger "if…then…" connective than the horseshoe. The truism has to be formulated in richer terms to be relevant to explanatory theories. I repeat, I am not suggesting that no such metatheorem can be proved; but I want to see precisely what it says. I conjecture that such a proof will yield some interesting corollaries, more helpful than the most general theorem itself. I have in mind such matters as the following: What differences exist between postulate sets linking predicates, versus functors, versus mixtures of the two? If postulates P_{ij} and P_{jk} assert class inclusions and exclusions defined by predicates θ_i, θ_j, θ_k, a theorem t_{ik} relating θ_i to θ_k may be validly entailed, but it may not—there could be an Illicit Process or an Undistributed Middle. By contrast, if postulates P_{ij} and P_{jk} are mathematical functions relating theoretical functors θ_i to θ_j and θ_j to θ_k, these being well-behaved, we know that a theorem t_{ik} relating θ_i to θ_k follows, as in the Chain Rule example in the text.

Consider a 5-postulate theory which links theoretical functors θ_1, θ_2, and θ_3 via P_{12} and P_{23} entailing t_{13}; and links θ_4 to θ_5 via P_{45}. Assume the theory contains no definition of any theoretical variable θ_j in terms of the other five θs. Then the classes of θs involved in P_{12}, P_{13}, t_{13} and in P_{45} are disjoint, not capable of entailing or contradicting any statements about the first set with statements about the other. It is as if we had two unrelated theories, even if the total theory has been concocted to deal with a rationally demarcated observational domain (e.g., animal learning). If there are observational functors y_1, y_5 operationally linked only to θ_1 and θ_5, and an observational *wff* relating y_1 and y_5 (their compresence being physically possible and concurrently observable), then this *wff* is undecidable, so the total theory is empirically incomplete. I presume there must be many such structural corollaries of the general theorem, and that many of them would carry important methodological freight for the empirical sciences. I suspect that reflection on the corollaries. when combined with statistical trends from history of science, would suggest a number of "policy" prescriptions along with the avoidance of isolates, formal undoings, and hidden variables proposed in text *supra*. There would be whole classes of postulate sets that, despite deriving the theorems, are ruled out. I expect that many of these exclusions would be on grounds of formal structure, despite the theorem derivability and absence of contradiction, and that they would involve no reference to interpretive text (which is a second important source of constraint on the number of possible theories available to the working scientist, as will be discussed in the next section).

Example: Suppose the observed input-output relation is derivable from postulates that include

$$P_0\colon \theta = log_e x$$

Assume the input variable x ranges from 1 to 2 (physically, or the metric so standardized, as in the life sciences we form z scores, T scores, percentiles, and probits). We may rewrite

$$\theta = (x-1) - \frac{1}{2}(x-1)^2 + \frac{1}{3}(x-1)^3 - \frac{1}{4}(x-1)^4 + \ldots$$

which converges for $0 < x \leq 2$, although very slowly for the region near zero. For the empirically realized region $1 < x \leq 2$ the worst case is at $x = 2$, where the error due to dropping all terms after $n = 145$ is less than 1% of the exact value. Suppose the inaccuracy of our input measurements swamps this small error, and will continue to do so. (In psychology this can often be guaranteed by the observational metric itself, e.g., IQ scores or MMPI personality scores increment by integer steps). So we may take the expression

$$\theta = (x-1) - \frac{1}{2}(x-1)^2 + \frac{1}{3}(x-1)^3 - \frac{1}{4}(x-1)^4 + \ldots + \frac{1}{145}(x-1)^{145}$$

as adequate for representing θ in terms of the input variable. (Notice we write this *directly*, no further postulates or interpretation—an identity of the formalism, of Carnap's "general calculus.") But given this tautology, some crazy theorist *could* proceed by writing 146 postulates, as follows:

$$\begin{array}{ll}
P'_0: & \theta = \theta_1 - \theta_2 + \theta_3 - \theta_4 + \ldots + \theta_{145} \\
P_1: & \theta_1 = (x-1) \\
P_2: & \theta_2 = \frac{1}{2}(x-1)^2 \\
P_3: & \theta_3 = \frac{1}{3}(x-1)^3 \\
\vdots & \vdots \\
P_{145}: & \theta_{145} = \frac{1}{145}(x-1)^{145}
\end{array}$$

from which he derives old P_0 as a theorem. We now have an alternative postulate set for deriving the input-output formula. I suppose one could provide an "interpretation" of the formalism by adjoining some (weak) embedding text, "the stimulus input produces states in 145 cerebral subsystems whose mode of reactivity varies [the laws being $\theta_1 = (x-1)$, $\theta_2 = \frac{1}{2}(x-1)^2$, etc.]; these subsystems act upon a (scanning, summarizing) subsystem whose state-variable θ is determined by input received from the other 145 subsystems, some excitatory and some inhibitory, such that their net effect on state θ is given by the composition law

$$\theta = \theta_1 - \theta_2 + \theta_3 - \theta_4 + \ldots + \theta_{145}."$$

The whole thing is quite absurd, and no sane psychologist would do it except as a joke. But it meets the logician's requirements.

Analogous cases can arise for predicate theories. The usual characterization of theories in the predicate calculus is somewhat misleading, since powerful scientific theories are not expressed in terms of relations between class-defining predicates but almost always involve characterizing relationships in terms of mathematical functions that describe the kinetics and dynamics of change, or (as in some social sciences) cross-sectional correlations and their inferred latent mathematical structures (e.g., factor analysis, multidimensional scaling, path analysis, taxometrics). The logic text paradigm expressions such as (x) $(Px \supset Qx)$ are not illuminating for a science that largely consists of mathematical formulations of Aristotle's efficient causes, with a little taxonomy sometimes thrown in as groundwork (*cf.* Earman, 1986, pp. 83–84). Suppose we need to derive a theorem

$$(x)[\theta_P(x) \rightarrow \theta_Q(x)]$$

whose theoretical predicates θ_P, θ_Q are operationally linked to observable predicates P, Q that are factually related by

$$(x)[Px \rightarrow Qx]$$

We "explain" this P, Q relation, in the sense of derivability, by a sorites

$$(x)[Px \rightarrow \theta_1 x]$$

$$(x)[\theta_1 x \rightarrow \theta_2 x]$$

$$(x)[\theta_2 x \rightarrow \theta_3 x]$$

$$\vdots$$

$$(x)[\theta_m x \rightarrow \theta_{m+1} x]$$

$$\underline{(x)[\theta_{m+1} x \rightarrow Qx]}$$

$$\therefore\ (x)[Px \rightarrow Qx]$$

which sorites can of course be made as long or short as we please. Again, the logician's "derivability" is met, but the theorizing is ridiculous.

Some theories, especially in the life sciences, combine predicate and functor postulates. As mentioned above, pairs of predicate postulates cannot always validly decide a *wff* that selects the "wrong" pair of their class terms. Although pairs of well-behaved functor postulates are regularly fertile, in (slightly) more complicated theories, the quantitative theories often suffer from indeterminacy. *Example*: In psychological trait theory, correlations between several pairs of operational traits (e.g., tests of IQ and manual dexterity) are subjected to multiple factor analysis, which produces a (test × factor) matrix of factor loadings that, when multiplied by its transpose, yields the test correlation matrix, to a good approximation. But since an infinite number of positions of the reference vectors will achieve this "data-reduction" numerically, psychometricians face the *rotation problem*, whose solution cannot come from the mathematician. Hence further constraints of a methodological nature are imposed (e.g., Thurstone's "simple structure," maximizing the number of near-zero loadings, and other parsimonious properties), generalized by J. B. Carroll and others to a quantitative, computer-programmable criterion. More recently has appeared *confirmatory factor analysis* (Loehlin, 1987) which attempts to provide a Popperian risk. Whether it achieves this is still in dispute. But consider a theory that postulates real latent *classes* (taxa) that differ in their quantitative distributions of indicator variables *x, y, z*, etc. Making weak conjectures about those latent distributions ("smooth" rather than step-function or gappy, unimodal—but not necessarily Gaussian, symmetrical, or equal variances) one can derive enough nonequivalent theorems concerning the various manifest indicator relations to test the latent model and get trustworthy estimates of its parameters. I believe that here the theory is overdetermined by the facts (Golden, 1982; Golden &

Meehl, 1978, 1980; Meehl, 1973, 1979, 1990b; Meehl & Golden, 1982). A general analysis of mixed category/dimension situations should be illuminating, with corollaries concerning various special cases.

Some obvious *consistent* and *explanatory* but *inadmissible* cases would be having more postulates than theorems, more theoretical predicates than observables (of course, more individual *entities* is allowable, e.g., molecules), more untestable θ-*wff*s than testable ones (i.e., excessive nonoperational consequences, "epistemic danglers"). My hunch is that in order to prove interesting, scientifically relevant results, the logician will have to reduce generality, to "make cases" as we say in card game applications of probability theory—proofs of subtypes of postulate sets specified by conjoint properties. If formulations for the most general case are not helpful, that's all right, it's like the important cases where an *Entscheidungsproblem* algorithm exists despite Church's Theorem for the general case. I have so far had no luck in tempting my logician friends to work on this, but perhaps a few readers will be moved to do so.

While conjecturing that a valid proof of some form of the logician's "many alternative theories" will have interesting corollaries not foreseeable (at least by me), I think we can already say a few helpful things about classes of postulate sets that, while formally adequate to derive the operational theorems of a given empirical domain, would be either excludable or strongly disapproved on methodological grounds, prior to invoking Comte Pyramid considerations. As rules of thumb, "principles" or "policies" set by the aims of the scientific game, I proffer the following (not, I repeat, strict *rules*, but helpful metatheoretical advice to the working scientist):

1. If the domain permits, prefer functors over predicates.
2. Avoid isolates, prefer pervasity and greater "interknitting," e.g., multiple output forks, lateral (intratheoretical) linkages.
3. Avoid formal undoings.
4. Avoid hidden variables.
5. Prefer a larger observation/theory concept ratio.
6. Prefer a larger observation/theory statement ratio.
7. Prefer qualitative diversity over the observational domain.

When to this list we adjoin compatibility with accepted theories and facts above and below in Comte's Pyramid (see below), the logician's formal truism hardly seems relevant, or at least not dangerous. The viable candidate list has been greatly narrowed down, and we proceed to appraise the remaining contenders by such "factual track-record" criteria as Salmonian damn strange coincidence, novelty, freedom from Lakatos's three kinds of *ad hockery*, and even technological potency (e.g., genetic engineering, putting a man on the moon).

There is the perennial problem of scientific inference being formally in the invalid third figure of the implicative syllogism, that unpleasant fact that gave rise to Morris Raphael Cohen's joke: "All logic texts are divided into two parts. In the first part, on deductive inference, the fallacies are explained; in the second part, on inductive inference, they are committed." What amounts formally to affirming the consequent appears methodologically in the paradoxes of confirmation. For metatheorists attempting to "proceed Bayesian" (without offering to compute any actual numbers), such as Reichenbach, Salmon, and Grover Maxwell, there is the bothersome point about the second term in the denominator of Bayes's Formula. Because of the logician's truism, it is composed of a sum of infinitely many terms, and since none of them is infinitesimal, it would appear that whatever our experimental evidence we can't get the posterior probability up to a respectable value. Even for a quasi-Popperian who does not think you can probabilify an operation, but who is still relying (though not in a Bayesian way) upon Salmon's Principle that theories are best evidenced by making predictions which, absent the theory, would constitute a "Damn Strange Coincidence," such a Damn Strange Coincidence doesn't take us very far (with or without formally using Bayes's Theorem) when we focus our attention on the alleged infinity of theories that are in competition.

In a naturalized epistemology, or philosophy of science considered as *metatheory* (= the empirical theory of scientific theories; and including, of course, "rational" components within that enterprise, e.g., logic, set theory, general mathematics, and probability theory), it is consoling to remember that no matter how fast how many people theorize, they will only concoct a finite number of theories before the sun burns out. It is unclear how helpful that is, if one wants to say more about a theory than how it compares with its "actual and (humanly) possible competitors," but I set that aside. It does seem, from the foregoing analysis, that the number of strong, explicit, quantitative networks, as we specified their properties, is not clearly infinite, and may not even be very large. Ignoring the absurd case and *confining ourselves simply to the formalism of functional dependencies*, the explanatory postulate set may be unique; and it may consist of only a few alternatives even if one relaxes the rule against formal undoings.

