

## If Freud Could Define Psychoanalysis, Why Can't ABPP Do It?

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### ABSTRACT

*It is doubtful whether Freud's (1914) definition of a psychoanalyst as one who makes transference and resistance basic to theory and therapy would be adequate today, although these two concepts do jointly imply some of the other core ideas (e.g., the unconscious, defense, conflict, importance of early life experiences). Certifying competence to practice psychoanalysis presupposes the empirical reality of a technical taxon, a question researchable by taxometric methods. Verbal, conceptual knowledge of classical and deviationist theory can be assessed by achievement test. Performance in conducting a psychoanalytic session can be cheaply and conveniently judged from tapes and protocols mailed to evaluators. Increasing heterogeneity and the unanswered challenges to technique and doctrine suggest that psychoanalysis may be a degenerating program, in which case ABPP should not embark on certification of analytic practitioners.*

Let me begin on a reassuring note to the American Board of Professional Psychology (ABPP). I do not mean to suggest any deficiency on its part, nor on the part of the psychoanalytic group. As I understand from my colleague Dr. Manfred Meier, the Board initiated some moves toward certification for the practice of psychoanalysis, but, confronted with the task of describing qualifications, apparently thought better of it. Assuming Freud's (1914) effort to demarcate psychoanalysis from related modes was adequate to the social situation in 1914 (which it may or may not have been), developments in theory and technique, and the efflorescence of a wide variety of therapeutic modes and theories of neurosis—each taking small, medium, or large portions from the corpus of psychoanalytic doctrine—would suggest that today's situation is *so* different that what was adequate as a definition in 1914 would not be adequate today. A more precise formulation of my title might be, "Was Freud's definition adequate seventy-eight years ago? How would it do today?"

For Board purposes I take the task as being to define psychoanalysis and, given that definition, to determine whether a candidate is competent at the procedure thus defined. When I served on the Board in the 1950s, I was not happy with the notion of certifying excellence, a purported rating on quality. All I would be concerned to determine is *whether somebody does what we call "psychoanalysis" and, if so, whether the quality of candidates' knowledge and performance is good enough to let them loose on the public with the label "psychoanalyst."* Some will disagree with such a modest specification of the certification task; but that need not prevent us from discussing how to proceed within such a minimal frame of reference.

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Whatever criticisms one may make of Freud as a theorist or practitioner, I don't believe he has ever been accused of being a muddy or obscure writer. He was a careful writer who said what he meant and meant what he said. We may safely assume that in one of the few polemics he ever wrote, the 1914 paper on the history of the psychoanalytic movement, he chose his words with care, since the whole point of that paper was to make clear how his creation, psychoanalysis, differed from the deviations of Adler, Stekel, and—a sad blow for Freud—Jung. There he said: “Any line of investigation which recognizes [the two facts of transference and resistance] and takes them as the starting-point of its work has a right to call itself psycho-analysis, even though it arrives at results other than my own” (p. 16). On first reading this definition, one thinks, “But, surely, that's far too broad, at least for today.” That is probably a sound criticism; but one must be careful not to treat a definition of this kind atomistically, as if the words used in it were unconnected with other concepts. Someone might object that the unconscious is obviously a core concept of psychoanalytic theory and fundamental to the rationale of the psychoanalytic technique; thus, a definition that leaves out that term is defective. The reply to that objection is that since we conceive resistance as defense manifested in the analytic session and defense as warding off anxiety-eliciting material from full awareness, there is an intrinsic link between the concept of resistance and the concept of the unconscious. If a therapist said, “In my interview technique, I recognize the fact of resistance, although I don't believe there are unconscious mental processes,” we would properly view that as a nonsensical remark. It is a truism among philosophers of science that all scientific theories contain both central and peripheral elements, concepts and propositions that are core, as contrasted with other concepts and statements containing those concepts that are peripheral (Meehl, 1990a,b). An experimental psychologist, for example, might claim to be a *neo*-Skinnerian; if asked what is “neo” about her theoretical views, she might say that, in order to accommodate the recalcitrant fact of latent learning, it is necessary to adjoin to Skinner's basic ideas about reinforcement, operant shaping, discriminative control, and strength a Tolmanlike postulate involving connections that Tolman would call “expectancies.” Skinner might not be very happy about that amendment, and the “neo-Skinnerian” might have adopted it with reluctance; but this would not lead us to say she was no longer entitled to label herself a “neo-Skinnerian.” But, if a neo-Skinnerian explaining the “neo” says, “Well, I don't agree that contingencies of reinforcement are important,” that makes no sense. Similarly, the meaning of Freud's concept of transference includes its role in a network of other ideas, such as the importance of childhood experiences and fantasies in determining the object relations of adult life, especially those irrational or unrealistic ones that develop toward the analyst. I don't mean to spin a whole ramified network of Freudian theory from transference and resistance, but merely to emphasize that anyone who has sincerely accepted these two notions has thereby accepted the unconscious and the importance of early mental life. This is the sheer *semantics* of the situation. The most powerful way of defining theoretical terms (according to philosophers of science) is not the “operational” definition, but the implicit definition, where the meaning of a theoretical term is specified contextually by its role in the postulated theoretical network (Cronbach and Meehl, 1955). You cannot accept transference and dismiss the child's erotic and aggressive life; you cannot accept resistance and deny the reality of the unconscious.

