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THEORY AND PRACTICE: REFLECTIONS OF AN ACADEMIC CLINICIAN[1]

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My title, which could be put feistier by asking, “What, if anything, holds us together?,” has been a recurrent theme in thinking about our strange profession at least since World War II. At times, this theme has taken on crisis proportions, where the question seemed to be, “Are we about to fall apart?” I do not propose today to discuss training, licensure, third party payments, and the like. Rather, I want to focus on some philosophical problems about the relation of theory to practice, between basic science and the healing arts, which I hope you will find interesting. These theoretical issues are related to such mundane matters as money, as we all know if we are honest with ourselves. For example, fear of psychologists as professional competitors played a role in some of the formulations of the controversial DSM-III, and the distinguished chair of that task force, Dr. Robert Spitzer, has given public lectures entitled “The Politics of DSM-III.” On the other side, one suspects that the distaste of some clinical psychologists for organic medical procedures, such as psychotropic drugs and EST, is related to the fact that these are modes of healing that we are not competent in or legally permitted to employ.

I doubt that there is a single academic subject matter, classified under a given administrative umbrella, in which both the substance and the research methods are so heterogeneous as in a psychology department. I lunch frequently with my brethren in experimental psychology and, except for the fact that, like me, they know a little

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undergraduate mathematics, discussions of scholarly subject matter across that table frequently sound like informal lectures. For example, Professor Burkhardt is explaining to me that graded potentials play as important a role in visual perception as axon spike, or I am explaining to him what we mean by "bootstrapsing" in taxometrics. It is possible to find two competent academic psychologists who, if they had lunch together, would be forced to discuss the Twins' chances for the pennant or Ronald the Red Killer's showmanship talents, because they would have negligible overlap in their knowledge and interests in psychology. One can inquire as to why this is, whether anything can be done about it, or—a question that should be asked first—does it really matter anyway? Why *should* a clinician classifying schizophrenia be able to converse with an expert on the electrochemical processes in the retina of the walleyed pike?

I'm afraid that I believe, at age 67, that the Old Oaken Bucket delusion as regards the integration of psychology is partly true. It would be hard to convey to the young people in this audience the flavor of "integrative optimism" that prevailed among psychology faculty and students in 1941, when I entered graduate school. When I talk about this to students or junior faculty, it seems that our attitude in this matter 45 years ago strikes them as being terribly naive. But, brethren, consider the great books that appeared in the decade 1935-1945, when I was an undergraduate and graduate student, receiving my doctorate the year that World War II ended. One thinks, for instance, of Dollard's *Criteria for the Life History* (1935), Thurstone's *The Vectors of Mind* (1935), Miller and Dollard's *Social Learning and Imitation* (1941), Allport's *Personality: A Psychological Interpretation* (1937), Murray's *Explorations in Personality* (1938), Dollard, Doob, Miller, Mowrer, and Sears' *Frustration and Aggression* (1939), and Hull's *Principles of Behavior* (1943). (I have omitted the most important single book of that period; namely, Skinner's *Behavior of Organisms* (1938), because only a few of us at Minnesota appreciated its earth-shaking significance.) These "great books" of that decade were produced by first-class intellects with quite different biases and interests and of almost zero overlap in research technique. However, it was possible for a person who was neither stupid nor hysteroid to see in them the signs of rapid advance and intellectually satisfying integration. Thurstone was telling us how to identify the individual differences factors of the mind, Hull was mathematicizing the laws of learning, the Yale group was translating Freudian concepts into learning theory and doing ingenious experiments to show reaction formation and displacement in the rat. While I don't suppose any of us had the crazy idea that psychology was practically on the threshold of becoming a field like chemistry or physics, these exciting developments made it reasonable to think that it wouldn't be very many years before a large integrative job between the clinic, the laboratory, and the mental testing room would be accomplished.

One change that took place shortly after this decade, a change that I helped bring about as a member of the Dartmouth conference on learning theory in 1950, was what has been called "the death of the grand theories," even within a restricted domain such as animal learning. Part of the trouble with the era of grand theories (Hull, Tolman, Guthrie, and Co.) was the psychologist's obsession to be more like a physicist, which led him to take Newton as the general model of all science. That meant focusing on only one *kind* of theory, when the history of the other sciences shows that there are at least three kinds of scientific theories, all important and all intellectually respectable. In my terminology, we *have functional-dynamic theories* (e.g., classical mechanics or thermodynamics), whose paradigm is systems of differential equations telling us how certain variables change over time in relation to others. Second, we have what may be called *structural* or *compositional theories,* which tell us what something is made of, what kinds of substances or parts it includes, and how they are put together. Examples would be theories of chemical structure like the benzene ring, the periodic table itself as a list of the kinds of substances out of which other more complicated substances are made, and the DNA theory. Third, there are *developmental* or *historical theories,* such as the big bang theory in cosmology, the theory of continental drift in geology, and Darwin's theory of evolution. We were wrong in focusing only on functional dynamic theories as if the other two types were somehow less interesting or respectable. In fact, structural or compositional theories are among the most important kinds of scientific breakthroughs.