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Theoretical Constraints in Comte's Pyramid of the Sciences

There is another important source of restriction on the number of theories the working scientist has to consider. One reason why we cannot completely Ramsify out all of the theoretical terms, liquidating the cognitive contribution of interpretive text, is that in the life sciences, biological or social, we theorize under constraints imposed by our discipline's position in a revised Comte Pyramid of the Sciences (Comte, 1830–42/1974, 1830–54/1983; Oldroyd, 1986). An exhaustive discussion of the varieties of reductionism and their relations (e.g., nomological reduction presupposes conceptual reduction, but is not entailed by it) is beyond the scope of this paper. We can note that what I call "compositional theories"—theories that assert what substances a given substance is composed of, or what substructures a given structure is built of, in what structural arrangements and with what causal relations—are frequently incapable of deriving completely the dispositions of the composite substance or complex structure, but nevertheless are capable of imposing constraints by virtue of the Pyramid of the Sciences. For example, it would be quite wrong to say that the whole process of mitosis is nomologically reducible to the laws of physics and chemistry, and no cytologist makes the grandiose claim to do so. Nevertheless, whatever microtheories or minitheories the cytologist invents to help understand the process, and perhaps quasi-derive certain portions of it, are constrained by his knowledge of the chemical composition of the living cell, mechanical constraints on the properties of the spindle fibers, laws of conservation of energy, and the like. A theory that required the spindle fibers to be tiny threads of copper wire wouldn't be acceptable even if it turned out to be capable of deriving characteristics of the mitotic process. I do not know whether the therapeutic action of the drug methotrexate against some carcinomas was predicted theoretically or not, but it "works" by inhibiting an enzyme that reduces folic acid, a step necessary in DNA replication, so the cells cannot mitose. However this idea came to be discovered by the biochemist or oncologist, there was not, realistically, "an infinite set of alternative postulates" available to explain the effect, and one could narrow the set down to the correct one in a hurry.

Suppose the psychologist finds an empirical growth function connecting input to output, and a decay function in extinction or unlearning a habit. He is greatly constrained by knowing in advance that these connections have to be formed in the brain (specifically, at the synapse); and that "input" is via a limited set of organs (the receptors). If his interpretive text motivating a growth function on conditioning and a decay function on extinction is to be explanatory, there aren't realistically many places to look. He would quickly discover that he doesn't get either kind of curve if he interpolates some of the "learning" or "extinction" trials in a totally different stimulus field. (Of course he wouldn't even try such a foolish thing, because he knows it wouldn't work.) So what are the possibilities? The objective stimulus situation may be fluctuating from trial to trial, leading to a mathematical description that involves the idea of sampling stimuli on successive occasions; or there may be variations in the orientation of the organism's head, or eyes, resulting in such a statistical sampling; these can be easily accentuated, diminished, or eliminated by experimental procedures. If we still get such a continuous process behaving as if there were "elements" being sampled and as if the proportion of new ones sampled per trial depended on how many were "left in the pot" (as in the Guthrie model), then some kind of connecting and disconnecting must be going on in the brain. One has some leeway about the size of the units—single neurons such as command neurons (Meehl, 1978b, 1978c, 1989), or modules, or module hierarchies—but the point is, the Pyramid of the Sciences, our knowledge of the brain's microstructure and its connection with the receptors as input and the muscles as output, does not permit us scientifically to contemplate "neural fluid" (or libido) flowing unless we consider it a metaphor or analogy, because there aren't any tubes or pipes or any such substance as libido circulating in the head (MacCorquodale & Meehl, 1948).

That an "infinite number of alternative explanations" is not realistic for an empirical science above physics in the Pyramid can be seen dramatically in this quote from Snyder's *Principles of Heredity*, published two years before Watson and Crick's DNA breakthrough:

> In the preceding chapters we have from time to time found parallels in the behavior of genes as seen in the results of breeding and the behavior of chromosomes as seen under the microscope. It may be well at this point in our study to review and summarize these parallels and to place them in their proper perspective in the general proof of the hypothesis that the genes are actually carried in the chromosomes.
>
> If we reflect upon the general facts of reproduction, we find it possible to state a series of principles and inferences, in which the parallels which we have been discussing find their place. Let us list them in logical order.
>
> **I. Genes are carried in the sperms or the eggs or both, since only these bridge the gap between generations.** Although it is true that in some species the embryo develops within the body of the mother, so that it might be conceived that genes could be received from the mother in some other manner than through the egg, yet it is equally true that in other species the sperms and eggs are shed from the parents, subsequently uniting and developing independently of both parents. Moreover, the principles of heredity appear to be the same for all species.
>
> **II. In general, within a species, the sperm and the egg contribute equally to the inheritance of genes.** The evidence for this statement lies in the results of reciprocal crosses, where the F_1 is usually identical, whichever way the cross is made. Both the sperm and the egg, therefore, carry genes. There are certain exceptions to this rule, however. We have already discovered that the sex chromosome of the sperm may be different from that of the egg. Moreover, either a sperm or an egg may carry a chromosomal aberration making it different from the other. Both of these exceptions serve only to strengthen the hypothesis that Mendelian genes are carried in the chromosomes, however, since genetic and cytological parallels exist for them, as will be pointed out in principle **V**, below.
>
> One more exception must be mentioned. In recent years evidence has been accumulating that the results of reciprocal crosses within a species are not always identical and that there is for some characters a maternal, hence cytoplasmic, transmission. This exception will be considered in detail in the next chapter. The fact remains, however, that both the sperm and the egg carry Mendelian genes.
>
> **III. Although the egg has a relatively large amount of cytoplasm in addition to a nucleus, the sperm is practically all nucleus.** Such little cytoplasm as the sperm contains is left out of the egg when the sperm enters, and only the nuclei unite in the actual fertilization. It would appear, then, that the nucleus is the essential part of the gamete in regard to the transmission of the genes.
>
> **IV. Of the nuclear constituents, only the chromatin material appears to be accurately divided at mitosis and segregated during maturation.** Moreover, the chromatin material is formed into chromosomes with a constant and characteristic number and appearance for each species. The other constituents of the cell do not appear to undergo the same quantitatively accurate division and segregation.
>
> **V. A striking series of parallels occurs between the behavior of genes as seen in the results of breeding and the behavior of chromosomes as seen under the microscope.** The parallels are as follows:
>
> 1. Genes normally occur in pairs in the cells of the individual. *So do chromosomes*.
>
> 2. The two genes of each pair segregate in the formation of the germ cells. *The two chromosomes of each pair also segregate in the formation of germ cells*.
>
> 3. Certain genes assort at random. *Likewise certain parts of the chromatin material (that is, whole chromosomes) assort at random*.
>
> 4. Certain genes behave as though only one member of the pair were present in one sex (sex-linked genes). *Similarly only one member (or at least only one normal member) of one pair of chromosomes is present in the corresponding sex*.

5. Certain genes do not assort at random but occur in paired groups (linkage groups) which tend to be transmitted as units. *The chromatin material is also gathered into paired groups (chromosomes) which tend to be transmitted as units*.

6. The members of a linkage group do not stay completely together as a rule, but during maturation exchange with a definite frequency homologous members of the paired groups (genetic crossing over). *The pairs of chromosomes also exchange homologous parts of their lengths during the maturation of the germ cells* (*cytological crossing over*).

7. In certain cases genetic crossing over is more frequent in one sex than in the other. *In these cases chiasmata formation is proportionately more frequent in the sex which exhibits more crossing over*.

8. At the time of genetic crossing over, the genes are arranged in a specific linear order. *At the time of cytological crossing over, the chromatin material is in an attenuated linear arrangement*.

9. The number of linkage groups is as a rule definite and constant for any species. *In those species thus far studied the number of linkage groups is equal to, or at least never exceeds, the number of pairs of chromosomes*.

10. Genes occasionally behave in peculiar unexpected ways (abnormal ratios, unusual linkage relationships, genetic deficiency, etc.). *In these cases the chromosomes are also found to be aberrant* (*nondisjunction, translocation, deletion, catenation, ploidy, etc*.).

It would appear to be an inescapable inference from the above facts and principles that Mendelian genes are carried in the chromosomes. (Snyder, 1951, pp. 299–301)

This set of facts imposes strong and tight constraints on any acceptable theory of "what genes are" (structural-compositional question) and "how they work" (combined structural-compositional and functional-dynamic questions). For example, an admissible theory cannot locate the large mass of genetic information in the cytoplasm—a constraint not refuted by the discovery of some slight cytoplasmic transmission from mother's biochemistry. It is interesting to reflect on the factual constraints within which Watson and Crick worked to achieve their Nobel quality breakthrough. It is no detraction from their contribution—probably the greatest single scientific advance in the latter half of our century—to say that they were so strongly constrained by discoveries not in Snyder's quoted text (one of them was made at about the time he would have been writing it) that any bright, informed scientist *who persisted* could hardly have failed. First, to Snyder's conclusory "… inescapable inference from the above facts and principles that Mendelian genes are carried in the chromosomes," Watson and Crick could add "… and, specifically, by the nucleotides." The long-held conjecture that genes were proteins was definitely excluded by Avery, McLeod, and McCarty's proof that the genetic information transmitting bacterial virulence was contained in nucleic acid rather than protein fractions, together with Chase and Hershey's radioactive tracing of phosphorus (not sulfur) in bacteriophage heredity, showing that the phage's DNA is what enters the bacterium and multiplies, leaving its protein coat outside. Second, any structural arrangement had to guarantee the exact molecular balance of adenine with thymine and cytosine with guanine, as had been shown by Chargaff. Detailed knowledge from physics and biochemistry about the size, shape, and bonding parameters of these four organic bases immediately excludes a large class of geometrical arrangements. Finally, any acceptable physical configuration must square with co-laureate Wilkins's X-ray pictures. The task was very like a crossword or jigsaw puzzle, trying and excluding different arrangements within these strong constraints. Watson's famous account, about their fiddling with the cardboard and metal models, clearly reveals this "trial-and-error until you hit upon it" procedure to be how things went in the context of discovery (cf. Allen, 1978, pp. 205 ff.). The example also shows how Popper, Reichenbach, and the Vienna positivists were wrong in saying there could be no logic of discovery (despite Popper's title). Obviously to have focused, say, on protein animo acid sequences as the *carrier* of genetic information would have been an irrational strategy. If our available body of knowledge informs us that "what's in there that carries the information is nucleic acids, exactly balanced pairwise, the whole thing looking thus in X-ray," is it farfetched to suggest that one of Langley *et al*.'s (1987) computer programs could have done it? All the program needs to do is rotate (in the plane) a thymine figure in relation to an adenine, and a guanine in relation to a cytosine, until both such pairs have the same size and shape, with allowable distances for the hydrogen bonds. That is precisely what Watson did with the physical models (Watson, 1968, figure on p. 124 and surrounding text), a trial-and-error job.

But assuming there were any other live options in 1953, *how does it look now*? Could we get a geneticist to agree in 1990 that "there must be infinitely many theories to explain these facts, since the logicians say so!"? Having cracked the code, thousands of new facts have been discovered that find their place in the elaborated theoretical network. Perhaps an even stronger persuader than all the "old" facts that have been explained and the "new" facts predicted, is the potent and ramified technology resulting from all this. *Example*: Relying on our detailed knowledge of the code—which ordered triplet of bases codes for which amino acid, which triplets are punctuation marks, and so on—the genetic engineer inserts a specified human gene into another organism so that the latter will manufacture insulin (*cf*. Hacking, 1983, 1988). Suppose a logician interested in context of discovery questions says to the geneticist, "I realize you are going great guns with the Watson-Crick model, both explanatorily and technologically. So of course you intend to stick to a theory which is so successful. But in the spirit of Feyerabendian proliferation, I'd like you to concoct a totally different theory. I want one that does not involve the idea of adenine, guanine, cytosine, and thymine being sequenced in ordered triplets (codons), and codons sequenced to form cistrons (genes). Ideally, don't use these four organic bases at all. And do it without having *any* identifiable biochemical entities that 'correspond' to the 20 amino acids. Of course you have to come out with polypeptide chains as end-product, that's what a protein is. But for the genetic machinery, make up something *totally different* from Watson and Crick, that fits all the facts." Perhaps the logician-psychologist could bribe a vacationing geneticist to have a try at this pointless endeavor. But I argue that, however clever he was, and earnestly trying, *he could not do it*. Given the present interlocking mass of facts, and excluding spooks, brownies, entelechies, whatever, there literally *is* no theory to fit the facts except bases in codons in cistrons, mRNA to ribosomes, TTT → UUU coded for phenylalanine, and so on and on.

Niiniluoto (1984, pp. 35–36), discussing Kepler's elliptical orbits, says, "Popper usually writes as if it were always fairly easy to find a great number of rival explanations," and reminds us of "the extraordinary efforts and difficulties that the hitting upon *even one* satisfactory explanation may involve." This second comment appears to clash with my remarks about the facts quasi-forcing DNA theory (how can explanations be hard to come by if nearly unavoidable?), but I think it does not. In some kinds of theorizing, or some stages of knowledge, the trick is to adopt a new perspective (Copernicus), set aside a received presupposition (Kepler, Bohr), invent a new entity (Dirac, Pauli, Freud, Morgan), apply—or even create—a powerful formalism (Newton, Heisenberg, Pearson, Thurstone). In other, "easier" situations one has a presumably complete list of the entities available, and knows their dispositional properties, making the task primarily one of combination, arrangement, configuration (Watson and Crick, Kekulé, Morgan once he accepted the fruitfly linkage statistics as chromosome maps). In the first kind of situation the context of discovery is likely to be psychologically harder, the "list of alternatives" being open and inventive; in the second, the options are given, comprising a fairly small closed set of structural, compositional, or combinatorial possibilities. (Common extreme case: Qualitative analysis by the industrial chemist or toxicologist.) But in both situations, in the context of justification—once the correct conjecture is hit upon—one sees it as "the only live option." (The life sciences' theories are constrained by their levels in Comte's Pyramid, plus fairly direct observability of most entities and processes. For an illuminating discussion of strong fact → theory inference within physics itself, see Dorling, 1973.)