On the other hand, there are components of classical theory which I dare say Freud would be much distressed to see widely abandoned, but which—while more core than peripheral—cannot be derived from the two concepts in his 1914 definition. One might suppose, for instance, that the Oedipus complex could be shown to be central on this basis, but that is a mistake. Transference involves the unrealistic recurrence in the adult of psychisms derived from the past, but that does not suffice to characterize the *content* of those psychisms as oedipal. Similarly the alleged ubiquity of castration anxiety in men and penis envy in women, whatever its empirical correctness (I am not discussing that here), cannot be derived from the concepts of transference and resistance.

I shall have some things to say about theoretical orthodoxy and the core/peripheral distinction later; but for now, I am going to proceed backwards. I will first look at the psychoanalytic technique—that is, after all, what the ABPP candidate is accredited to *practice*. I will then inquire what is the minimum theory base in terms of which psychoanalytic technique can be rationalized (in the good sense of the logician's, not the analyst's, meaning of that word).

My current research involves developing new taxometric mathematics for the classification and genetic analysis of mental disorder (Meehl, 1973, chapter 12; Meehl and Golden, 1982; Meehl, 1992a) so I naturally ask a threshold question: Is the psychoanalytic procedure taxonic? Roughly, does the psychoanalytic interview constitute a species, a taxon, a type, a true category? Or is it merely an outlier on a set of quantitative dimensions? Are we talking about a difference in kind as well as a difference in degree? I presuppose that sufficiently highly clustered outliers on numerous differences of degree can be counted as a difference in kind, depending upon the distance and the tightness of the clumping. (As Marx and Engels said, there are differences of degree where quantity turns into quality.) Some psychologists are confused by the fact that the indicators of a species, type, or taxon are usually, when looked at closely, quantitative. But this does not refute the conjecture of a taxon. That a patient's body temperature is a quantitative variable does not prevent meningitis from being a disease entity with elevated temperature as one of its symptoms. Both the strategy and tactics of the psychoanalytic session occur in varying degrees among therapists who would call themselves analysts as well as among therapists who would not. The only exception to this is the use of the couch. (Even that is not quite dichotomous. I knew a psychoanalytically oriented therapist who put his patients on a recliner, partly because he was not an institute-trained analyst and his M.D. brother, in the same community, was. The recliner was a kind of trade-union concession. I always thought this was silly and still do. As I once told him, I totally reject the notion that my attendance or nonattendance at a medically controlled institute accredited by the International Psychoanalytic Association can validly determine my choice of office furniture when nobody would claim that such a credential can determine my choice of words during a session.) Space does not permit exposition of taxometrics, so the reader will have to take my word for it that there is a set of statistical procedures, applicable to quantitative indicators of a conjectured latent taxon, type, disease entity, species, syndrome, skill, or ideology, which—on the evidence to date with real data and extensive Monte Carlo runs—does an excellent job distinguishing taxonic from dimensional (factorial) situations.

I would combine two ways of identifying psychoanalytic practitioners. The first is like a recursive definition: A psychoanalyst is somebody who calls himself

a psychoanalyst and is called a psychoanalyst by others who call themselves psychoanalysts, who in turn are called so by those who do not call themselves psychoanalysts. There is nothing circular about this, and logicians and mathematicians routinely use this sort of definition. Of course, the usefulness of such a bootstrapping procedure by nomination of self and others—a kind of sociometric diagram—hinges upon interjudge reliability. The taxon is a taxon of *persons*, identified jointly by the social semantics of themselves and of others not claiming to belong to the taxon. We do not require perfect agreement in these sociometric choices, but we do anticipate that the distribution of choices will be bimodal. My prediction is that it would be U-shaped. Perfect initial stringent definition of the “seed” subset, about which *no* disagreement (either from within or without the tentative taxon) could arise, is not necessary.

Second, a collection of statements by practitioners, *prima facie* both within and without psychoanalysis, listing concrete and reliably checkable items about interview strategy and tactics, would be taxometrically analyzed. If there is a clear technique taxon and if it maps statistically onto the practitioner taxon, we will have shown semantic coherence in the concept “psychoanalysis.” If such taxonicity is lacking or if there is a poor mapping of technique upon practitioners, we should drop the whole idea; ABPP should not certify anything that lacks empirical reality. An interesting sociological puzzle arises if there is a taxon achieved by sociometric labeling, but the technique taxon is so loose as to map poorly onto it. One would entertain the dark suspicion that the mere prestige, at least in some parts of the country, of the label “psychoanalyst” is what is involved among therapists who are not practicing a helping mode similar to that of Freud. I think I can be fairly objective here, as it happens that in my part of the country the word *psychoanalyst* is not prestigious. In Minneapolis, most psychotherapists, whether physicians or psychologists, as well as the majority of the sophisticated public, tend to think of psychoanalysis as an interminable, costly procedure of little therapeutic efficacy.

