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#085

Some Methodological Reflections on the Difficulties of Psychoanalytic Research

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Being here in the somewhat ill-defined role of a "methodologist" with psychoanalytic experience, I shall first make a few general comments reflecting my own views on philosophy of science. Since it is impossible to develop or defend them in a brief presentation, let me simply say that these views accord generally with the consensus of those who claim expertise—if such exists, as I believe—in that field. That they are not widely accepted in psychology reflects a failure to keep up with developments, many psychologists espousing a philosophical position that is some thirty years out-of-date.

Whatever the verisimilitude (Popper, 1959, 1962) of Freud's theories, it will surely be a matter of comment by future historians of science that a system of ideas which has exerted such a powerful and pervasive influence upon both professional practitioners and contemporary culture should, two-thirds of a century after the promulgation of its fundamental concepts, still remain a matter of controversy. That fact in itself should lead us to suspect that there is something methodologically peculiar about the relation of psychoanalytic concepts to their evidential base.

Let me begin by saying that I reject what has come to be called "operationism" as a logical reconstruction of scientific theories. Practically all empiricist philosophers (e.g., Carnap, Feigl, Feyerabend, Hempel, Popper, Sellars), thinkers who cannot by any stretch of the imagination be considered muddleheaded, obscurantist, or antiscientific in their sympathies, have for many years recognized that strict operationism (in anything like the form originally propounded by Bridgman) is philosophically indefensible (but see Wilson, 1967, 1968). In saying this, they do not, however, prejudge those issues of scientific research *strategy* that arise between a quasi operationist like Skinner and a psychoanalytic theorist. And it is commendable that Skinner and his followers (unlike some psychologists) have been careful to avoid invoking "philosophy of science" in their advocacy of either substantive views or research strategy.

Associated with my rejection of operationism is the recognition that biological and social sciences are forced to make use of what have come to be known (following the late, great philosopher Arthur Pap, 1953, 1958; see also Cronbach & Meehl, 1955) as "open concepts," the "openness" of these concepts having two or three distinguishable aspects which space does not permit me to develop here. One important consequence of this openness is that we must reject Freud's monolithic claim about the necessity to accept or reject psychoanalysis as a whole. This is simply false as a matter of formal logic, even in explicitly formalized and clearly interpreted theoretical systems, and such a systematic "holism" is a fortiori untenable when we are dealing with what is admittedly a loose, incomplete, and unformalized conceptual system like psychoanalysis. It is well known that proper subsets of postulates in physics, chemistry, astronomy, and

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genetics are continually being changed without "changing everything else" willy-nilly, and it is absurd to suppose psychoanalytic theory, unlike these advanced sciences, is a corpus of propositions so tightly interknit that they have to be taken "as a whole."

I would also reject any requirement that there should be a *present mapping* of psychoanalytic concepts against constructs at another level of analysis, such as neurophysiology or learning theory. All that one can legitimately require is that psychoanalytic concepts ought not to be *incompatible* with well-corroborated theories of the learning process or nervous system function. But the situation in these two fields is itself so controversial that this negative requirement imposes only a very weak limitation upon psychoanalytic theorizing.

I would also combat the tendency (found in some psychonomes) to treat the terms "experimental" and "empirical" as synonymous. An enterprise can be empirical (in the sense of taking publicly observable data as its epistemic base) *without* being experimental (in the sense of laboratory manipulation of the variables). Such respectable sciences as astronomy, geography, ecology, paleontology, and human genetics are obvious examples. We should not conflate different dimensions such as the following: experimental-naturalistic; quantitative-qualitative; objective-subjective; documentary-behavioral. It is obvious, for example, that one can carry out objective and quantitative analysis upon a nonexperimental document (e.g., diary, personal correspondence, jury protocol).

I should make clear that while I am not an "orthodox Popperian," I find myself more in sympathy with the logic and methodology of science expounded by Sir Karl Popper (1959, 1962; and see Bunge, 1964) than with that of any other single contemporary thinker. While I share with my Minnesota colleagues Feigl and Maxwell reservations about Sir Karl's complete rejection of what he calls "inductivism," I agree with Popper in emphasizing the extent to which theoretical concepts (often implicit) pervade even the so-called "observation language" of science and of common life; and I incline to accept refutability (falsifiability) as the best criterion to demarcate science from other kinds of cognitive enterprises such as metaphysics.