[The Watson-Crick episode illustrates two metatheoretical theses worth mentioning here, pending extended treatment elsewhere (Meehl, in preparation). The first is that a clever, creative, persistent, and lucky scientist can achieve a major breakthrough despite cognitive moves that, with hindsight, appear somewhat stupid. Watson's penultimate, pseudo-breakthrough, wrongly pairing like-with-like bases, was "torn to shreds" (Watson, 1968, p.120) by the visiting American crystallographer Donahue, who informed Watson that the tautomeric forms presented in standard biochemistry texts were wrong, and the correct forms could not be hooked up as in Watson's like-to-like model. Less than a day was lost (in their race with Linus Pauling); had Donahue not been at Cambridge, it could have gone on longer. But *why even try* pairing like-to-like, when Crick had learned weeks before about the Chargaff 1 : 1 ratio of purine : pyrimidine? One guideline ("principle," not "rule") of scientific research reads: "*Ceteris paribus*, if T_1 and T_2 are both compatible with a narrow-range numerical fact, but T_1 *entails* it while T_2 does not, concentrate on T_1." While this is good Popperian advice, it surely does not require Sir Karl's views as a rationale. The principle can easily be derived Bayesian—a very non-Popperian basis. Working scientists tend to follow it whether or not they ever heard of Popper or Bayes, or would agree with either systematic position. (*Cf.* Meehl, 1990c). Fiddling with the like-to-like configuration goes against this principle, since in Watson and Crick's knowledge situation at that time, the *cetera* were *paria*, the only options under consideration being the pairings. Their quick success, despite this "methodological mistake," exemplifies a second metatheoretical point (which should be obvious from the armchair): Departure from a strong principle—even when it's not countervailed by another strong principle—need not cause failure. This is one of the "obvious but neglected" theses in metatheory that leads to my emphasis on *explicit actuarial analysis* of episodes in history of science. Such analysis will undoubtedly reveal differences in the extent to which different principles of scientific method—each shown to be "statistically advisable"—can be violated with impunity.]

These considerations clarify and alleviate a well known disharmony between scientists and metatheorists in their metalinguistic habits concerning empirical "proof." I am mildly troubled when nonphilosophical colleagues in psychology, medicine, or genetics employ such locutions as "Jones demonstrated. . . ," "Smith has conclusively shown. . . ," "Given these facts we are forced to conclude. . . ," and the like. Physical scientists from Galileo and Newton onward (and Sherlock Holmes!) have used the word 'deduce' for empirical inferences where the logician would not use that strong term; the logician would point out that our empirical knowledge is only probabilistic, that inductive inference is ampliative, that the most reasonable inference from any set of facts can be mistaken, and so on. In the empirical sciences one is never, so the standard line goes, "forced" to conclude anything, except in a psychological sense. Now all of this can be set aside as a harmless semantic imprecision by the scientist, the metatheorist knowing that he knows better how to put it if he wanted to take the time and trouble. But sometimes it seems to affect scientific discussion non-negligibly. As Carnap pointed out in his *Syntax* over a half century ago, much of scientific discourse is metalinguistic, whether or not the scientist writing is philosophically interested or knowledgeable. (Someone should do a formal content analysis of experimental journals to ascertain what proportion of the sentences are strictly object language assertions about observations and theoretical entities.) I have a distinct impression that metalinguistic discourse about whether or not the facts "force us," "show conclusively," "leave us with no alternative," etc., has a significant impact on both substantive theorizing (concoction and appraisal) and the path taken in experimentation. I further conjecture that the influence is especially strong, and sometimes pernicious, when the metatheoretical comment takes a negative form, attacking another scientist while equivocating on the slippery term 'prove.' One commonly finds a critic saying, "While Jones is quite correct in saying that facts f_1, $f_2, \ldots f_m$ are consistent with his theory, these facts do not *prove* it"; or ". . . we cannot say that Jones's theory is the only explanation. . . "; or ". . . other possibilities cannot be ruled out." The objection is often made without telling us just what the other options are, as if the scientific critic were relying solely on the logician's truism about many postulate sets to derive any finite set of theorems, without feeling an obligation to suggest what they might be. And the term 'prove' is being used without the reader knowing whether its intended meaning is *deductively entail* or *strongly corroborate*. Jones's data may of course provide excellent grounds for the latter despite the logician's "affirming the consequent" objection to the former.

But more important here is that 'deduce' may sometimes be literally correct, given general constraints of the domain and enough facts. To reconcile the scientist's strong language (supposing he is philosophically sophisticated and means what he says) with the philosopher's received doctrine about empirical knowledge, we must do some parsing of the philosopher's claim. The standard philosophical position has several components, not all equally important to the scientist. I think one can distinguish at least five components of grave philosophical doubts about empirical knowledge: Hume's skeptical attack (two elements?); the argument from illusion and perceptual subjectivity (Plato to Descartes, Berkeley and Co.); the sampling problem (statistical); and the formal invalidity of the third figure of the implicative syllogism ("affirming the consequent," "underdetermination," "too many possible theories"). The Hume component involves being deeply skeptical about regularities and, hence, about such things as the sun rising tomorrow and replicatable experiments. The scientist *qua* scientist (no philosophical hat on) does not fret that, postulating that there are permanent kinds and natural laws, as I am doing throughout this paper (but see Appendix II). Perhaps a distinguishable component (say, Hume_2) is the absence of necessity, the problem of a metaphysical causal nexus. The working scientist, despite Hume, requires some sort of distinction between "mere correlation" and causality, as is clearly shown by the development of statistical techniques intended to aid in making that distinction (partial correlation, analysis of covariance, path analysis), plus the universal preference for experimentation rather than statisticizing the organisms "as they come," with no clearly testable *handle function* of causality, as the ordinary language philosophers call it. Causal influence is an indispensable concept to the working scientist, and in the life sciences (biological and social) much energy and ingenuity are expended in efforts to ascertain which correlations (cross-sectional or timeseries) reflect direct ($x \rightarrow y$) causal influence, and which "indirect," e.g., $z \begin{smallmatrix} \nearrow x \\ \searrow y \end{smallmatrix}$, or other patterns (*cf*. Kempthorne, 1957, p. 285). Nor is this distinction confined to complex situations in economics, sociology, medicine, and behavior genetics. Simpler cases abound in psychology, for example. Given a correlation of $r = -.30$ between grades and hours spent studying, a sane counselor does not recommend to a poor student that he study less, so that he will move along the regression line toward higher grades.

The terrible problem of nomic necessity is beyond the scope of this paper; I shall merely say that deductive necessity is plenty strong "necessity" to satisfy me, so the Carnapian or Popperian way of distinguishing natural laws from "accidental universals," if it can be worked out, will suffice. I realize there are deep and unsolved problems in that approach, but I record an optimistic prophecy that they can be solved (*cf*. Armstrong, 1983; Earman, 1986, chap. 5; Lewis, 1973; Loux, 1979; Molnar, 1969; Nerlich & Suchting, 1967; Popper, 1949, 1967; Reichenbach, 1976; Simon & Rescher, 1966; Skyrms, 1980; Suchting, 1974; Swoyer, 1982; Tooley, 1977; Tuomela, 1978a; Vallentyne, 1988; and papers cited in footnote 11, pp. 385–386 of Meehl, 1970). Hume's elusive necessity is, then, deductive necessity, as theorems derivable from Carnap's fundamental nomologicals (Carnap, 1966, pp. 208–215) conjoined with contingent statements to give structure-dependent nomologicals (e.g., "all mammals die without oxygen"). That there *exist* structures of such-and-such sort is a contingent particular, similar to the contingent universal "All of Meehl's friends speak English." (I vividly recall Carnap's saying to me reassuringly, "If biologists customarily call their structure-dependent contingent universals 'laws,' that is, strictly speaking, a mistake. But a harmless mistake. The logician, of course, wishes to be clear about it.") If one prefers to avoid explicitly structure-dependent laws that presuppose existence statements, the latter can be plugged into the protasis as a conjunction of properties, yielding a derivative nomological that does follow (if reductionism is true) from the fundamental nomologicals and would, of course, apply to organisms (e.g., unicorns) that have never evolved given the initial conditions.

[On that analysis, two similar structure-dependent laws of biology turn out to be importantly different. The pre-Australian "All mammals bear live young" is an accidental universal, quite analogous (aside from indexicals) to "All the coins in Meehl's pocket are silver," or Reichenbach's non-indexical "All solid gold cubes are less than a kilometer on a side." By contrast, Quine's "All animals with a heart have a kidney" is probably derivable from fundamental nomologicals, given sufficiently detailed characterization of 'animal,' 'heart,' and 'kidney.' A metazoan with organs requires a pump for fluids circulating nutriment and waste. Nitrogenous wastes must be filtered out of the circulating medium, and 'kidney' denotes such a filter. So Quine's example is a true nomological in Carnap's sense, being derivable from the fundamental nomologicals, *the details of structure being included in the law's antecedent*. It is "structure-dependent" in the sense that the world might not have evolved animals, or metazoa, had it been a different world of the same world family (as defined by the fundamental nomologicals). We note with approval that this derived nomological licenses counterfactuals (e.g., "If there were unicorns with hearts, they would have kidneys"), and licenses the biological counterfactual by Omniscient Jones in a lifeless world of our world family, "If the world contained animals with hearts, they would have kidneys." This I find reassuring.]

"If *X* happens in System *S*, then *Y must* happen" finds the desired 'necessity' in deducibility of the derived nomologicals from the fundamental nomologicals, a metalinguistic assertion (Hiż, 1949). Sometimes the deduction can be carried out, at least *ceteris paribus*. Sometimes it cannot, except as a "sketch" or "promissory note" (Feigl). But we conjecture in the metalanguage that such entailment relations do obtain between lower-level nomologicals we know only partly and those nomologicals "next layer up" that we do know, at this time and stage of science. As for the fundamental nomologicals, *this* biological-social scientist is content with their being "mere" Humean universal correlations (the Big Committee in the Sky surely could have decreed otherwise). But I am equally happy if anyone prefers to introduce a primitive causal concept denoted by '$\xrightarrow{c}$' or Burks' '$\boxed{c}$' in a suitable axiomatization (Burks, 1951, 1977; and cf. Bressan, 1972; Good, 1961–1962; Reichenbach, 1976; Salmon, 1984; Sayre, 1977;

Simon, 1952; Simon & Rescher, 1966; Suppes, 1970). If that is done, I do not mind an informal metalinguistic non-binding explanation of the primitive concept that appeals (sinfully, to most philosophers) to our intuitions of effective volition, of exercising the "handle function," where we manipulate *X* to bring about *Y*. As a working scientist with methodological interests, I view the concept of causal nexus as something I absolutely require and can, with care, use responsibly; I leave it up to the logician and metatheorist to explicate it. If a proposed explication has big flaws, I tell him to go back to the drawing board until he comes up with a good one. Any "empirical" philosophy of science should surely start with what scientists need and do, rather than forbidding locutions because, for example, they cannot easily be translated into an extensional language. I am inclined to think that causality is like such concepts as: corroboration, observable, disposition, reduction, simplicity, randomness, probability, analyticity, partial interpretation, implicit definition, and verisimilitude—all concepts scientifically indispensable whose metatheoretical explication is unfortunately difficult.

Fortunately, the scientist *qua* scientist, not putting on a philosopher's hat, is untroubled by either Hume_1 or Hume_2. With or without Reichenbachian justification (or Popperian rejection) of induction, the scientist blithely assumes metalinguistically that "there are natural laws" (despite **6.36** of the *Tractatus*), that the world is orderly, that particulars exemplify nomologicals "because they have to," that causes we manipulate *bring about* their expected effects, that there are permanent kinds with consistent properties, and so on. Hume, either as to repeatable regularities or the nature of the causal nexus, is a scientific non-problem. The argument from illusion is not negligible, but it is largely solved by technical training and—the big thing—substitution of instruments for the fallible eye, ear, nose, and finger tips. The Illusion Argument Writ Large, "Is there an external world at all?" is as much a non-problem as Hume's. The sampling problem is largely solved by the statistician, except for spatio-temporal regions inaccessible due to remoteness. "All swans are white" presents scientific problems in statistical inference, but not as the philosopher's paradoxes of confirmation. But when we come to formal invalidity of affirming the consequent, *that* presents a real problem, very much with us, especially in the life sciences, and one which most social scientists barely appreciate (Meehl, 1978a, 1990a, 1990c, in preparation).

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A Complication Regarding Formalism and Interpretive Text

This whole line of reasoning meets with an objection that if two theories are identical as regards the calculus, but the formalism is associated with two or more different interpretive texts, we would distinguish them. So the above development, dealing solely with the formalism, will not help to decide how many theories are "in principle" available to explain the same set of experimental facts. Despite my lack of union card qualifications as a logician, I must say something about this matter of the relationship between interpretive text and calculus as jointly constituting an empirical scientific theory.