Let me be clear what I am *not* saying about technique. I am not asking whether a practitioner treats all patients all of the time “classically.” The splits and schisms in the psychoanalytic movement and its intellectual isolation for many years from academic scientific psychology on the one side and from medical school psychiatry on the other have led to an unfortunate conflation of two questions which, however one may prefer to answer each of them, are distinct. First, is practitioner Smith competent in psychoanalysis *when using it*? Second, to what extent does practitioner Smith, competent in psychoanalysis, use it with some, most, or all clientele? A patient seeking psychoanalysis has a right to know that the practitioner self-labeled and institutionally labeled “psychoanalyst” *knows how to conduct an analysis*. But that is obviously not the same as holding that any such qualified practitioner will never say of or to a client, “I am a psychoanalyst, but for you I believe this is not the treatment of choice.” One can refrain from analyzing despite knowing how to do so; but one can hardly analyze if one does not know how to do so.

Confining ourselves to practitioners who are unquestionably analysts (if anyone is) and to those patients whom they consider analysands, I think we can predict what we would find statistically with respect to their strategy and tactics *in analysis* (whether they also do other kinds of therapy being beside the point here). We would find an almost invariable presence of the following strategic and

tactical properties: the couch; the fundamental rule (although imposed by training the patient to follow it, rather than by enunciating it as Freud did); interpretation—putting into words what the analyst discerns as a hidden process or theme in the patient’s words, postures, and gestures—as the fundamental mode of intervention, constituting almost all of the interventions, others (e.g., information giving, reassurance, Rogerian reflection, the ubiquitous “what occurs to you...” and “tell me more about that”) being preparatory or ancillary to interpretation; relative silence by the analyst, only a small percentage of words spoken in most sessions being the therapist’s; considerable reliance upon interpretation of dreams and encouragement to report them; the accumulation of reminiscences, especially those of childhood (although it seems that things have changed somewhat in this respect); a relatively high interview density [although Lorand (1946) insisted he had analyzed successfully on a once-a-week basis]; focus of interpretation on the transference; and the mutual expectation of a long series of sessions necessary for “completing” the process, insofar as it is completable.

Are these nine features of strategy and tactics statistically taxonic among today’s practitioners? Nobody knows from hard data. Foregoing fancy taxometric methods, suppose these nine were each formulated dichotomously, with care in locating the cutting score for quantitative ones (e.g., how many sessions per week?), and we asked practitioners, however they label themselves, to check off each as true or not true of their modal or preferred case. I would predict confidently that this distribution would be asymmetrically U-shaped, with negligible frequency of practitioners in the middle region. I expect a large J curve on the low side (the nonanalysts) and a clearly discernible but low frequency cusp on the high side. I am fairly confident that the practitioners found in the cusp would be self-described as psychoanalysts and that others who don’t call themselves analysts would so describe them. The only discordances would be persons (like myself) who learned analysis in some noninstitute setting and to whom the institute trained M.D. analysts refuse the label. This is not a list of excellencies but simply a list of practices, however well or poorly done. We know how a plumber differs from a carpenter, even if the plumber is a poor one!

As in other areas certified by the Board, the assessment of the candidate is partly based on verbal knowledge and partly on a practice sample. With respect to verbal knowledge, we have the vexed problem of orthodoxy, which is easily solved by making the distinction between *what one knows* about classical psychoanalytic theory and its modifications (e.g., by Horney, Rado, Sullivan, Fromm-Reichmann, and others) and *the extent to which one subscribes* to the various components of theory, classical or amended. It is simply the distinction between knowledge and belief. Another distinction within the domain of theoretical knowledge is between *core* and *peripheral*, and related to this—statistically but not always conceptually—first-level psychodynamic concepts versus metapsychology. I make that distinction not pedantically but because, having started with the identification of aspects of the strategy and tactics of how one conducts a psychoanalytic session, it is clear that some features of those practices are intimately tied to first-level psychodynamic theory (e.g., the existence of parapraxes, the standard dream symbols, the stream of associations being the resultant of psychic forces classifiable as impulse and defense, the standard list of twenty or so defense mechanisms, and others). One who didn’t subscribe to any of these aspects of the theory would be hard pressed to justify confining interventions to interpretation,

imposing the fundamental rule, using the couch, and so forth; whereas not a single one of the facets of technique listed above hinges upon such a core concept of Freud's metapsychology as the libido theory, or upon the division of the psyche into the three psychic institutions of ego, superego, and id (Schafer, 1976).