There is a certain tension between these views. What I have said about operationism, open concepts, and the scientific status of nonexperimental investigation makes life easier for the psychoanalytic theorist; but the Popperian emphasis upon falsifiability tends in the opposite direction.

As a personal note, I may say that, as is true of most psychologists seriously interested in psychoanalysis, I have found my own experience on the couch, and my clinical experience listening to the free associations of patients, far more persuasive than any published research purporting to test psychoanalytic theory. I do not assert that this is a good or a bad thing, but I want to have it down in the record. In the "context of discovery" (Reichenbach, 1938; but see Lakatos, 1968a) this very characteristic attitude is worth keeping in mind.

The inventor of psychoanalysis took the same view, and it might be good research strategy to concentrate attention upon the verbal behavior of the analytic session itself. If there is any strong empirical evidence in support of Freud's ideas, this is perhaps the best place to look, since this is where he hit upon them in the first place! We have today the advantage which he regrets not having, that recording an analysand's verbal behavior is a simple and inexpensive process. Skinner points out that what makes the science of behavior difficult is *not*—contrary to the usual view in psychoanalytic writing—problems of *observation*, because (compared with the phenomena of most other sciences) behavior is relatively macroscopic and slow. The difficult problems arise in slicing the pie, that is, in classifying intervals of the behavior flux and in subjecting them to powerful conceptual analysis and appropriate statistical treatment. Whatever one may think of Popper's view that theory subtly infects even so-called observation statements in physics, this is pretty obviously true in psychology because of the trivial fact that an interval of the behavior flux can be sliced up or categorized in different ways. Even in the animal case the problems of response class and stimulus equivalence arise, although less acutely. A patient in an analytic session says, "I suppose you are thinking that this is really about my father, but you're mistaken, because it's not." We can readily conceive of a variety of rubrics under which this chunk of verbal behavior could be plausibly subsumed. We might classify it syntactically, as a complex-compound sentence, or as a negative sentence; or as resistance, since it rejects a possible interpretation; or as negative transference, because it is an attribution of error to the analyst; or, in case the analyst hasn't been having any such associations as he listens, we can classify it as an instance of projection; or as an instance of "father theme"; or we might classify it as self-referential, because its subject matter is the patient's thoughts rather than the thoughts or actions of some third party; and so on and on. The problem here is not mainly one of "reliability" in categorizing, although goodness knows that's a tough one too. Thorough training to achieve perfect interjudge scoring agreement *per rubric* would still leave us with the problem I am raising.

There are two opposite mistakes which may be made in methodological discussion on the evidential value of verbal output in a psychoanalytic hour. One mistake is to demand that there should be a straightforwardly computable numerical probability attached to each substantive idiographic hypothesis, of the sort which we can usually compute with regard to the option of rejecting a statistical hypothesis. This mistake arises from identifying "rationality in inductive inference" with "statistical hypothesis testing." One need merely make this identification explicit to realize that it is a methodological mistake. It would, for instance, condemn as "non-rational" all assessment of substantive scientific theories, or the process of inference in courts of law, or evaluation of theories in such disciplines as history or paleontology. No logician has succeeded in constructing any such automatic numerical "evidence-quantifying" rules, and many logicians and statisticians are doubtful whether such a thing could be done, even in principle. It is obvious, for instance, that a jury can be put in possession of a pattern of evidence which makes it highly rational to infer beyond a reasonable doubt that the defendant is guilty; but no one (with the exception of Poisson in a famous ill-fated effort) has tried to *quantify* this evidential support in terms of the probability calculus. Whether a distinction can be made between quantifying the corroboration of nomothetic theories (an algorithm for which, says Lakatos [1968b, p. 324], is precluded by Church's theorem) and quantifying the probability of particularistic (= idiographic) hypotheses is difficult to say, although we should pursue that line of thought tenaciously. Ideally, I suggest, a Bayes-Rule calculation on the idiographic constructions of psychoanalysis should be possible.