One role that the interpretive text plays in mathematicized sciences is to legitimate moves within the formalism. I have not done a statistical content analysis of this role's frequency, but it is quite striking, when one leafs through textbooks of chemistry, physics, genetics or the more developed branches of psychology, how rare it is to see a proof of the kind that one finds in a textbook of mathematics, unencumbered by sizeable amounts of embedding text. In mathematical writing one can find whole pages of formalism with no embedding text except the familiar metalinguistic transitional phrases "from which it follows that…," "without loss of generality…," "substituting from Equation [7] above…," "by mathematical induction…," "differentiating partially wrt…," "hence we see…," "solving for x…," "excluding the case $z = 0$…," "applying the Chain Rule…," etc. (I shall treat the interpretive text of empirical science as *words*, but I intend my remarks to apply to other kinds of nonmathematical embedding, such as Tinker Toy models of the ethanol molecule, linkage maps in genetics, schematic diagrams of how the radio is wired, or even Freud's crude drawing showing the topographical relationships of the psychic institutions.) When one reads scientific writings it is apparent that certain transitions in the formalism *cannot be taken* without reference to the interpretive text. I recently reread the derivations presented by Millikan (1917) in *The Electron* and Estes' (1950) classic derivation of the conditioning acquisition function from his mathematization of Guthrie's stimulus sampling theory. Almost every transition in the formalism is attended by interpretive text that is *needed* to make the "mathematical" transition. I believe that, *more often than not*, derivations in textbooks and treatises of the empirical sciences require intrusion of interpretive text as one proceeds through a derivation chain in the formalism. I have never noticed this fact particularly troubled scientists or metatheorists (although maybe the spooky problems of quantum mechanics are an exception here). Why not? Consider the received view of the logical empiricists, where one can always in principle parse the substance of a scientific theory into the calculus and the embedding text, and then reconnect them in certain ways. Referring to "the calculus" in the usual way makes it sound as if one had formation and transformation rules such that, *absent interpretation*, one could move through the formalism and reach the terminal expressions that are the desired theoretical theorems, which are then operationally connected with the observation base. What goes wrong here? We may assume, except for rare cases like quantum mechanics, that it's not a defect in the machinery of the general calculus. It must then be a deficiency in the formal premises of the special calculus. Without having considered hundreds of examples, I am not prepared to make a strong generalization. But I can say something about this puzzle which I believe is intuitively persuasive. I cannot imagine in what rigorous sense the interpretive text could "legitimate" a move in the formalism, since we assume that the transformation rules *per se* are adequate to their task for doing the mathematics. What, then, could the text contribute other than (a) to legitimate a substitution (which corresponds to asserting an identity of some kind) or (b) to motivate the assertion of an additional postulate in the special calculus? But in that case, it would be incorrect to speak of the adjoined interpretive text as defining an "alternative postulate set," because our initial adequacy requirement was that the postulate set should be capable of deriving all of the *wff*s required to explain all of the observational *wff*s; that is, we said the theory had to be "empirically complete." If one cannot make a certain move within the formalism, relying solely upon the transformation rules of the general calculus applied to the formal premises of the special calculus as initially stated, but instead must adjoin some interpretive text in order to make the required move, then *the first calculus set was not adequate to its formal task*. Therefore we are not considering two "empirically equivalent distinguishable competitors."

I used an expression above that sometimes one or more statements in the interpretive text can "motivate" the writing of a postulate in the special part of the calculus. "Motivate" here is a tricky word, the use of which by mathematicians I have never fully understood. I have the impression that it is not usually a claim of derivation, but a pedagogical aid to the reader. For the nonmathematician like myself, it prevents puzzlement or frustration about "Why are we doing this now; what's it got to do with anything; what don't I understand?" I take it that, for the mathematician of sufficient expertise, it has a more cognitive function, in that the "deep structure" of the formal relations is better apprehended by him as he reads. When 'motivates' has a stronger meaning, rather than as a metalinguistic pedagogical crutch, I discern two cases. In the first case, the conjectural assertions about the theoretical entities made in the interpretive text are such that they entail a statement in the formalism, but this statement is more general (weaker) than what the theorist proceeds to write down in the formalism. *Example*: A psychologist or physiologist is discus—sing a theoretical process in the organism that he knows: (a) has an upper limit, such as the physiological limit of reaction time, or perfect memory for recalling everything in a memory task; (b) is conjectured to proceed continuously rather than with step functions or other discontinuities; and (c) as it normally goes on, does not regress. Any function he selects to represent such a continuously increasing process with a "lid" on it will have a positive first derivative and a negative second derivative. Thus he can write in the formalism:

$$\frac{dy}{dt} > 0 \qquad \text{and} \qquad \frac{d^2y}{dt^2} > 0$$

everywhere.

In order to get on with a highly corroborable theory he would like to say something stronger than this, so he chooses among available "familiar" mathematical functions one which satisfies the above general conditions for manner of change (e.g., a logarithmic function, a simple positive growth function, a power function with $k < 1$). In such a case the interpretive text *does not completely determine* the postulate in the formalism, but it *restricts the range* of admissible ones.

A second kind of motivation from the interpretive text would be one in which the formal postulate is literally a restatement of what the interpretive text asserts in words, physical model, picture, or schematic diagram. *Example*: Psychologist E. R. Guthrie, in explaining how his theory of "all-or-nothing," single event connection (or alienation) of a stimulus to a response could agree with the obvious fact of gradual conditioning and extinction, offered an analogy: Two starving painters in Paris periodically manage to sell a painting. They have the money changed into pennies, then they get roaring drunk, and come home and scatter the pennies randomly around their apartment. Until their next sale, the supply of pennies will dwindle, so it will get harder and harder to find the pennies when they want to buy a loaf of bread. Hence the rate of expending will be roughly proportional to how many pennies are left to be found. If the population of "atomic stimuli" (both exteroceptive and proprioceptive) is like this in an experimental animal undergoing conditioning, or extinction, it follows immediately that the graph of learning will be a simple positive growth function, of the kind derived supra from the differential equation

$$\frac{dy}{dx} = \mathrm{K}(1 - e^{-\mathrm{a}x}).$$

A more interesting case than merely "motivating" is that in which what was in the formalism simply a postulate is now seen to be a theorem flowing from statements made in the interpretive text. But if that derivation really goes through rigorously, the interpretive text must be making "mathematical" statements (as in the examples above) even though they are not written in the form of equations. We would be able to write down the new formal postulates that are substantially equivalent to assertions made in the interpretive text, and could then go through the derivation within the formalism itself. In that case the original postulate has become a theorem. Do we say that this new postulate set is "different" from the old one? It's a semantic matter, but I would not see it as different in the sense of being a competitor with the old postulate set, rather as one that we might call *deeper*, because one of the original postulates is now derived. What makes it "explanatorily deeper" is a complicated and difficult matter which I do not consider here.

It might be objected that we said the old postulate set (without the interpretive text) was complete, and if that is so the addition of new postulates that simply restate something said in the interpretive text should, according to a well known theorem of logic, result in a contradiction. Two answers: When I said that the postulates were complete I did not mean "categorical" in the strong sense of the logician, but merely in the more informal sense of Russell and Whitehead's introduction, that the set is complete if it is adequate to get all the theorems that we want to get. In this case, that theorem class is defined as the whole set—large but finite—of theoretical *wff*s corresponding to the whole set of *wff*s in the observation language that express accepted experimental findings. Secondly, even if the formal postulates were complete, in the logician's technical sense of categorical, the theorem that an adjoined statement if not redundant will generate a contradiction is always relative to a stated vocabulary. In the situation imagined here, the "deeper" postulates underlying the original one (which now becomes a theorem) involve an extension of the vocabulary. It would obviously be impossible to prove by logic alone that an empirical hypothesis could not itself be derived from deeper empirical hypotheses about antecedents, components, or "hidden variables." (Perhaps this bears on von Neumann's alleged proof of the impossibility of hidden variables in QM?) If all the theoretical terms are Ramsified out, including the cross-domain "metaphysical" ones I listed above, then presumably a consistent total Ramsifier would hold that the motivating texts may differ, but that they do not constitute alternative postulate sets. That is, all of the meaning of the descriptive terms, when Ramsified, is given by upward seepage from the operational or coordinating text, no surplus meaning being available. If one is a consistent total Ramsifier, no "surplus meaning" inheres in the original theoretical terms of the interpretive text which we eliminate by means of the Ramsey Sentence.

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Concluding Remarks

Probably needless to say, but to avert any possible misunderstanding, I do not assume that a scientist would proceed in such a "mechanical" manner as described above, merely tallying the box score when his current postulates are getting him into hot water with the facts. For example, in order to arrive at a reasonable ball-park estimate of the correlation between verisimilitude (crudely defined) and corroboration (even more crudely defined), we either forbade formal undoings in the mathematics, or confined our discussion to theories that did not contain any, or at most very few. But of course no sensible scientist would confine himself to a crude box score of experiments without examining the role of the various postulates in the derivation chains that terminated in a falsified empirical prediction.

The currently popular meta-analysis in the social sciences has been wrongly criticized by some who have not read the theoretical rationale of it (Glass, McGaw, & Smith, 1981; Hunter, Schmidt, & Jackson, 1982), as though the method stopped with calculation of effect sizes (ES) and their standard deviations, permitting no "incisive, scholarly reflection on the internal properties and relationships of individual studies." Nobody even slightly familiar with the method as advocated by its inventors could object on this ground. After the meta-analyst has found that the overall ES shows that "something is going on" with the intervention being investigated, the next step—by far the larger part of the statistical analysis—involves comparison of special ES's and their variances for subsets of studies, with respect to the list of factors considered *a priori* likely to influence them.

In our situation, a theorist will take note of the fact that Postulates 3 and 7 often appear in falsified derivation chains, but he will not be blind to the further interesting fact that experimental or statistical falsification tends not to occur with respect to those derivation chains that contain both postulates. Mere inspection of the formalism will immediately reveal—if by some oddity it had not been noted before—that there occurs a formal undoing in the mathematics. These kinds of considerations belong to a later stage in the development of corroboration indexes, and it would complicate the argument I am concerned with here, which aims to show that Lakatos is mistaken in saying that belief in *any* stochastic linkage between verisimilitude and corroboration must rely on a sheer leap of faith.

A more serious complaint, which might argue for my using some other term than 'corroboration,' is that a box score showing merely the percentage of replicable successful predictions of derivation chains ignores the prior probability of the predicted finding, paying no attention to Popperian risk or, for inductivists, Salmonian damn strange coincidences. While I have suggested (Meehl, 1990c) an index of corroboration based upon the dangerousness of a numerical prediction, and another based upon the riskiness of a predicted function form (with or without specification of the parameters), and I have some tentative notions as to how what is here dealt with as a crude box score of percent hits could be improved along those lines, I set those aside for presentation at a later time. Absent a rigorous proof, I can only suggest that if a crude box score of "successes" and "failures" can be shown to have a strong stochastic link to verisimilitude of the postulate set, inclusion of prior riskiness in a corroboration index could hardly *reduce* the size of that linkage, and would probably enhance it somewhat.

Finally, restriction to theories sufficiently quantified to permit the writing of mathematical functions, and to theories of the "input → intervening process → output" kind that I often consider as a psychologist, leaves other sorts of theories aside, and I do not wish to argue by weak analogy that the stochastic verisimilitude-corroboration linkage could be derived there in a similar fashion. My modest aim here was to show that for at least one nonnegligible class of scientific theories, (*a*) there are good reasons for expecting a high correlation between verisimilitude and corroboration in the long run, and (*b*) that the logicians' truism about an "infinite number of postulate sets would explain any finite set of observational theorems" is not as epistemologically dangerous to probable inference as it seems, and (depending on one's view of Ramsification) may not even be literally correct.

APPENDIX I

The Sin of Psychologism—Keeping It Venial

What Popper, the Vienna Circle, and one of their intellectual ancestors, Frege, called the sin of 'psychologism' (see Abbagnano, 1967) is not considered sinful among contemporary metatheorists, and in some quarters is looked upon as a virtue. It goes without saying that the historian of science, whether proceeding by the case study method (with or without statistical summary over cases) or by some yet to be developed actuarial procedures of the kind I advocate (Meehl, 1983, pp. 371–372, 1990c, in preparation), cannot view an inquiry into the psychology of the scientist he is studying, or the psychological or sociological aspects of an episode involving a group of scientists, as methodologically forbidden. When we today contrast empirical metatheory or, more broadly, naturalized epistemology with the metatheory and epistemology of the logical positivists and their predecessors, we should not use terms like '*a priori*' or 'armchair' to imply that the logical positivists, or the great British empiricists who contributed to their intellectual heritage, made no use of empirical generalizations about the human mind. Everyone knows that Locke, Berkeley, and Hume freely referred to facts about the human sense organs, mind, and society, but it is sometimes supposed that the logical positivists and others close to them (e.g., the Berlin School, Popper, or C. I. Lewis) did not do so. It must be admitted that the Vienna Circle and Americans influenced by them were not quite consistent in this matter, but it is obvious that anyone who talks about "intersubjective confirmability," "protocol sentences," "ostensive definition," even "directly observable," is invoking concepts that go beyond formal logic or the mathematical theory of probability. I recall resisting the amount of naturalized epistemology urged by our late Center director Grover Maxwell, saying that I would prefer to have my philosophy of science totally free of psychology, an aspiration which he quickly liquidated by the simple question, "Well, Meehl, tell me which principles of scientific methodology you can deduce from Russell and Whitehead?" One might think at least *modus tollens* is an example, but even that doesn't do, because, following Popper, we take it for granted that the falsifying consequent is an observational statement.