It may be objected that we will be abetting cognitive hypocrisy if we examine therapists on their knowledge of psychoanalytic theory, including the theory of technique along with the general theory of development, neurosis, and character, and do not include some kind of *assent* (analogous to requiring all Anglican priests to subscribe to the Thirty-Nine Articles or requiring Roman Catholic priests to utter Pope Pius X's antimodernist oath). I suppose that is possible in the abstract. But I find it hard to imagine somebody wanting to be certified as a psychoanalyst by ABPP who didn't believe in the unconscious, or defense mechanisms, or mental conflict as the source of neurosis. Here again, I suppose there is a possibility that—in some geographical areas or some subcultural groups—a prestigious halo around the notion of “analysis” might lead to such cognitive double-talk; but I should think it would be rare enough that we cannot afford to set up Board procedures with an eye to “catching” such semantic crooks. In discussing these questions I find myself asking the same thing I would ask about any other areas of Board certification as to competence, or what I ask when a clinical psychologist is taking a final oral in our department. Would I be comfortable referring someone I loved, a spouse or child or intimate friend, or a professional colleague or student who, for whatever reasons of personal growth or psychological impairment, wanted to have an analysis? It wouldn't bother me to know that Dr. Jones has doubts about the libido theory, or the universality of the Oedipus complex, or that the superego is the crystallization from the “resolution” of that complex. But I would be distressed if a self-labeled psychoanalyst didn't show any familiarity with these ideas. I wouldn't worry about the possibility of the therapist putting people on the couch, imposing the fundamental rule, and interpreting what they said in terms of unconscious processes of impulse and defense, *although not believing in the first-level clinical theory*, because I find it hard to imagine a psychologist wanting to do such a thing.

I trust it will not be interpreted as *n Aggression* if I use one of Freud's favorite metaphors, the military. *Tactics* covers such matters as when in the hour to interpret, or even one's choice of words (e.g., “you say that because of your fear lest...,” or “could it be that...?” or “perhaps this is again...?” or “let's try this on for size...”). *Strategy* involves a larger scale of planning; for example, being alert for a session where the experienced patient easily perceives a resistance to remembering *all* of a fragmented childhood episode, providing a favorable occasion for offering a historical construction that the analyst has corroborated by several high-risk (silent) predictions. What is the Clausewitzian *policy* that the strategy and tactics implement? It would be the theory of neurosis and of psychodynamics generally as source of the theory of treatment, the rationale of the psychoanalytic process. An adequate theory of technique would be based jointly on treatment experience (but the quantitative research is sparse and unclear) and general psychodynamics (but these constraints are too loose, and disagreements about them too numerous). Surely there must be *some* technical implications? Indeed there are. For example, over sixty years ago Franz Alexander, who became director of the first “orthodox” medical institute in the United States, suggested that any correlation between recall of repressed memory and beneficial effect is

due to the “corrective emotional experience” of the therapeutic relationship, the analyst being “safer” than judgmental parents, hence defense is less needful, hence the memory becomes available. But the remembering as such has little curative efficacy (Alexander [1930], cited in Alexander and French [1946, p. 20]). (Alexander and French explicitly trace here the influence of the 1923 book by Ferenczi and Rank, in which Abraham and Jones smelled the heresy, but Freud defended by denial.) The recall is merely spinoff according to Alexander; whereas Freud all his life attributed an intrinsic healing influence to the lifting of repression, doubtless because of the great insights having come from the Breuer–Freud hypnocratic method. Presumably this major theoretical difference would influence one’s strategy and tactics. Marshall Edelson (1988), for example, a medical institute trained analyst who is the most methodologically sophisticated of anyone writing on the subject, and who is critical of conventional analytic scholarship on epistemological grounds, nevertheless subscribes to hyperconservative substantive views. He holds that an essential element of all psychoanalytic explanation—of symptoms, dreams, parapraxes, character traits, associative linkages and gaps—is a *sexual fantasy*, or defenses and fears concerning such (pp. 110-111, 216, 218). Aggression, success striving, dependency, are psychic realities, but they are derivative, not primary; and they do not play the specific etiological role that an infantile sexual fantasy does. Obviously that theory will influence strategy differently from a heterodox perspective like mine, where the whole list of twenty Murray needs (Murray, 1938; Meehl, 1992b) is available for explanatory purposes. Understanding a certain psychism in a brain surgeon on the basis of, say, *n Nurturance* fused with *n Recognition* could be adequate for me; whereas for Edelson such an explanation would be either erroneous or, at best, superficial, requiring more analysis to be done to get at the real, underlying motive. Such considerations mandate that ABPP certification of technical competence will have to be relatively relaxed with respect to theory.

In assessing candidates’ *theoretical knowledge*, we face the old problem of the written examination format. I presume psychoanalytic psychologists would tend to have a strong leaning in favor of the essay examination over the multiple choice test, but I am going to be blunt and challenging about this mistake. Research on the essay examination and its competitor the multiple choice test goes back into the middle 1920s, and surveys of the research literature make it quite clear what the situation is (Tyler, 1934; Page, 1966; Coffman and Kurfman, 1968; Coffman, 1969, 1971; Hopkins and Stanley, 1981, Chapter 8; Dwyer, 1982; Mehrens and Lehmann, 1984, Chapter 5). I am surprised that many Ph.D.s in psychology never learned about this research evidence, although the social sciences are noted for their noncumulative character and a tendency to reinvent the wheel. While there have been refinements in statistical method and a few interesting novelties (e.g., the computer can grade essay exams indistinguishably from human readers [Page, 1966]), *all* of the main arguments on the essay/objective test issue were adequately dealt with by Ruch (1929) over a half-century ago. No matter how carefully each is constructed (and usually, as in ordinary college examining, both kinds are constructed sloppily), *both* the essay format and the objective exam have their respective incurable defects. The incurable defect of the multiple choice test is that it cannot assess the examinee’s ability to exposit ideas in a clear and persuasive way. Notice I said *exposit*, I did *not* say that the multiple choice test cannot assess ability to think—a cliché that has been repeat-

edly shown to be false. (As an undergraduate I took the National Examination in physics because our physics department was cooperating with the builders of it; I can assure you, that exam tapped my ability to set up a problem and find a solution more than any essay examination in psychology or sociology ever did.) To sample the examinee's ability to formulate a problem, think through the solution, and apply it correctly one must, of course, construct items with care and concern for tapping into reasoning as well as recall of facts and principles. But it cannot sample expository skill.