The opposite error is the failure to realize that Freud's "jigsaw-puzzle" analogy does not really fit the psychoanalytic hour, because it is simply not true (as he admits elsewhere) that all of the pieces fit together, or that the criteria of "fitting" are sufficiently tight to make it analogous even to a clear-cut criminal trial. Two points, opposite in emphasis but compatible: Anyone who has experienced analysis, practiced it, or listened to taped sessions, if he is halfway fair-minded, will agree that (1) there are sessions where the material "fits together" so beautifully that one is sure almost any skeptic would be convinced, and (2) there are sessions where the "fit" is very loose and underdetermined (fewer equations than unknowns, so to speak), this latter kind of session (unfortunately) predominating.

The number of theoretical variables available, and the fact that the theory itself makes provision for their countervailing one another and reversing qualities (e.g., the dream work's sometime expression of content by opposites), lead to the ever-present possibility that the ingenuity of the human mind will permit the therapist to impose a construction which, while it has a certain *ad hoc* plausibility, nevertheless has low verisimilitude. What we would like to have is a *predictive* criterion, but the trouble is that the theory does not claim to make, in most cases, highly specific content predictions. Thus, as Freud himself pointed out, while we can sometimes make a plausible case for the occurrence of certain latent dream thoughts which were transformed through the dream work into the manifest content of a dream, the same set of dream thoughts *could* have been responsible for a manifest content completely different. Similarly, in paleontology, the fossil data may be rationally taken to lend support to the theory of evolution, but there is nothing in the theory of evolution that enables us to predict that such an organism as the rhinoceros will have been evolved, or that we should find fossil trilobites. Or, again, the facts may strongly support the hypothesis that the accused had a motive and the opportunity, so he murdered the deceased; but these assumptions would be equally compatible with his having murdered him at a different time and place, and by the use of a knife rather than a revolver. I do not myself have any good solution to this difficulty. The best I can come up with is that, lacking a rigorous mathematical model for the dream work, and lacking any adequate way of estimating the strengths of the various initial conditions that constitute parameters in the system, we should at least be able to apply crude counting statistics, such as theme frequencies, to the verbal output that occurs during the later portions of the hour when these are predicted (by psychoanalytically skilled persons) from the output at the beginning of the hour. I look in this direction because of my clinical impression that one's ability to forecast the *general theme* of the associative material from the manifest content of the dream plus the initial associations to it, while far from perfect, is nevertheless often good enough to constitute the kind of clinical evidence that carries the heaviest weight with those who open-mindedly but skeptically embark upon psychoanalytic work. Let me give a concrete example of this (one on which I myself would be willing to lay odds of $90 to $10, and on more than a mere "significant difference" but on an almost complete predictability within the limits of the reliability of thematic classification). If a male patient dreams about fire and water, or dreams about one and quickly associates to the other (and here the protocol scoring would be a straightforward, objective, almost purely clerical job approaching perfect interscorer reliability), the dominant theme in the remainder of the session will involve *ambition* as a motive and *shame* (or triumph) as an affect. In 25 years as a psychotherapist I have not found so much as a single exception to this generalization. This kind of temporal covariation was the essential evidential base with which Freud started, and I suggest that if sufficient protocols were available for study, it is the kind of thing which can be subjected to simple statistical test. Since there is no obvious phenotypic overlap in the content, a successful prediction along these lines would strongly corroborate one component of psychoanalytic theory, namely that involving the urethral cluster. Now I believe that there are many such clusterings which could in principle be subjected to statistical test, and my expectation is that, if performed, they would provide a rather dramatic support for many of Freud's first-level inferences, and a pretty clear refutation of others.