What the positivists and their empiricist predecessors (and for that matter those called rationalists, like Kant and Descartes) did in their "armchair epistemology" was to rely on certain empirical generalizations about the human mind that were obvious to any thoughtful person, and did not require experimental or statistical study of the kind that we would today classify as falling under the heading of cognitive psychology or the sociology of knowledge. That persons have perceptions, that human sense organs are responsive to certain kinds of external physical happenings but not to others, that perceptions may err, that persons have biases, that an object's appearance depends on illumination and perspective, that we are fallible as regards recording, retaining, retrieving, and verbally reporting episodes in our experience, that some kinds of observations result in nearly 100% consensus among sane rational people exercising due care (e.g., Campbell's famous list, 1920/1957) are all empirical statements, obviously not truths of formal logic or mathematics. So if the newer metatheorists advocate more than this, one presumes that they go beyond these familiar everyday truisms about the human sensorium and intellect, both in (*a*) enriching the metaconcepts and (*b*) utilizing more scientifically formalized methods of investigation, as by cognitive psychologists, sociologists of science, etc.

The matter of psychologism is also related to the current tendency to dismiss Reichenbach's (1938) dichotomy between the context of discovery and the context of justification. While the familiar English terms are Reichenbach's, the distinction is older than that, as pointed out in the clarifying paper by Hoyningen-Huene (1987). As he says, the core distinction (oddly bypassed by the critics) is that between the factual and the normative. I believe this easy dismissal of Reichenbach is a serious mistake, and—like much current positivist-bashing—does historical injustice. He nowhere says that the statements a metatheorist makes in the two contexts are disjoint classes, and it takes little thought to realize that they could not be. (In attributing to the logical empiricists views not explicitly stated by them, I supplement Quine's "Principle of Charity" with a more mundane point: None of these men were dumb, and if you find yourself attributing to them some belief that requires stupidity, you are likely mistaken.) The point of Reichenbach's distinction is that the *question* being asked is different in the two contexts. That does not mean that in the course of seeking the answer to one kind of question we are forbidden to employ any of the information, empirical or logical or mathematical, that we would find ourselves properly employing in answering the other kind of question. A metatheorist who denies that *any such* distinction as Reichenbach's can be made goes against what we learn in freshman logic, that one ought not to commit the genetic fallacy, or the argumentum *ad hominem* or *ad personam*. A *historian* of science studying the history of biochemistry and dealing with the invention of the benzene ring could not properly omit mention of Kekulé's famous dream of the hoop snake. That is part of the history of the happening, and it would be bad history to delete something that apparently played a crucial role in the context of Kekulé's discovery. But if a contemporary chemist told us that he didn't believe in the existence of the benzene ring, and his reason for disbelieving it was because Kekulé was influenced by a dream—obviously a grossly unscientific source!—we would not countenance such an argument. That example suffices to show that there is *something* to Reichenbach's old dichotomy that must be preserved in the interest of rationality. "What psychosocial forces contributed to Scientist *X* preferring a theory of this kind?" and "What constitute rational grounds for holding this theory?" are simply not the same kind of question, and a metatheory that conflates them is unsatisfactory.

How much psychology of the individual, and how much sociology of knowledge, should the modern metatheorist include in his enterprise? Obviously the historian of science engaged in case study is not free to exclude anything of these that appears to have played an appreciable role in what happened during a given scientific episode. Just what historical information he should include is currently unclear. The metatheorist's aim is to distill from the history of science helpful advice, principles, and guidelines for scientists engaged in theorizing, theory testing, and appraisal of theories. Even a well supported statistical generalization about how most scientists *in fact* proceed in some of their decision making cannot *ipso facto* be transformed into an advisory principle, since there is no guarantee that what scientists have a tendency to do, by and large, is something that they ought to do. We do not at present know, *absent thorough statistical analysis of a large number of episodes*, which of the common "cognitive habits" of working scientists tend to pay off, and which ones do not, or are even counterproductive (Faust, 1984). What one wants in prescriptive metatheory is a formulation of guiding principles, based upon success stories *as well as failure stories*, together with further principles that tell the theory appraiser under what conditions a given guiding principle should be allowed to countervail another so as to raise the odds of (not assure!) success. The relevance problem here is akin to that familiar to students in such fields as ethics and analytical jurisprudence, where we invoke second-order decision principles or preference rules, whether one kind of *prima facie* right regularly trumps another, and the like. The complexity of this kind of second-order decision rules is one of the reasons why I advocate a formalization of the amend—ment and appraisal process based upon large-scale actuarial studies of the history of science.

Unlike some metatheorists of the younger generation, perhaps because I have not totally freed myself of the logical positivism of my youth (Campbell, 1990; Meehl, 1990c, Reply), I do *not* want to increase the extent to which metaprinciples distilled from the history of science will contain multifarious references to psychological and social factors. (It will be convenient to compress these into one term, 'psychosocial,' to include cognitive psychology, the values and motivations of the scientist, his prejudices of whatever source and origin, and the economic and political forces acting upon him directly and indirectly.) I agree with the contemporary definition of metatheory as rational reconstruction of the empirical history of science, and therefore itself an empirical discipline; but I prefer to hold the psychosocial content of prescriptive metatheory to an unavoidable minimum. I offer four reasons for this preference.

First, there is the matter of sheer logical relevance. We properly include Kekulé's dream in a case study, but it does not contribute to formulating metatheoretical advice, either as to theory invention or theory appraisal. A collection of fruitful dreams by chemists or others (is Kekulé's the only known example?) does not lead us to give the unhelpful advice, "Have creative dreams," which nobody would know how to effect if we gave it. And it must not lead us to metatheoretical advice, "Do not appraise a theory favorably if you find out that the theorist was inspired by a dream." The basic criterion of logical relevance is strong enough, and usually obvious enough, that what I have just said suffices to justify this basis of preference.

Second, I have in mind considerations as to division of labor. The metatheorist of the future faces a challenging educational assignment to become competent in his scholarly specialty. Consider the burdens of learning basic epistemology, a useful amount of symbolic logic, the history of metatheory in some detail, at least one particular science (which he is going to philosophize about) in some depth, and the techniques and criteria of historiography (though not necessarily becoming competent as a historian of science). If we add to all this, which is a pretty big order, that he has to know experimental cognitive psychology, psychodynamics (Freud, Adler, Jung, Horney, and their critics), sociology, and a dash of economics and political theory (so he can converse with Marxist methodologists who attribute Darwinism to the rise of British industrial capitalism), the whole thing will become undoable. It might be suggested that the problem can be handled by suitable interdisciplinary cooperation. As one who has engaged for half a century in interdisciplinary research (with publications in seven areas), I permit myself the definite opinion that this rarely works unless a fair amount of the content of the relevant disciplines is in all the cooperating minds. I believe this is what most scholars who have engaged in interdisciplinary research will attest. On grounds of division of labor, it seems unrealistic to say that the empirical metatheorist must develop scholarly competence in cognitive and motivational psychology as well as sociology, political science, history, philosophy, economics, and possibly other disciplines before he can do his job properly.

Third, the metatheorist, whether proceeding by an impressionistic survey of case studies, by a statistical meta-summary of the same, or by a combination of these two with a set of statistical indexes of theory performance (Meehl, 1990c, in preparation) will have an even harder time than now in documenting interpretations of what took place in an episode if it is required that the psychodynamics and social vectors impinging on the scientist or on a group of scientists must be studied in detail. I can testify as a clinical practitioner that reliable information and inference about psychodynamics is difficult to attain given one hundred psychoanalytic couch hours with the patient/client strongly motivated to be honest; to achieve it via documents (as in psychohistory) is usually impossible.

Fourth, if a heavily psychosocial metatheory eventuates in a set of guidelines which, after development by the metatheorist, are then conveyed to the working scientist, the kind of detailed and highly personal information about other theorists and experimenters that the appraising scientist would be forced to deal with is quite out of the question. Imagine trying to appraise somebody's theory of animal learning if it is required to take account of the (still living) theorist's Oedipus complex, or his procapitalist bias, his schizoid genes, and so on. The thing would become completely unmanageable; and the result would be that the working scientist would continue to ignore the metatheorist in the same way that most working scientists, especially in the developed fields, ignore him today.

I do not dispute that there will be some borderline cases, and that we should be careful not to exclude elements of the psychosocial that require to be included in a proper metatheoretical appraisal. But *they should be held down to a minimum*, and that minimum should be expressed in metatheoretical principles that the working scientist can apply without going into components of the psyche and the social group that can rarely be adequately appraised in ongoing science. Of course the distillation of a metaprinciple from a collection of case studies does not require that all of the statistical trends found in the case studies be embodied in the principle. The only ones that should be embodied are those that are logically relevant in theory appraisal. *Example*: Before he became involved in studying the "nature of love" in Rhesus monkeys by utilizing artificial (wire and terrycloth) "mothers," the distinguished psychologist Harry Harlow made important contributions to the psychology of learning which were antecedents of the whole cognitive psychology movement, and helped undermine associationistic behaviorism as the dominant paradigm in American psychology. We learn in reading Harlow's biography (Sidowski & Lindsley, 1989) that when he began his research with monkeys at the University of Wisconsin the economic situation was such that he had a tightly limited supply of experimental subjects. (Monkeys are a lot more expensive to buy than white rats, cost more to house and care for, and used to have a disturbing tendency to die of such diseases as tuberculosis and pneumonia.) Because of this severe shortage of experimental animals, Harlow had to violate the familiar canon of animal behavior research that one should use fresh ("naive") animals in each experiment. It was as a result of using the same monkeys in a series of different sorts of experiments that he discovered the surprising extent to which monkeys "learn to learn." Out of that developed his fruitful theory of the establishment of *learning sets*.

It would be wrong for the historian of science engaged in a case study, either of the individual scientist Harlow or of the rise of cognitive psychology against associationistic behaviorism at mid-20th century, to leave out the interesting fact that financial exigency was serendipitous here. But when we take this fact that is of interest to the historian of science, and ask what helpful advice the metatheorist can distill from this or similar episodes involving a laboratory's relative financial poverty, we surely don't want to make reference to the economics of the situation. We don't say to scientists, "Do your research under adverse financial circumstances, where it's hard to obtain apparatus, subjects, space, and research assistants." The content of the helpful advice would instead be some—thing like, "It is sometimes fruitful, capable of leading to unexpected, novel, and illuminating observational results, to depart from an accepted canon of experimental procedure, such as 'Always use naive organisms in psychological experiments.' " We note that this piece of advice (which is a good deal more useful than "Try to have creative dreams containing visual analogs to your problem") does not say anything about money. The optimal formulation of a principle that we distill out of the Harlow episode, and the *strength* of this advice, should depend upon how many similar episodes we get in various disciplines. It is a helpful heuristic hint that says nothing about the politics or economics of the University of Wisconsin, or the fact that Harlow began his work in the days of the Great Depression.

If I am correct that one should try to hold the psychosocial components of the reconstruction and resulting metatheoretical advice to a minimum, how is that minimum to be defined? We cannot hope, and should not demand, that it be reduced to zero, which would presumably be the position of a Vienna positivist phobic about psychologism. The short answer would be: "In formulating a metaprinciple offered to the working scientist to help him in concocting, testing, amending, and appraising theories, and given that a certain psychological tendency has emerged from the case material [studied—as I advocate—actuarially, but that's not crucial here], what should be distilled into the metaprinciple is that portion of the empirical generalization (*a*) that turns out to be a correlative of success, and (*b*) the logical relevance of which we can discern as plausibly responsible for that correlation."