The essay exam's incurable defect is due to time constraints. Nobody has ever figured out a way to tap knowledge of a heterogeneous domain of facts, principles, concepts, and methods in a reasonable amount of time. If the time allotted for responding to a single discussion item is less than a half-hour, the essay format loses its alleged distinctive advantages. Suppose you have available two hours of essay time. Then you can offer the examinee four items to write about. Even if we confine the knowledge domain to, say, theory of technique and theory of neurotic and psychotic symptom formation, the theoretical knowledge domain cannot conceivably contain less than fifty narrowly specified themes, topics, or principles. Thus the test sample is less than 10 percent of the domain to be covered. Without a large-scale and expensive series of studies which would constitute a major research enterprise in itself, we have no accurate way of estimating the representativeness of four such items or their difficulty levels. The result is that the examinee is subject to a large element of "luck" in what four items are presented. The common solution of allowing choices—"Answer one of these two"—does not solve the problem. There is no assurance that the two members of a pair have the same difficulty level or lead to the same grader expectations. Further, the research shows that in choosing between alternatives, examinees lack a reliable appreciation of which one they will do better at. No one has concocted a solution to the domain coverage problem that has even unresearched plausibility, for the simple reason that it is due to physical limitations of time, speed of writing, and speed of thinking.

Thus, in the competition between a carefully built essay examination and a carefully built multiple choice examination, the essay suffers from an incurable defect regarding domain coverage and hence fairness to many individuals. The multiple choice test suffers from an incurable defect if the examiner wants to assess expository skill, style, and persuasiveness. What is the relative badness of these two defects? For my part, I am not interested in assessing persuasiveness in a psychoanalyst, except in the psychoanalytic session itself. Why try to assess that skill indirectly in an essay on theory instead of directly, in the practical examination, evaluating an interview protocol? The old research on specificity shows it would be a poor bet to try to estimate the persuasive power of a candidate's analytic interpretations based on expository skill in an essay on Freudian theory! As a general principle, psychologists have learned that, for most situations, the best way to assess a behavior domain is to sample the behaviors you want to evaluate rather than some other kind of behavior. That is a truism of psychometrics which I assume everybody accepts.

There is a second grave defect in the essay exam that is curable but is not treated with the respect it deserves and, hence, is rarely cured. That defect is *unreliability of grading*. The shocking finding that different graders disagree markedly in evaluating the quality of essay examinations began with the classic studies of

Starch and Elliott (1912, 1913a,b), although I believe that Sir Francis Galton and Harold Edgerton adumbrated this in some research in the 1880s. Interjudge reliability coefficients of the usual “globally” graded essay exam range from the .20s to the .80s and typically fall in the .50s or .60s. Since the square root of the reliability coefficient sets an upper bound to validity, no matter how qualitatively superior the essay format is in generating the kind of cognitive behaviors we want to assess, *none of that intrinsic qualitative validity does us any good* if the behavior product cannot be *reliably* assessed. I hope I need not instruct psychologists that this statement does not hinge upon their ideology or their theory of the mind or their preferences in education or anything of the sort; it is a theorem of high school algebra concerning the validity coefficient. Even if the cliché were true—that multiple choice tests cannot assess a person’s ability to think, and essay exams can do so—it would be vitiated by the finding that readers cannot reliably grade the results of such thinking.

Since the statute of limitations for an action in tort against the Board has long since passed, I permit myself to recount a horror story from my service on the Board in the 1950s. Being the first nongrandfather to serve on the Board, I had taken the exam and was quite dissatisfied with it. Four of us—Kenneth Clark, Ed Bordin, Ed Henry, and myself (I cannot refrain from noting that this is three Ohio State Ph.D.s from the age of Toops and a Minnesota Ph.D. trained by Paterson and Hathaway)—insisted on studying the pairwise interscorer reliability coefficient of the essay exam on research. Behold, the scoring reliability achieved a magnificent value of  $r = .26$ ; so the fate of the poor ABPP candidate hinged 6 percent on what he produced and 94 percent on chance. Ed Henry, from his experience at the Army Command and Staff School, advocated adopting what the military call the “school solution” system of scoring, generally referred to (among educational psychologists) as “analytical scoring,” distinguishing it from the usual “global” scoring of essay exams. Readers are provided with a master list of ideas to check off, whether terms, facts, definitions, principles, arguments, or correct answers to a problem. Weighted or unweighted, these points are summed to get the score. Being the most passionate objector to the unreliable test that I had been subjected to, I suffered the usual fate of an advocate, namely, I was delegated by the Board to concoct an essay exam on research with an associated school solution that could be reliably graded. I did so for two or three years. The acme of my career as an essay examiner was describing a fake experiment that contained twenty-nine defects, ranging from tables whose degrees of freedom did not add up to subtle mathematical points about differences in two second derivatives generating a spurious interaction effect. I am pleased to report that this analytically scored essay test had a scorer reliability of .86. To keep the subjectively oriented scorers happy one permits a global scoring as well, but you can just as well throw it in the wastebasket because, when you have the analytical scoring number, the global rating contributes nothing except unreliable variance. (I was horrified to discover that in a phony experiment containing twenty-nine errors, candidates with Ph.D.s in clinical psychology from accredited programs in some instances could not detect more than 2 or 3!) The rationale underlying that approach is that most clinical practitioners do no research, are not interested in doing it, probably would not do it very well if they were forced to do so. It is foolish to examine them on research production. What matters is that a practitioner can *assess* research and not be seduced by a faulty study into using a poor diagnostic or