Whether or not one is a convinced "Bayesian" is largely irrelevant here, provided we can set *some* safe empirical bounds on the priors, which we can presumably do for the "expectedness" (of our test-observation) in the denominator of Bayes's Formula, relying on statistics from a large batch of unselected interviews. Even expectedness values $\cong \frac{1}{2}$ can become a basis for fairly strong corroborators if there are several all "going the right direction." And if we *are* real, feisty, honest-to-goodness "Bayesian personalists" about probability, it might be plausibly argued that a fair basis for assigning the priors would be guesstimates by academic psychologists largely ignorant of Freud. This basis of prior-probability assignments permits us to go outside the analytic session into those diverse contexts (for which explicit statistics are lacking) of daily life, history, biography, mythology, news media, personal documents, etc.—data sources which collectively played a major role in convincing the nontherapist intelligentsia that Freud "must have something." Example: No philosophically educated Freudian would have trouble guessing which of these four philosophers wrote a little-known treatise on *wind*: Kant? Locke? Hume? Santayana? A Freudian would call to mind Kant's definition of a moral act as one done *solely* from a sense of duty (rather than, say, a spontaneous loving impulse or a desire to give pleasure); the pedantic punctuality of his daily walk, by which the Königsberg housewives allegedly set their clocks; his remarkable statement that "there can be nothing more dreadful than that the actions of a man should be subject to the will of another"; and his stubborn refusal over many years to speak with a sister following a minor quarrel. But I doubt that a panel of (otherwise knowledgeable) psychologists, ignorant of Freudian theory, would tend to correctly identify Kant as having a scholarly interest in wind—even if we helped them out by adding the fact of Kant's excessive concern with constipation in his later years. The same is no doubt true of my rash prediction (upon first descending the stairs inside the Washington monument) that the wall plaques would show more financial contributions by fire departments than by police departments. (They do.) Point: The very "absurdity" or "farfetched" character of many psychoanalytic *connections* can be turned to research advantage, because the prior probabilities of such-and-such correlations among observables are so very differently estimated by one thinking outside the Freudian frame.

As must be apparent from even these brief and (unavoidably) dogmatic remarks, I locate the methodological difficulties of testing psychoanalytic theory differently from many—perhaps most?—who have discussed it, whether as protagonists or critics. For example, I do not waste time defending (Frenkel-Brunswik, 1954) the introduction of unobservable theoretical entities, knowing as I do that the behaviorist dogma "Science deals only with observables" is historically incorrect and philosophically ludicrous. The *proper* form of the "behavioristic" objection is, as always in sophisticated circles, to the *kind* of theoretical entity being invoked (read: its role in the postulated nomological network, including linkages to data statements). Methodological insight quickly shifts our attention away from such philosophical issues to examination "of the merits," as the lawyers would say. Let me emphasize that I do not rely tendentiously upon philosophy-of-science considerations as a *defense* of psychoanalytic theory either. To rebut a dumb objection is merely to rebut a dumb objection; it does not make a scientific case. Those of us who are betting on a respectable verisimilitude in the Freudian corpus must beware of taking substantive comfort in this indirect way, as some "Chomskyites" are currently taking comfort from (easy) refutations of unsound philosophical positions employed by certain of their S-R-reinforcement opponents. We must try to be honest with ourselves even though we are (as always in science) "betting on a horse race." It simply won't do to get relaxed about the dubious methodological status of, say, a postulated "bargain between ego and superego" as explaining why Smith cuts himself shaving before visiting his mistress, on the ground that the superego is a theoretical construct, and that's peachy, since physicists can't see the neutrino either!

Having mentioned the neutrino, I am led to a comment on falsifiability in the inexact sciences. You will recall that when Pauli cooked up the neutrino idea in 1931—solely to preserve the conservation laws *ad hoc*!—the theory itself showed that the neutrino hypothesis was probably not falsifiable, because the imagined new particle had zero charge and zero rest-mass. It was not until 1956, twenty-five years later, that a very expensive, never-replicated experiment by Reines and Cowan successfully detected the neutrino (more) "directly." The auxiliary assumptions involved (e.g., would the cross section of cadmium nucleus be large enough?) were themselves *so* problematic that a negative experimental result could just as plausibly have counted against *them* as against the theory of interest. While Popper's stress on falsifiability (and the correlative idea that theories become well corroborated by passing stringent tests) is much needed by the psychologist, partly as an antidote to the current overreliance on mere null-hypothesis refutation as corroborating complex theories (see Rozeboom, 1960; Bakan, 1966; Meehl, 1967; Lykken, 1968), it has become increasingly clear that a too-strict-and-quick application of *modus tollens* would prevent even "good" theories (i.e., theories having high verisimilitude) from getting a foothold. "All theories are lies, but some are white lies, some gray, and some black." The most we can expect of psychodynamic theories in the foreseeable future is that some of them are gray lies. My own predilection is therefore for a neo-Popperian position, such as is represented by Feyerabend (1958a,b, 1962a,b, 1963, 1964, 1965a,b, 1966, 1968, 1971, forthcoming), Lakatos (1968a,b, 1970), and Maxwell [1974]. But what precisely this methodological position means for the strategy of testing psychoanalytic theory is difficult to discern in the present state of the philosophers' controversy. My own tentative predilection is for stronger theories (Platt, 1964; but see Hafner & Presswood, 1964), such strong theories being subjected to more tolerant empirical tests than Popper or Platt seems to recommend. Discussion of this very complicated issue would take us too far afield, but suffice it to say that I now view the position presented in my 1967 paper as overly stringent, although its main point is still, I think, a valid one.