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Convergence, Prediction, and Diversity

To exemplify the admissibility (in the context of justification) of psychosocial data, while holding it down to a minimum fixed by relevance, consider the long-standing puzzle and unresolved disagreement (e.g., Carnap versus Popper) about a theory's prediction of novel facts. The first logic textbook I studied as a teenager (Castell, 1935) distinguished between the "argument from convergence" and the "argument from prediction" in the chapter on proving a hypothesis. In the former, one discerns that facts f_1, f_2, f_3 *converge upon H*, they "make sense" or "hang together" on the conjecture *H*, which *explains* them. In the latter, *H* (however we came by it—usually by discerning a convergence) *predicts* facts f_1, f_2, f_3. One needs no formal opinion poll to know that scientists have a strong predilection for the argument from prediction, giving it more probative weight than is given to convergence, *ceteris paribus*. Consider two knowledge situations identical as to their fact-theory set $[T, f_1, f_2, f_3]$ but differing "historically," in the context of discovery. In the pure convergence situation, a scientist concocted *T* to explain f_1, f_2, and f_3. In the (usual) mixed convergence-prediction case, the scientist knew only f_1, and f_2, concocted *H* to explain them, derived hitherto unknown f_3, and then found it in the lab. I have never found a scientist in any area who did not prefer the predictive case, although some who have studied philosophy of science are a bit nervous about their preference (but see Brush, 1989). That this preference for successful prediction is not based on technical knowledge of scientific method, probability theory, or general epistemology is shown by the same preference being found among detectives, prosecuting attorneys, automobile mechanics, psychotherapists, physicians, historians, etc. (In the practice of psychoanalysis, I follow a rule of thumb to avoid convergence-suggested interventions in the course of a session until I have silently made two or more successful predictions about the analysand's associations, *cf*. Meehl, 1983, pp. 373–374, 400–406.) Given this ubiquitous epistemic preference, it has been a minor scandal of metatheory that no clear exposition, nor cogent demonstration of its rationality, exists. There is a major threshold problem about what 'novel' should be taken to mean in this context. Metatheorists of high competence have not even agreed on the preference's soundness. For some (e.g., Giere, Popper) successful prediction is the *sole* basis of empirical corroboration. Carnap, on the other hand, considered it irrelevant. I vividly recall his replying to my Popperian emphasis on prediction, "But, Meehl, how could the date of a statement affect its logical bearing on another statement?" The short answer to this seemingly devastating query is that metatheory always treats of more than the logical relations between statements. (What I have called 'Grover Maxwell's Thunderbolt,' to go along with Occam's Razor and Hume's Guillotine, asks, "What principles of scientific methodology can we deduce from *PM*?" the answer being, of course, "None.") But these moves, while fending off Carnap functioning as pure logician, do not satisfy as a rational reconstruction of our empirical preference for the argument from prediction over convergence. I believe that the foregoing alternative to Lakatos' sheer leap of faith can give us some assistance.

Consider a theory T_i that has been degraded from T_{OJ} by deleting or distorting Postulate P_i. The empirical corpus contains *m* (replicated) observational statements that correspond to *m* theoretical *wff*s derivable from T_i. Let the pervasity of Postulate P_i be π_i. Sampling *m* statements randomly from the empirical corpus, the relative frequency (= probability) of finding a set that is compatible with [= fails to falsify] the false theory T_i is $(1-\pi_i)^m$. But suppose T_i was concocted with *k* of the *m* facts "in mind," and the derivation chain to each is valid (i.e., the theorist did not commit a mathematical mistake in the formalism, or a fallacy in the essential embedding text). Then the *k* facts cannot falsify, so the potential falsifiers are the remaining $(m-k)$, so T_i's survival probability is $(1-\pi_i)^{m-k} > (1-\pi_i)^m$. For a given accepted fact set, the probability of detecting the falsity of a degraded theory is a decreasing monotone function of $\frac{k}{m}$, the proportion of facts "known to and used by" the theorist.

Moving from the truth dichotomy (and its correlated falsification risk) to verisimilitude (and its correlated box score), the ratio $\frac{k}{m}$ sets a lower bound on the false theory's "success rate," hence the latter is biased upward as a way of ordinating verisimilitude in reliance on the Spearman rank correlation theorem *supra*. *Example*: Assume Postulate P_i has a pervasity $\pi_i = .8$, that four out of five derivation chains adequate to generate the observational corpus involve Postulate P_i essentially. If $k = 5$ of $m = 10$ facts were "used" to concoct the theory, then even if the remaining $(m - k) = 5$ were mispredicted, the success rate in our fact sample would be $\geq .5$, and its expected value would be .6, inflated by a factor of 3. Considering a large class of actual or possible theories ordinated as to verisimilitude relying on the rank correlation with box score, a displacement from .10 to .60 (in the possible range .00 to 1.00 of box scores) would make for grave errors in appraisal.

A less potent but non-negligible factor is sampling stability. With k facts eliminated from the set, our fallible estimate of π_i is based on a sample of $m - k$, and the standard error of a proportion being $\left(\frac{pq}{n}\right)^{1/2}$, the stability of our π_i estimator declines as the root of $(m - k)$. Hence the forced pre-fitting of T_i to k known facts suffers from both a sizeable optimistic bias and an increased random fluctuation.

How does this rational reconstruction fare when evaluated by the "minimize psychosocial content" principle enunciated in the preceding section? I think rather well. It makes the least possible reference to the theorist's psychosocial situation, namely, *that minimum needed to assure derivability of k facts selected from the empirical corpus*. As an old ex-positivist with a distaste for psychologism (and, far more, for sociologism), I would prefer to liquidate this psychosocial reference as well; but I am pretty well convinced it can't be done. The basis on which T_i and F_k are selected from the huge set of *T*s and *F*s so as to be associated with each other, making possible my statements about relative frequencies, biases, etc., is the historical fact that the theorist knew about the k but not about the $(m - k)$ when he concocted T_i, and (assuming he committed no mathematical or logical bloopers) this guarantees the derivability of the k-set. I note with satisfaction that this is *all* we have to inquire about concerning psychosocial matters to demonstrate the epistemic superiority of the argument from prediction over that of "mere convergence." We have not had to examine the theorist's motives or emotions. We have not inquired whether he liked or hated his PhD advisor, whose theory T_j differs from T_i; whether he is French or English (Duhem poked fun at the English liking for mechanical models, saying English physicists made one inhabit a factory instead of the Gallic realm of mathematical concepts; Nobel laureate Lenard rejected relativity, pre-Hitler, as "Jewish physics"); nor how well he was tax-supported. We have not, praise God, asked about his breast-feeding in infancy; whether he was an anal, oral, phallic, or genital character; whether he was Marxist or Republican, or witty, or workaholic, or gay, or creative, or senile—or even stupid, *so long as he's competent enough to avoid making invalid derivation chains*. None of this psychosocial stuff clutters up our meta-metatheorizing, or has to be ferreted out by a working scientist when he relies on our rough actuarial metaprinciple (not *rule*!): "In general, if a theory derives m facts, k of which were known to the concocting theorist, verisimilitude is more accurately appraised when $\frac{k}{m}$ is smaller."

The above reconstruction helps in thinking about how to state the relevant definition of a novel fact, whether it should be defined as one known to no one, or as one not reported somewhere in the scientific literature, or as a fact not generally known by the scientific community, or not known by the theorist, or known but not consciously utilized by him in concocting T (*cf.* Murphy, 1989, and references cited therein). It seems on my analysis that the last one or two are the viable candidates, but I shall not treat further of this thorny problem.

A related problem is the scientist's preference for a qualitatively diverse set of facts rather than the "same number" of facts in a narrow domain. Is this anything more than a mere psychological preference? Of course we want replication before admitting a putative observation into the corpus, itself a complex matter I do not address. It involves questions of statistics, experimental design, reliability of measuring instruments, and the track record of the reporting scientist. Many scientists' track records as to replicability being somewhat mixed (I speak as a psychologist, I don't know how it stands in, say, chemistry), the known or inferred biases of researchers are relevant to one's decision to include protocols in the corpus; this is a clear instance of the "genetic fallacy" not being a fallacy at all. Granting this, a matter beyond the scope of the present paper, I think it is pretty clear that, *ceteris paribus*, almost all scientists would prefer five different experiments each replicated twice to the same experiment replicated 10 times; and they would give more evidentiary weight if the five were "very different setups" than if they had a large qualitative overlap in the observational dimensions studied, differing mainly or solely in parametric respects. My impression is that this preference for qualitative diversity is about as strong as that for the argument from prediction, so it would be nice to rationalize it also. I believe it is fairly easy to sketch a convincing line of argument, but to do a rigorous job one would have to be competent in symbolic logic. Thus, I offer the sketchy "proof" hoping that some logicians among my readers will be moved to work on the problem.

The observational vocabulary of the fact domain contains a finite set of predicates and functors O_1, $O_2, \ldots O_n$. What is "observational" depends upon the epistemic status of auxiliary theory and the tested reliability of instruments, interesting questions, but not relevant here. I flatly reject the popular Kuhnian claim that all observations are theory infected. Perhaps that is true for particle physics (although *cf.* Franklin, 1986), and it doesn't hurt much there because physicists have good theories. It is most certainly *not* true for psychology. When I was doing experiments on latent learning, my protocol contained such observational statements as: "Rat #13 turned right at the choice point and locomoted to the right hand end box." There's nothing theory-infected about that statement, not even an itsy-bitsy psychological theory. Such commonsensical "theory" as the genidentity of Rat #13 on two different nights is shared (presupposed, not even mentioned) by the competing theories of Hull and Tolman, and is of no theoretical interest (Meehl, 1983, "Excursus on observations," pp. 389–395). That the theoretical/observational distinction has a solid and clearly judgeable reality in psychology has been nicely shown in the empirical study by Clark and Paivio (1989). The reader may draw a line as he pleases, so long as there are *some* statements in the observational language that can clash with what happens in the lab or the clinic file statistics. If a scientist's theory infects his protocols to the extent that it assigns the determinate of a determinable independently of his perceptions (or, today, independently of his computer printout of an instrument's reading) we don't consider him a "clever experimenter properly sensitized by his theory." On the contrary, we call him a fraud.

What makes T_{OJ} an empirical (rather than metaphysical) theory is that a proper subset of the theoretical terms are "operationally defined," fairly directly coordinated with terms of the observational vocabulary. We said above that T_{OJ} is "complete" in our rough sense, that all true observational *wff*s of the domain are operationally coordinated to theoretical *wff*s that are consequences of the postulates of T_{OJ}. Thus any observational law containing observational functors (O_1, O_2) will correspond to a *wff* in theoretical language containing a pair of theoretical functors θ_1, θ_2, the correct *wff* relating them $L(\theta_1, \theta_2)$ being the terminus of a derivation chain valid in T_{OJ}. Considering a postulate P_i of T_{OJ}, it may or may not occur in the derivation chain to $L(\theta_1, \theta_2)$. If it does, then T_i, the theory degraded from T_{OJ} by deleting or distorting P_i, will be falsified by any experiment testing $L(\theta_1, \theta_2)$.

There will usually be hundreds or thousands of distinguishable experimental designs, concocted by ringing the changes on experimental parameters, that are capable of testing $L(\theta_1, \theta_2)$. Thus, in Clark Hull's theory of mammalian learning, the output fork law relating response amplitude to response probability can be be tested using as subjects rats, monkeys, human children; employing thirst, hunger, shock-avoidance, as motivation; measuring locomotion, chain-pulling, bar pressing, gesture, speech as the instrumental response. For each of these experimental dimensions, different quantitative levels can be set, yielding experimental patterns whose number goes up exponentially as we refine the parametric divisions. One badly neglected sampling problem, probably more important than the sampling of organisms to which so much meticulous attention is conventionally paid, is the sampling of *situations*, which should be in some sense "representative" of the statistical ecology of the species studied (Brunswik, 1947, 1955; Hammond, 1954; Postman & Tolman, 1959: Sells, 1966).

Suppose a minitheory T_{OJ} deals with a simple, restricted domain having only 10 observational functors, each of these being operationally coordinated to theoretical functors θ_1, $\theta_2 \ldots \theta_{10}$. So there are $\binom{10}{2} = 45$ *wff*s relating the θ-pairs. I call the proportion of these *wff*s whose derivation chain includes P_i the *pairwise pervasiveness* of P_i, ${}_2p_i$. Choosing a pair of functors (θ_1, θ_2) randomly from the set of 45 *wff*s, the probability of getting a falsifying pair is ${}_2p_i$. Choosing a set of *m* pairs at random, the probability of T_i escaping refutation is $(1 - {}_2p_i)^m$, a monotone decreasing function changing exponentially with the number of pairs examined. Suppose P_i is weakly pervasive, occurring in derivation chains for 15 of the 45 θ-pairs. Then the probability of T_{OJ} escaping refutation if we perform 10 experiments sampling the domain randomly is $(.67)^{10} = < .02$, a Salmonian damn strange coincidence. If, instead of "covering the factual waterfront" randomly, we perform 10 experiments all involving the same theoretical functors (θ_k, θ_1), they will either all falsify T_i or none of them will, depending on whether (θ_k, θ_1) is among the pervaded set, a circumstance with probability equal to the pairwise pervasity ${}_2p_i = .33$, an order of magnitude larger than that yielded by the diversified collection of experiments.

This analysis also shows why it is good strategy for scientists to sample the experimental domain in a *non*random manner, and why one component of "scientific cleverness" is concocting experiments that map the θ-terrain more rapidly than would be possible by blindly "trying lots of things." For our simple case of 10 θs, performing 45 experiments testing all of the (θ_k, θ_1) *wff*s will ensure falsification of any degraded theory $T_{ijk\ldots}$; this will happen even for one T_j whose sole bad postulate P_j has pairwise pervasity ${}_2p_i = \frac{1}{45}$, because that single experiment relating functors (θ_k, θ_1) will occur in the set performed.

Of course the real-life situation is more complicated and interesting. The postulates of T_{OJ} are connected with the derivable *wff*s in a network whose strands linking P_1, P_2,$\ldots P_n$ with operational θ-*wff*s vary with each postulate's pervasity and depend on inner structural relations that have not, I believe, been studied by logicians with real empirical theories in mind. For example, if Postulate P_i does not contain θ_1 and θ_2, the only way it can occur in a derivation chain to $L(\theta_1, \theta_2)$ is by being conjoined with one or more postulates that collectively do contain these functors and some of which relate θ_1 and θ_2 to θ's occuring in P_i. This is the kind of meta-metaquestion alluded to above as requiring competence in symbolic logic that I do not possess.