therapeutic approach, to the detriment of the patient and the taxpayer. We want to evaluate clinicians as *consumers* of research rather than as (potential but not actual) *producers* (Meehl, 1971).

Critics of objective tests don't understand that if you construct a multiple choice test carelessly, it will do at least as well, and, given the domain sampling and reliability problems, probably better than an essay exam constructed and graded with equal carelessness. Whereas if you put enough effort into construction and analytic grading of an essay exam, that same amount of effort devoted to a high quality objective exam will yield more assessment information. All of this reasoning is premised on the idea that the point of a written exam is not the assessment of therapeutic skill or the therapeutic personality, but of verbally formulable theoretical knowledge, including theory of technique. If theoretical knowledge is irrelevant to psychoanalysis, then we need not bother with *either* an essay or multiple choice test. An ingenious, indefatigable, and largely successful effort to construct achievement tests, having the merits of both essay and multiple choice format but lacking the defects of either, is to be found in Ralph W. Tyler's classic monograph (1934). Unfortunately, the Board's resources do not permit us to implement his powerful ideas, which have rarely been put into practice and are now generally unknown or ignored.

My own preference would be to construct a multiple choice test with stratification of domains, the percentage representation being determined by ratings on importance by Board members and diplomats. We might even want to get such judgments from the unsuccessful examinees, perhaps assigning them less weight but recognizing that maybe some of them flunked because we were behind the times in domain representation. The (internal consistency) reliability of an item should be determined with reference to other items in the domain. I would not include an essay exam unless it was clear what I wanted it to tap that could not be assessed with a high-quality multiple choice test, and I would insist upon its being scored analytically. As to its content, I suggest the task should be reasoning about the pros and cons either of a case or of a controversy about interview technique.

Now for the practical examination. Here my ideas are radical and heretical, motivated by minimizing cost, travel, and time. Basic principle: It is quicker, easier, less costly, and less trouble for all concerned to mail transcripts and tapes than to transport humans. In my experience on the Board, when evaluating therapy protocols there were a few clear flunks, a solid majority of equally clear passes, and not more than 10 or 15 percent "doubtfuls." It is wasteful to gather a half-dozen busy practitioners or professors and the candidate to discuss a therapy protocol, except for those few doubtful cases. I realize here I go against a basic feature of human nature which—it has always been a puzzle to me—finds great satisfaction in having a meeting. I am convinced that for many persons, both in and outside of the academy, "having a meeting" constitutes a significant part of their social, sexual, and intellectual life. So I have little hope of persuading most people on this score. I would ask the candidate to submit a "good" recorded session, and the tape, with transcript, would be mailed to one member or ex-member of the Board and to one other diplomat. A case could be made for using two different "good" sessions, but I shall keep things uncomplicated for this discussion. The instructions to the evaluator are simple and global: First, "Is this psychoanalysis?" Second, "Rate the quality of technique on a 10-point scale, with a cutting score below which is inadequate." I prefer the tactic of the industrial

psychologist that uses a combined regressive and successive hurdles psychometric model. If both evaluators say “pass,” that is the end of it. If both say “fail,” that is the end of it. But if one says pass and one says fail, then we pool the quantitative judgments and apply a cutting score to their sum.

It may be desirable to specify some quantitative *difference* between the ratings which would result in the protocol being submitted to a third judge, whose vote would be decisive. Finally, it should be possible for a candidate who has failed (including being flunked on the decision of the third judge) to appeal from this procedure, either by submitting yet an additional tape or by taking an oral examination in which the candidate’s technique can be defended, given the puzzle that three competent practitioners have disagreed. Over a sufficient time period we could calibrate the judges as industrial psychologists sometimes calibrate raters.