Perhaps the psychologist should first learn Popper's main lesson, including why Popper considers such doctrines as psychoanalysis and Marxism to be nonscientific theories like astrology (because all three are pseudo-"confirmable" but not refutable), and then proceed to soften the Popperian rules a bit. Whether these suggested "softenings" really conflict with a sophisticated falsificationism, or whether Popper himself would consider them objectionable, we need not discuss here. (But see the distinction between Popper_0, Popper_1 and Popper_2 in Lakatos, 1968a.) Specifically, I advocate two "cushionings" of the Popperian falsifiability emphasis:

1. A theory is admissible not only if we know how to test it, but if we know *what else we would need to know* in order to test it.

2. A theory need not be abandoned following an adverse result, if there are fairly strong results corroborating it, since this combination of circumstances suggests that either (a) the auxiliary hypotheses and *ceteris paribus* clause of the adverse test were not satisfied, or (b) the theory is false as it stands but possesses respectable verisimilitude (i.e., is a gray lie), or both.

I think that these are sensible methodological recommendations that can be rationally defended within a "neo-Popperian" frame, and they do not appear to me to hinge upon resolution of the very technical issues now in controversy among logicians and historians of science. But I hasten to add that such "softening" of the pure, hard-line *modus tollens* rule must not be accompanied by a theoretical commitment such that we persist indefinitely in what Popper stigmatizes as "Parmenidean apologies," clinging to the cherished doctrine in spite of all adverse evidence (Popper, 1965). *When* Parmenidean apologies are desirable, *which kinds* and *how long* to persist in them ("theoretical tenacity") are difficult questions. (To get the feel of them I recommend reading of Feyerabend, "Pro-Parmenides: A Defense of Parmenidean Apologies"; also Feyerabend, 1964.)

One big trouble with the application of neo-Popperian strategies to a theory such as Freud's is that the best case for either Parmenidean apologies or continuing use of a "gray-lie" theory in the face of strong and accepted falsifiers is the concurrent existence of strong corroborators, and this usually (not always) requires that the theory have made successful *point* predictions (i.e., predictions of antecedently improbable numerical values). The successful prediction of a mere directional difference is not of this kind, having too high a prior expectedness in Bayes's Formula absent the theory of interest. (If I am right, this atheoretical expectedness in the social and biological sciences approaches ½ as the power of our significance test increases [Meehl, 1967].) Yet an attempt to formulate psychoanalytic theory so as to generate such high-risk numerical point predictions is hardly feasible at present. For one thing, the auxiliary hypotheses which are normally treated as (relatively) unproblematic in designing a test experiment are unavailable pending the development of powerful, well-corroborated *non*psychodynamic theory (e.g., psycholinguistics). I must say that this state of affairs renders the prospects for cooking up strong tests rather gloomy.

From the standpoint of the experimental psychologist, for whom the experiment (in a fairly tough, restrictive usage of that term) is the ideal method of corroborating or discorroborating theories, the obvious drawbacks of the psychoanalytic hour as a data source are two, one on the "input" (= control) side and the other on the "output" (= observation) side. On the input side, unless the analyst's enforcement of the Fundamental Rule relies entirely upon the psychological pressure of a silence—a technical maneuver which is sometimes the method of choice but other times, I think, clearly not—we have the problem of the timing and content of the analyst's interventions as being themselves "biased" by his theoretical predilections. (It would be interesting to play around with the psychoanalytic analogue to a yoked-box situation in operant behavior research.) On the output side, the problem of "objectifying" the classification of the patient's verbal behavior is so complex that when you begin to think hard about it, the most natural response is to throw up your hands in despair. Tentatively I suggest two contrasting methods of such objectification, to wit: First, we rely upon some standard source such as Roger's *Thesaurus* or the Palermo-Jenkins tables or a (to-be-constructed) gigantic atlas of couch outputs emitted under "standard" conditions of Fundamental Rule + analyst silence, for determining whether certain words or phrases are thematically or formally linked to others. Such a "scoring system" bypasses the skilled clinical judge and hence avoids theoretical infection of the data basis. I need hardly point out its grave defect—so grave that a negative result would not be a strong falsifier—which is that the mainly idiographic theme indicators (those which make psychoanalytic therapy fun!) would be lost.