Shifting attention from truth (and refutation) to verisimilitude (and box score), it is apparent why experimental diversity is desirable. The proofs *supra* relating box score with verisimilitude involve getting estimates of T_{ijk}'s rank in the ordination of degraded theories via the rank order of box scores *when the complete set of experiments is considered*. If one chooses a subset neither representatively (trying all the θ-pairs) nor randomly, the box score is simply not a good sample, and using it in reliance on the verisimilitude–corroboration correlation theorems is not justified. For example, if none of the experiments tried involve (θ_7, θ_9) whose relation $L(\theta_7, \theta_9)$ is the only law whose derivation chain utilizes Postulates (P_i, P_j, P_k), these three not functioning except as a triad, then theory T_{ijk} is incorrectly ordinated by the box score to the extent of having three corrupted postulates. In a 10-postulate theory this represents a serious displacement in the verisimilitude ordination.

If the minimal psychologism here involved troubles latter day Carnapians, Popperians, or other logical purists—I am not acquainted with any such—I can only proffer the advice Luther gave the scrupulous Melanchthon: *Pecca fortiter!*

APPENDIX II

For Humeans and Fictionists

The main text explicitly assumes nomic regularities and realism as to theoretical entities. I do this because: (*a*) Scientists operate on these assumptions; (*b*) It makes the exposition straightforward, easier, and simpler; (*c*) It is my own philosophical position. The location of my discipline in Comte's Pyramid is such that the explanatory entities and processes are often quasi-observational for sciences in the next tier below (e.g., the attenuated muscle twitches of Hull's fractional anticipatory goal response r_g, a rat's damped "munching or slurping," are observables for the physiologist). Mentalistic concepts are observable if anything is, except when unconscious, as in psychodynamics. Construing realism about these is complicated, but for an indentity theorist they must be brain events (Feigl, 1967; Feigl & Meehl, 1974; Meehl, 1966). "Pure intervening variables," as in Skinner's system, present no problem (MacCorquodale & Meehl, 1948).

I find it hard to adopt a fictionist stance when thinking about these matters, even when I would like to manage it for generality of treatment. This is not, I think, a piece of realist dogmatism on my part. I was a fairly contented *Aufbau* positivist in my youth, and do not despise the great Carnap of 1928, or the phenomenalist Ayer of 1940. My problem today is that I cannot *understand* fictionism. I literally do not know what a physicist would *mean* if he said "We're going to aim a beam of neutrons to hit that target 100 yards away. Naturally we have to take care that some drunk doesn't wander through the alley between buildings and get hit by them, and then sue us for carcinogenesis. But there aren't any such things as neutrons" (*cf.* Giere, 1988, pp. 115 ff.). I am even inclined to think fictionism may be self-contradictory, and not by misapplication of Tarski's semantic conception of truth. Sketch of such an argument: I say, considering a simple theory about a theoretical predicate θ, "This theory explains the observed facts." That claim covers each particular we admit into the corpus. The impoverished, 2-step theory reads, where *P, Q* are observable predicates,

$$(x)[P(x) \rightarrow \theta(x)]$$

$$\frac{(x)[\theta(x) \rightarrow Q(x)]}{\therefore\ (x)[P(x) \rightarrow Q(x)]}$$

and this derivability is what constitutes the "explanation" provided by the theory. If it explains each experimental particular (for simplicity, I replace a definite description by an equivalent proper name, as we do when we say "the Michelson-Morley experiment" or "Mendel's study of garden peas"), we must write

$$P(a) \rightarrow \theta\ (a)$$

$$\therefore\ \theta\ (a)$$

$$\theta(a) \rightarrow Q\ (a)$$

$$\therefore\ Q\ (a)$$

But having writen $\theta(a)$, we cannot avoid, by the logician's Existential Generalization, the transition

$$\frac{\theta\ (a)}{\therefore\ (\exists x)\ \theta\ (x)}$$

which, as fictionists, we proceed to contradict in the metalanguage by saying "theoretical entities do not exist," which would seem, for a given theoretical predicate θ, to imply $\sim (\exists x)\ \theta\ (x)$. The metalinguistic left hand taketh away what the object-linguistic right hand doeth. I don't subscribe to many metaphysical propositions, but one of the few I hold is "Nonentities exert no effects." An ordinary language formulation would be "If there isn't any Santa Claus, then it isn't he who brings the presents. *So who else is bringing them?*"

Setting aside my inability to truly empathize with fictionism, do my derivations have any relevance to a Humean skeptic about regularities (and necessity), or to a genuinely consistent fictionist? I believe they do. Separating issues, one *could* be a realist about theoretical entities but a Humean skeptic about regularities extrapolated into the future. (We don't meet such persons, interestingly, but I pass over why not.) It is not incoherent to hold that electrons are actually "out there," would be if we hadn't discovered them, exerting causal influence, constituting the true explanation of macro-events—and yet feel compelled by Hume (and Reichenbach, and Popper) to admit there's no assurance whatever that electrons will have the properties tomorrow that they now have. The Big Committee or Zoroaster's demiurge might sabotage our little inductive enterprise at $t_0 + \Delta t$, who can say such a thing is "not conceivable"? Santayana's "animal faith" is, unfortunately, the last word. So it behooves me to parse the issues, despite the rarity of the conjunction "Humean skepticism" and "theoretical realism."

What do my proofs do for the Humean realist? Consider all the true *wff*s (theoretical + observational) describing events in the Minkowski manifold. I am concerned here with the life sciences, so we bypass problems about the light cone, tachyons, etc. Everyone (including Hume and Popper) will agree that matters *may* continue after t_0 as before. If so, T_{OJ} and its derivable *wff*s are exactly as I have treated them, and the situation is like Reichenbach's defense of induction. "*If there is* a T_{OJ}, theories deviating from it will have a factual track record that correlates with their closeness to it." Here T_{OJ} holds for all time, or at least from early after the Big Bang until the expansion stops and the Big Collapse begins (Nietzsche, Poe's *Eureka*); for me as a theorizing psychologist, until life on Tellus is extinct. On the other hand, if the Big Committee introduces a radical cosmic cataclysm (altering Carnap's fundamental nomologicals) at t_0, the correlation of pre-t_0 theories' verisimilitudes with factual fit will be good, as proved. After t_0 it's up for grabs; who knows what will happen? Although I cannot forbear to point out that, even in that crazy case, if there is a *new* true theory T'_{OJ} that holds from t_0 to some further t_m (not utter chaos at all levels), then the correlation between new theories' T'_i, T'_{ij}, T'_{ijk},... verisimilitudes vis-à-vis T_{OJ} with the factual particulars in the post-t_0 Minkowski region will be good, and my proofs will hold for the new state of affairs. My arguments concerning pervasity, forecasting, and qualitative diversity go through as before. So I think the Humean law skeptic + theoretical realist can take almost as much from my arguments as one who postulates changeless natural laws, with nomic necessity, despite Hume. And the derivations, unless I am mistaken, *mean* the same thing for him as for me, at least when we discuss the social and biological sciences.

For the fictionist, the case is different. He would hardly request a proof that observational track record must correlate with verisimilitude, since the latter concept is intrinsically realist about theories. Given the semantic conception of truth, verisimilitude is an ontological property of theories. If T has postulates about electrons completing atomic shells, but there aren't any electrons, then T is false [$T \neq T_{\mathrm{OJ}}$], despite T's ability to derive the valences of elements. If there is no such fluid as libido, then dammed up libido does not cause the neurotic's anxiety. We may say that a fictionist is not *entitled* to demand such proof, on fictionist principles. However, by a reformulation of my task in instrumentalist terms, something useful can be provided even to a dyed-in-the-wool fictionist. Consider again the humongous set of observational *wff*s describing all the experiments (actual? actual and possible?) on mammalian learning that Earthlings will perform before the sun burns out. Let T_{IS} [= Theory of Instrumentalist Smith, as contrasted with T_{OJ}, held by Omniscient Jones] be perfectly adequate to derive all those correct *wff*s. Let T_i, T_{ij}, T_{ijk},... be theories obtained by altering or deleting Postulates *i, i* and *j, i* and *j* and *k*, and so on. Then my arguments go through as before, although T_{OJ} is defined by perfect verisimilitude and T_{IS} by perfect instrumental adequacy. We are now inquiring about the human knower's sampling the domain of observational *wff*s and extrapolating via T_{IS} (or its degraded forms) to other observational *wff*s as yet unobserved. If T_{IS} does this pragmatic job perfectly, degrading it should have the same adverse instrumental consequences that we showed will occur by degradation of T_{OJ}. If someone replies to this, "Very well, but then, how do you know T_{OJ} can be *different* from T_{IS}?" I would happily respond "I don't, and have even suggested in the text (pp. 23 ff.) that it can't. But you, in arguing thus, are making a case against fictionism—which is fine with me. The point is that if perfect performer T_{IS} *can* differ from my ontological conception of T_{OJ}, and you repudiate an interest in T_{OJ}, I offer my proofs that degradations of T_{IS} will do a poorer job for you than T_{IS} in your journey through the *wff*s of the observational world. As a fictionist, what more can you reasonably ask of me?" When the original problem is restated instrumentally, we have questions about transition from one event sample to another event sample. The "parameter" (in the sense of inferential statistics) can be reasonably discussed between us, and derivations can be offered for both formulations, despite my preference for verisimilitude as an ontological concept. I say T_{OJ} 'works' because it's true, and I sample the fact domain to estimate how well a theory T 'works' for the exhaustive set of observational *wff*s, as a means to my end of judging T's similitude to T_{OJ}. You do the same thing, bypassing the question of truth except for the observational *wff*s themselves. There is a big philosophical difference about our aims. If there is a false theory $T_{IS} \neq T_{\mathrm{OJ}}$ that fits *all* observational facts, I shall be deceived. But you will not complain, your sole aim being successful prediction.

The scandal of Hume's Problem and the unsatisfying character of purported solutions or *dis*solutions (e.g., Carnap, 1950, 1952b; Kant, 1787/1958; Keynes, 1929; Popper, 1935/1959; Reichenbach, 1938) has led, in my opinion, to a specially critical (pessimistic?) attitude among philosophers, one that differs from their attitude to some analogous "big problems" of metatheory. Since I detect a smidgeon of this pessimistic criticality in myself—I would prefer to go along with Salmon (1957, 1963, 1965, 1967, 1968) in his ingenious amendment and valiant defense of Reichenbach, or accept my revered teacher Feigl's (1954) reply to Burks (1953), if I could—perhaps a brief therapeutic airing is appropriate. The underlying feeling is a kind of "epistemological guilt," that *unless* one can *really solve* Hume's Problem, in the strong sense of bringing the rational justification of empirical science into *quantitative* accord with our animal faith in natural orderliness, then logical or statistical reconstructions that are "rational" *within* that (illicitly presupposed) non-Humean framework are not worth much. They may be interesting as "proofs if…," but too *iffy*. I believe this position, while understandable given the history of ideas, is mistaken. First, as a general point, in most theoretical cognitive enterprises we consider it worthwhile to classify, analyze, conjecture, derive, refute, etc., operating within a system of concepts and premises that we "accept," however tentatively, without being confident we could prove them. Second, when the metatheorist is functioning in one of his several social rôles, that of *ancilla scientiae*, it seems legitimate to ask the working scientist, "What are *your* rock-bottom presuppositions, which I may take as framework when I attempt to help you in theory-appraisal, research strategy, critique of experiments, etc.?" The metatheorist may not share those scientific presuppositions; or may share them but feel uncomfortable, *qua* philosopher, in doing so despite Hume. But neither of these differences from the working scientist logically prevents his analyzing and reconstructing within the scientific frame, any more than a mathematician is prevented from deriving theorems within Riemannian geometry *for the physicist*—despite doubts about whether our physical world's space is Riemannian. Finally, it is worth remembering that in most theoretical disciplines, the "fundamental" ideas are typically hardest to explicate. If scientific progress hinged upon complete clarity regarding all basic concepts and satisfactory proof of all basic postulates, we would be paralyzed. *Example*: Experimental psychologists possess a vast, varied, and well-corroborated body of knowledge concerning effects of reinforcement on operant behavior—strengthening of responses, shaping of effector topography, response chaining, response competition, influence of various reinforcement schedules on rate and resistance to extinction, etc. And stemming from this theoretical base we have a powerful instructional technology (programmed texts, teaching machines, animal training). This has been accomplished despite persisting disagreement about the basic nature and exact role of the prime operation that pervades the whole enterprise, *viz*., *reinforcement*. *Example*: Metamathematicians do not agree about some pretty basic matters (e.g., formalism, logicism, intuitionism; what is a number?; since set theory is consistent with both Cantor's Conjecture and its denial, does Excluded Middle not apply to it?). These fundamental troubles do not lead us to close down mathematical journals, on the ground that "everything mathematicians write is pointless until these rock-bottom matters are settled." *Example*: There is hardly any concept more basic to both theoretical and applied statistics than *probability* (and its linked concept *random*!), yet both remain the subject of unresolved—and sometimes heated—controversy. So far as I know, no philosopher or mathematician has provided a rigorous definition of 'randomness' that commands even majority, let alone Quaker, consensus. But statisticians meanwhile write books and articles deriving interesting and useful theorems concerning probability relations among random variables.