When I chaired oral examinations on therapy protocols as a Board member, I sometimes had to intervene vigorously to protect the candidate from an examiner who dogmatically labeled something “poor technique” because it wasn’t what the examiner would have said. At times this ideological rigidity was so extreme that a tactic was judged from an inappropriate frame of reference (e.g., a psychodynamic examiner condemning an early RET practitioner for “encouraging intellectualization”). I think it likely that we would have a similar problem in evaluating psychoanalytic protocols. In the 1930s Edward Glover sent a questionnaire to the members of the British Psychoanalytic Society, a homogeneous, close-knit group whose members had more regular contacts than is usual, and many of whom had been analyzed or controlled by the same analyst, or the same analyst once removed (Glover, 1940). The variation in strategy and tactics was surprisingly great, even on what one might consider fundamental matters (e.g., When during the hour to interpret? Attach special value to childhood memories? Permit relaxation of the association rule? Regard transference analysis as the main therapeutic device?). At least one-third of the respondents disagreed about 57 of the 63 total items; opinion was evenly divided on 21 items, among them being tracing themes to produce conviction, interpreting symbols, and offering constructions to aid memory. The *only* item of Quaker unanimity was avoidance of technical theoretical jargon—and the eminent Hungarian analyst Sandor Lorand disagreed even with that!

Thus when we speak of “classical technique” it is not clear just what we mean. It can safely be assumed that the variability among British analysts around 1932 must have been considerably less than that among nonmedical American analysts, trained in a variety of settings, in 1992. I can attest to marked differences between my first analyst, Vienna trained in the late 1920s, and my second, a product of the Columbia Psychoanalytic Clinic during Rado’s directorship. (Although I must emphasize that no informed listener to those sessions could possibly doubt that what transpired was psychoanalysis rather than, say, RET or nondirective therapy. The interesting Q-sort research of Fiedler [1950a,b, 1951] in the late 1940s has been greatly overinterpreted by some in this respect.) The historical origins of these variations are obvious. Freud never wrote the promised book on technique. There was no technique seminar in Vienna until Wilhelm Reich started one in the middle 1920s, where he observed that even such a core technical tenet as “interpret defense before impulse content” was frequently violated. Most of the first-generation analysts were not analyzed, and we know that the main way one learns “how to do it” is in one’s own analysis. (Psychology

students who have had no couch time usually have a bizarre notion of what an analytic session sounds like.) Tape recordings have never played the role in analytic supervision that they take in the Ph.D. internship experience (credit for the latter being largely due to Carl Rogers and his students). To one trained in scientific psychology, it is absurd to supervise a procedure that involves words—including parapraxes, subtle thematic allusions, speech rate, pauses—without tapes to allow the controlling analyst to hear things the controllee might not have noticed. There is also an element of defensiveness, mentioned by Fenichel in his 1936 lectures on technique at the Vienna Institute (Fenichel, 1941). Robert Knight, in his presidential address to the American Psychoanalytic Association 40 years ago, said, “I believe we may all be a little afraid that we are practicing analysis somewhat differently from the way others are doing it, and perhaps a little ‘improperly’ in terms of being less orthodox, introducing more modifications which we regard as necessary for the patients we treat, and so on” (Knight, 1953, p. 220). The “apostolic succession” has in reality preserved a variety of technical traditions, despite the core of interpretation as main tactic, fundamental rule, and so on. If there were some way to pair off the session evaluators for technique variation, I would favor it, but it would require the evaluators themselves to submit tapes and protocols for the Board’s use in pairing them.

Perhaps the candidate should be allowed to specify the orientation of judges; for example, someone might want the transcript to be judged by a Sullivanian, somebody else by a disciple of Rado or Horney. How much “label” deviation should be allowed in this choice? The obvious criterion would be that we don’t include in our list of possible alternative ideologies anybody who avoids the label “psychoanalyst.” (Adler abandoned the word *psychoanalysis* for what he did, as did Rank. I don’t know what to say about Jung.)

I say nothing about the power of psychoanalysis as a mode of healing, except this: If the Board consensus is that it doesn’t help patients to any appreciable extent, obviously there is no point in certifying competency. We have little to go on, except that broadly “psychodynamic” or “psychoanalytically oriented” therapy shows up in a respectable position in the ranking by Smith, Glass, and Miller (1980) based on their meta-analysis of outcome studies. That a patient with a monosymptomatic trauma-based phobia mistakenly seeks psychoanalysis rather than desensitization therapy is unfortunate, but that tells us nothing about whether the practitioner practices psychoanalysis competently or not. All things considered, reflecting on my own analysis and patients I have seen, and in comparison with my recent work using a mixture of psychodynamic and rational emotive therapy, I am inclined to agree with training analyst Philip Holzman who thinks psychoanalysis provides an excellent growth opportunity for a clientele he labels the “worried well,” a nice phrase upon which I could not improve.

I conclude with a reminder that should not be necessary but apparently is, because some people talk and act as if they didn’t know it, although surely everyone does. For years I have been puzzled by psychologists’ tendency to be immobilized in practical decision making by the absence of absolute Quaker unanimity. Our discipline provides effective sophisticated techniques for adjudicating matters of partial agreement, whether as to facts, values, or aims; but we often forget these scientific methods when we are not “doing science,” reasoning as if no

social action is possible unless every participant believes and feels exactly like every other.<sup>1</sup>

Suppose, for example, a group of scholarly practitioners is faced with the task of specifying what knowledge is “core” to a certification of competence. Discovering that some hold topic A to be core and others do not, we need not throw up our hands in despair, “Alas, not everyone agrees completely—*so we cannot decide to do anything.*” This reaction is absurd, and if generalized would lead to total paralysis in conducting human affairs. If that were the principle, no society, legislature, scientific group, or neighborhood bridge club could get off the ground, nor would we have the Constitution of the United States. We establish a sufficiently high percentage of agreement to avoid total fractionation, and then we tell the subset of persons who feel very strongly against something that the rest of us can agree upon that they must either change their thinking or simply resign themselves to not joining our club. I, for example, look upon the couch as a more important feature of what I call psychoanalysis than I do the accumulation of childhood reminiscences. But I am sure many will feel strongly the other way. How does a scientific psychologist deal with such problems? I describe only one of several approaches, not claiming that it *optimizes* but that, to use Simon’s handy concept, it *satisfices* (for any rational nonperfectionist).