Alternatively, we permit the judgment of a skilled clinician to play a part in classifying the responses, but we systematically prevent his having access to other portions of the material (e.g., to the manifest content of the dream with which the patient commenced a session) so that he will not be "contaminated" by this material. Point: As much as any area of research in clinical psychology, the study of the psychoanalytic interview brings home the importance of solving, by ingenious methods, the perennial problem of "How do we get the advantages of having a skilled observer, who knows what to listen for and how to classify it, without having the methodological disadvantage that anyone who is skilled (in this sense) has been theoretically brainwashed in the course of his training?" In my view, this is *the* methodological problem in psychoanalytic research.

This brings me to my final point, which is in the nature of a warning prophecy more than a reaction to anything presently happening in psychoanalytic research. The philosophical and historical criticisms against classical positivism and naive operationism have (quite properly) included emphasis upon the role of theory in determining what, when, and how we observe. But most of the discussion of these matters has drawn its historical examples from astronomy, physics, and chemistry. In these examples, as I read the record, what the experimenter *relied* on in "making observations" was (relatively) nonproblematic and independently corroborated portions of the theoretical network for, say, constructing apparatus. The theory of interest was not "relied on" in that sense, although of course in another sense it was "relied on" in deciding what to do and what to look for. It seems to me important to distinguish these two sorts of reliance on theory, and if they are conflated under the broad statement "Theory determines what we observe," I think confusion results. Furthermore, it is misleading (for several reasons) to equate a mass spectrometer or a piece of litmus paper with a psychotherapist as an "instrument of observation." I seem to discern in some quarters of psychology a growing obscurantist tendency—partly anti-empirical but also even at times antirational—which relies upon the valuable and insightful writings of Kuhn (1962) and Polanyi (1958) for what I can only characterize as nefarious purposes. It would be unfortunate indeed if efforts to objectify psychoanalytic evidence and inference were abandoned or watered down because of a comfortable reliance on such generalizations as "Scientists have commitments," "We often must stick to a theory for want of a better," "You have to know what you are looking for in order to observe fruitfully," "There is no such thing as a pure observational datum, utterly uninfluenced by one's frame of reference." These are all true and important statements, although the last one needs careful explication and limitation. I do not think general comments of this nature are very helpful in deciding how much an analyst subtly shapes the analysand's discourse by the timing of his interventions ("uncontrolled input"), or whether he classifies a bit of speech as "anal" in a theoretically dogmatic manner ("observer bias in recording output"). If the exciting developments in contemporary philosophy of science are tendentiously employed for obscurantist purposes, to avoid answering perfectly sensible and legitimate criticisms, it would be most unfortunate. The good old positivist questions "What do you mean," "How do you know" are still very much in order, and cannot be ruled out of order by historical findings about where Einstein got his ideas! "Millikan relied upon a lot of physical theory, treated as unproblematic, when he 'observed' the charge on the electron" is a correct statement of the case. But such a statement is not, most emphatically *not*, on all fours with "Blauberman (Ross, 1961/1963) is a qualified psychoanalyst, therefore we can rely upon his use of psychoanalytic theory when he classifies a patient's discourse as phallic-intrusive." What one *observes* in the psychoanalytic session is words, postures, gestures, intonation; everything else is inferred. I think the "lowest level" inferences should be the main object of study for the time being—we should be objectifying and quantifying "low-level theoretical" statements like "Patient is currently anxious, and the thematic content is hostile toward his therapist," rather than highly theoretical statements like "He has superego lacunae" or "His dammed-up libido is flowing back to anal channels." In the process of such objectifying-and-quantifying research, I can think of no better methodological prescription than the one with which Aristotle sets the standards of conceptual rigor as he begins his consideration of ethics, "It is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject permits." No more—but no less, either.

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