Pressing the skeptical position to its extreme, we are unnerved to find that even deduction can only be "justified" circularly, as Lewis Carroll pointed out a century ago. In today's language, that an argument fits a valid (syntactical) proof schema does not say tautologically that it is (semantically) truth preserving (Haack, 1976). I had an acquaintance, not mentally ill, who insisted that "as a mystic doubtful about logic and against scientism," he honestly wasn't sure about *modus ponens*, and dared me to "make" him be. Or consider the Rule of Detachment. It has an inherent component of praxis, it licenses a reader (or writer) to persist in asserting, recording, believing something, in the course of a proof, given a valid derivation step earlier on. I once treated a (nonpsychotic) patient, a physicist, who suffered a grave study impairment, because in perusing a proof in physics he had to keep checking back to derivational steps in preceding pages; he "understood" the Rule of Detachment but could not permit himself to rely on it, to operate in accordance with it. This was because he was not certain he could fully trust his short-term memory. He found himself unable to rely on a *postulate* (that's what it has to be!) concerning his own memory that nonobsessional persons routinely make—despite the well-known argument *reductio* that clearly refutes an infallibility claim. Even as postulated and relied on by healthy persons, its form has to be stochastic. Bertrand Russell is one of the few contemporary philosophers who takes the terrible problems of the enduring ego, the specious present, the trustworthiness of memory, and the solipsism of the present moment with the seriousness they deserve, *given adoption of the strong skeptical stance*. Sincere, persistent, absolute Cartesian doubt cannot be practiced, we are biologically incapable of it. But if it could be managed, the cognitive process would simply be estopped. Once you get into Descartes' damned stove, you're cooked. It is inconsistent for a philosopher to dismiss these stomach-aches by saying (which Hume and Russell cheerfully concede) "Well, of course, if you take it *that far*—but nobody can *sincerely* do that, so why raise it?", but then to argue that belief in the reality of the external world and the existence of natural laws is impermissible. For my part, I find it *easier* to doubt a relative consistency proof in mathematics, or the Axioms of Infinity, Choice, or Reducibility, or my memory of a week-old conversation, than to doubt the existence of nomic necessities. Or what if I doubt the five axioms of *PM*, crazy as that seems? Or—to really run judgment empiricism ragged—what if a perverse Pythagorean Ramsifier argues that the theoretical entities of geography could be the natural numbers, given the Löwenheim–Skolem theorem (Berry, 1953, p. 53)? "Absurd," you say. Yes, utterly absurd. But can the *logician* refute it?

However, I must be honest, and confess I agree with Salmon (1965) who discerns a disanalogy between the "circularity" of relying on *modus ponens* and reliance on induction, but the difference seems to me to be a quantitative one, not a difference in kind. (His rejoinder is also interestingly "psychologistic," a point that would merit careful scrutiny were it not for time and space limitations here!) Salmon says we cannot *conceive* of a *modus ponens* transition leading to a false conclusion. Thus while Haack, following Lewis Carroll, is, strictly speaking, *logically* correct—"we cannot prove, without circularity, that no counter-example to *modus ponens* will ever be found," says Salmon—yet there is this difference: we can easily conceive of worlds in which our future inductive inferences are falsified, or in which "perverse" [counter-] inductive rules succeed. True enough. *Modus ponens* represents an extreme case—probably the most extreme—of "inconceivability," yet Salmon is too much of a tough-minded post-positivist to *assert* that inconceivability guarantees truth, as his text shows. Some of my other "deductive" examples above are located between *modus ponens* and a counter-Hume postulate about nature; others (e.g., Axiom of Infinity, relative consistency proofs, or that my recent memory of a long proof is good enough to apply the Rule of Detachment) are, by my introspection at least, no more compelling than a sufficiently general anti-Humean nomic metapostulate, perhaps even less so. But I wish to distinguish here (as Salmon implicitly does in arguing against the Oxbridge "dissolvers") between a general kind of *operating inductively* (despite Hume) and *selecting a rule* for optimizing (I would prefer satisficing) one's inductive operations. If postulating in the metalanguage some such statement as "there are nomologicals" (i.e., nomic necessities linking theoretical universals that will persist into the future) is a sheer leap of faith, having made that leap, one may seek the best method for discovering them. This is a rational process, pursued by Salmon in his amendments and elaborations of Reichenbach, and by me in the present paper.

[At the risk of being misunderstood as offering one more fallacious "reply to Hume"—which, I repeat, is an impossible task—let me sketch briefly a formulation that takes some of the sting out. Hume's question is intrinsically anthropocentric, being about "the future," as is Reichenbach's justification of the straight rule. I suppose there is nothing wrong with that, when we are doing the epistemology of human knowers rather than that of Omniscient Jones. But consider for a moment a less anthropocentric way of looking at things. It is well known that the fundamental nomologicals of physics do not contain time as an explicit variable. In sciences like psychology where t may occur as independent variable (e.g., a forgetting curve) it is *time since a certain event or state*, not time as a calendar date (*cf.* Spengler's "chronological time," 1918/1926, pp. 97, 126, 153). The astrophysicist locates events in the 4-dimensional Minkowski frame, and the logician prefers a tenseless language. Suppose we present our world-theory T, the system of fundamental nomologicals that fits and explains the facts, making no reference to whether the particulars that instantiate those nomologicals are dated pre- or post-t_0, which happens to be the calendar date of our assertion. Someone asks, "What makes you consider T credible?" We reply, "T is credible because it fits and explains the facts." Some of the particulars instantiating T are separated by huge Minkowski intervals from those particulars that scientists observe fairly "directly" (e.g., spots on photographic plates in Earthling astronomical observatories). Events "before" t_0 and "after" t_0 are equally real, as Aristotle, Aquinas, and most contemporary physicists and metaphysicians agree. When one asserts T without mentioning egocentric particulars, as a set of nomologicals holding throughout the Minkowski manifold, there does not *seem* to be any "postulate" about induction involved or required. Reference is not made, either in T or the metalinguistic comment on its evidence, to "the future." The *rational grounds* for holding T are the evidentiary facts, admittedly a sample of events from a small region in spacetime. But the number of such evidentiary facts sampled is gigantic, and their character varies widely, from the complex experiments of nuclear physics to the millions of billiard shots as in Hume's famous example. If T were not verisimilar, its factual track record would be a "damn strange coincidence," (Salmon, 1984; Meehl, 1990c). How does the Humean's challenge read, in this timeless epistemology? I assert T, arguing that it is well supported by the evidence. The Humean does not deny that, but he proposes an alternative theory T_H. T_H is like T except it amends T by saying, with a metalinguistic excursion, "and events later than t_0, the cosmic calendar date of our present conversation, instantiate different nomologicals, or none at all." That is, of course, logically possible. But why would one opt for T_H rather than T? It seems a rather whimsical choice, demarcating a space-time region for a profound theoretical distinction by the date of two Earthlings' philosophical discussion. One might even consider it quite unwarranted—lacking a Leibnizian sufficient reason, so to speak. (It's almost as if we could shift the burden of proof onto the Humean!) Of course we cannot compel the Humean to speak timelessly, forbidding him to ask the classic question in its anthropocentric way. But I submit that when having "therapeutically" adopted the timeless position ourselves for the nonce, our non-Humean metaphysical postulate seems somewhat less arbitrary, daring, and egocentric than it did before. Of course this whole line of thought presupposes the validity of arguments from explanatory power, but that is not Hume's Problem, as I understand it. I hope it is not impertinent to remark that the recent evolution of Salmon's thought is encouraging in its emphasis on causal explanation and the "damn strange coincidence" principle of scientific proof. I venture the opinion that if he applies his intellect to spelling out the rationale of credence based on explanatory power, this will be an even more important contribution to metatheory than his explication and defense of the amended straight rule, or the integrated causal-statistical mode of explanation.]

Do not misunderstand me, I am not here offering a deductive argument (impossible) or an inductive argument (circular). Nor am I buying the Oxbridge incantation that induction is simply "what we mean by rationality" in empirical matters (Salmon, 1957, 1965). We have to start by accepting Hume, that no such *argument* will be valid. What I am doing here is not validating the proof of a conclusion but *vindicating* (Feigl, 1950) *the making of a postulate*. This language emphasizes that we do not rationalize ("justify") the postulate, rather the making of it. Perhaps even better, we explain and defend the maker in his doing so. I say with Kant, "'Ought' implies 'can.'" To a skeptic who criticizes, "You ought not to *postulate* such things, you must *prove* them," my plea is involuntariness. I find that I cannot, in the best philosophical mood, *get* myself to doubt that there are causal regularities "out there." Nor can I proceed, either as scientist or practical man, without some such postulate. Since it seems I am stuck with adopting a batch of postulates, at least a half dozen, even to do pure logic and mathematics (or to know that, and who, I am!), it gives me no intellectual guilt to add another postulate about natural laws. If "judgment empiricism" (the doctrine that all contingent knowledge can be validated on the basis of experience and logic alone) won't wash (Maxwell, 1974; Hawthorne, 1989; Russell, 1948), I'll just have to make the best of it. Of course, such an acceptance of the human epistemic condition may leave me with a preference for a *minimal* postulate, guided by the aim to "make empirical science cognitively do-able." The present paper does not attempt to work that out, it simply speaks of Omniscient Jones's correct postulate set. I incline to think that a combination of the Armstrong–Dretske–Swoyer–Tooley position with amended Keynes is the way to go, the metaphysical postulate saying something like, "A finite proper subset of empirical universals linked by nomic necessity causally underlie (produce and explain) the quasi-nomological and stochastic correlations presented by the macro-events of our perceptual experience." It's deliberately loose, general, informal, *advisory*. Taking it from there are scientific observation, theoretical conjecture, experimentation and statistics.

I can best put it thus: I adopt a (vague) postulate about lawfulness in the empirical order, about the external world ("independent of my will," as Descartes reminds us), that has two properties for me as cognizer and agent. One property is "psychological" [$\simeq$ biological], to wit, I cannot *make* myself honestly doubt it. (This is Santayana's "animal faith.") The other, somewhat more philosophically respectable, is that I cannot (rationally, instrumentally) *do anything*—in the clinic, library, laboratory, as a scientist; or as investor, voter, taxpayer, parent, spouse, therapist, teacher—without implicitly relying on it. I cannot even decide to starve to death from philosophical discouragement, absent some such postulate, since I have no "reason" at all to expect caloric suicide will work. (Besides, I may return as a cockroach due to bad karma, who knows?) So the nomological postulate has these two compelling characteristics, (1) that I *cannot* seriously doubt it (intuitive compulsion), and (2) if I *could*, I would be paralyzed, both as thinker and actor. If the combination of these two features does not warrant a postulation (= "sheer leap of faith"), I cannot imagine what would. *But these are considerations formulated in the pragmatic metalanguage*. If you are thinking, "But, Meehl, one's intuitions, however compelling, do not *prove* anything," you have not been reading carefully. *Of course* our intuitions don't prove anything. We do not deceive ourselves that intuitions and pragmatic pressures are "proofs of the postulate." If the postulate could be proved, we would not call it a postulate.

It may be objected, that a metaphysical postulate about causal regularities persisting into the future is a "sheer leap of faith," which this paper's title repudiates. Yes, it surely is a sheer leap of faith, which I have no wish to deny. But it is not Lakatos's leap mentioned in my title. *His* leap is our belief in a correlation *between* verisimilitude (how close we are to getting the laws right) and corroboration (how good our theory's factual track record looks). That this ontological–epistemological correlation may rationally be expected to be positive, and very high as the fact domain is widely sampled, I claim to have shown, via logic and mathematics alone. It's a conditional proof—if there *are* no real nomologicals statable in postulates, there is nothing to which our theories *could* be (more or less) verisimilar! But if there are such natural laws, our postulates' closeness to stating them will be stochastically linked to empirical performance. This conditionality of the proof is why I alluded to Lakatos's "whiff of inductivism" at the start.

Given all this, I take the position that a metatheoretical derivation of why we may rationally expect theories' factual fit track records to be stochastically related to their verisimilitude, *postulating* nomologicals concerning existent ("real") entities, is a worthwhile endeavor. This is what the working scientist postulates, and he will not fault the metatheorist for developing a proof within that framework. If pressed, I am quite content, with either my scientist or philosopher hat on, to construe 'postulate' here by its etymology (Lat. postulare, *to demand*). Perhaps it should bother me a lot, but it doesn't.

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[At the time this paper was written,] Paul E. Meehl [was] Regents' Professor of Psychology, Professor of Psychiatry (Medical School), and Adjunct Professor of Philosophy. He was a co-founder (with Herbert Feigl and Wilfrid Sellars, 1953) of the Minnesota Center for Philosophy of Science. He is coauthor (with S. R. Hathaway) of *Atlas for Clinical Interpretation of the MMPI* (1951), and author of *Clinical versus Statistical Prediction* (1954), *Psychodiagnosis* (1973), and *Selected Philosophical and Methodological Contributions* (1990). His current research [was] on taxometric methods for classification and genetic analysis of mental disorders.