We identify a rater group by seed + iterative cooptation, professionals who label themselves and others in the group as “X” (e.g., psychoanalyst). Tight criteria of mutual sociometric choice are employed *at this stage*, as we can study the effect of loosening inclusion criteria later on. An open-ended format is used to elicit suggestions from them as to what are core features (of education, supervision, tested verbal knowledge, or whatever). These items are put on cards which judges Q-sort as to “coreness,” “centrality,” or “importance.” Factoring the matrix of interjudge Q-correlations, we weight the judges by their first factor loadings, and each item’s value is derived from the composite of these weighted judgments. Ordering the items by value, we simply move down the list until the number of items reaches a pragmatically acceptable limit (i.e., one that we estimate people can, or will, put up with). A dispersion index may be calculated for each item, thus defining a 2-space of “average coreness” × consensus. What is so difficult about this for a social scientist? Nothing—provided each agrees beforehand not to be intransigent if clearly out-voted. We can also agree beforehand that if the correlations are low, the enterprise should not be pursued further. Similarly, on the performance sample side, if the interjudge reliability cannot be raised much higher than the  $r = .26$  (mentioned above for ABPP’s old research essay exam), it would be foolish for the Board to attempt certifying psychoanalytic technique competence.

#### ADDENDUM

I did not discuss the scientific status of psychoanalytic theory, whether of neurosis or technique. Presumably the latter should be “derivable” from the former, but on the current scene that conceptual relation appears fragile. I do not

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<sup>1</sup> My undergraduate advisor, Donald G. Paterson, and my doctoral advisor, Starke R. Hathaway, taught me to think like a psychologist all the time—not only in the laboratory, clinic, library, or classroom. It is remarkable how many Ph.D.s in psychology never learned to do that.

think it hypercritical or pessimistic to say that psychoanalysis today exhibits symptoms of a “degenerating research program” (Lakatos, 1970; Meehl, 1990a). One need not conduct a literature search to realize that the divergencies in theory and technique among therapists in a broadly “psychoanalytic” tradition are vast, increasing, and show little or no signs of the sort of cumulative, self-corrective, convergent development characteristic of post-Galilean science (cf. Allers, 1940; Wheelis, 1956; Fierman, 1965; Fromm, 1970; Meehl, 1970, 1983; Schafer, 1976; Silverman, 1976; Fisher and Greenberg, 1977; Wachtel, 1977, 1982; Farrell, 1981; Malcolm, 1981; Eagle, 1984; Strupp and Binder, 1984; Grünbaum, 1984, 1986; Cooper, 1986; Weiss, Sampson, and the Mount Zion Psychotherapy Research Group, 1986; Haynal, 1988; Dinnage, 1988; Edelson, 1988; Abend, 1990; Goldberg, 1990; Langs, 1990; Masson, 1990; Weinshel, 1990; Auld and Hyman, 1991; Eagle and Wolitzky, 1992). It is hard to mention a single component of theory or technique that has not been challenged by someone in the broadly defined “psychoanalytic tradition.” To give just one example, focusing on transference interpretation has been a core feature since the 1920s, but recent process research by the highly sophisticated Mount Zion group puts the technical merit of that doctrine in jeopardy (Silberschatz, Frettnner, and Curtis, 1986). That social fact, whatever its origin, might suffice—*would* suffice, for many psychologists—to warrant ABPP declining to embark on accreditation of such a dubious specialty.

A second topic I avoided was the recent legal “success” by psychologists in compelling the medical analytic institutes to admit nonphysician candidates. I am insufficiently familiar with the details to discuss this subject properly, but my opinion is unfavorable. First, I think we psychologists should resist the idea that the medically controlled and approved institutes are the only context in which one can learn psychoanalysis, whereas the lawsuit itself seems to concede that. Second, many believe that the institute system was (in the long run) a bad thing, both for academic psychology and psychiatry departments on the one hand and for the psychoanalytic movement itself on the other (see Holzman, 1976). Third, my libertarian leanings lead me to dislike litigation of this sort as a general principle. I am troubled by the notion of our using state power to compel physicians who believe (I think wrongly, as Freud did) that a medical training is desirable for a psychoanalyst to educate nonphysicians. Suppose persons with a high-school diploma or with a B.A. in Romance languages insist that Minnesota’s psychology department admit them to the Ph.D. clinical program. Can we prove, by hard data, that they are unsuitable? We surely cannot. But do I want some Federal judge in his infinite wisdom to command us to admit them? No, I surely do not.

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