In psychology, Skinner, Hull, and portions of Freud are functional dynamic; other portions of Freud are structural, although in a funny way; and psychophysiological notions like Hebb's are also structural. Finally, Freud's theory of libido development or Piaget's theory on the development of cognition are historical. Each of these kinds of theories has its own criteria of evaluation and, in a sense, its own theoretical purpose. They are not sharply distinct; a structural theory of how a grandfather clock works explains why the clock runs faster in the winter in terms of contraction of the metal in the pendulum arm and Galileo's law of the pendulum. This explanation invokes structural statements about the arrangement of the clock's innards, but derives molar properties of the whole structure by making use of functional dynamic principles of mechanics and heat. It seems that one of the difficulties with psychology is that connecting theoretical levels after the manner of the grandfather clock is harder to accomplish, which leads many psychologists to say that the attempt should be set aside until such time as it falls into place almost automatically, a position that Skinner takes with regard to molar behavior's relation to the nervous system.

When I was a student, at least one common factor was present in all of the psychology faculty, scholars with such different approaches as Paterson, Heron, Hathaway, and Skinner; namely, *the general scientific commitment not to be fooled and not to fool anybody else.* Some things happen in the world of clinical practice that worry me in this respect. That skepsis, that passion not to be fooled and not to fool anybody else, does not seem to be as fundamental a part of all psychologists' mental equipment as it was a half century ago. One mark of a good psychologist is to be critical in evaluating evidence. In this respect, one would have a mentality somewhat like an efficiency expert, a prosecuting attorney, or a detective. I have noted some psychological court testimony in which this critical mentality appears to be largely absent.

It is not a question of whether one abandons "scientific standards of proof," because one is operating in a clinical context where hard data may be hard to come by. It is more than that. It has ethical implications when I make life-and-death decisions about people and collect the patient's or the taxpayer's dollar for doing so by using a diagnostic procedure that has been repeatedly shown to have negligible validity. A deep pervasive dimension that separates psychologists in these matters is the Russell-Whitehead distinction between the simpleminded and the muddleheaded. This difference has little or nothing to do with being bright or dull, since we find brights and dulls on both sides. In the research context, I sometimes have the impression that simpleminded psychologists have a hard time discovering anything interesting, whereas muddleheaded ones discover all sorts of interesting things that are not so. The simple-minded have a tendency to be hyperoperational, too closely tied to rigid standards of evidence (often based upon misconceptions of both philosophy and history of science), and a distaste for explanations that seem to them needlessly complex. The muddle-headed may be on better grounds ontologically, since the world is complicated and the human brain is at least as complex as the kidney. The problem about the muddleheaded is less in their preference for certain classes of explanatory concepts than it is in their weak standards of evidence.

I have asked myself which of these two cognitive disorders is more serious, and I do not come up with a clear answer. I have, however, noticed one fact that gives me a slight preference for the simpleminded. If you work hard at it and are ingenious with your clinical examples, you can sometimes arouse the intellectual interest of a simple-minded psychologist and get him to see that things are a little more complicated than he had imagined. Simplemindedness as a methodological orientation is, in some cases, curable. But I have never known a muddlehead to get well. Muddleheadedness is an incurable intellectual disease. I think I understand the reasons for this difference. To "fix" somebody, to cure somebody, you have to have some kind of leverage. While the simpleminded has a bias, you can get leverage because of his commitment to explaining evidence. If you present the right kind of evidence, it will grab him. But you don't get any leverage on a muddlehead, because muddleheadedness itself immunizes its victims from critical objections. You can't make him bothered by the fact he thinks sloppily, because part of muddleheadedness consists of not knowing that one is thinking sloppily.

In discussing the relation of basic science (and of quantitative research at the clinical level) to clinical practice, it is imperative to make a certain distinction. The pragmatic context forces us, as clinicians, to make decisions—whether it's a decision about what to tell a judge, or whether a patient should be seen inpatient because of suicide risk, or whether to offer an interpretation at a certain point in a therapy session—on less cogent evidentiary grounds than we would prefer in a research seminar. There is no need to be apologetic about this. All applied sciences, whether engineering or dentistry or accounting or clinical psychology, of necessity permit the practitioner to make judgments in certain settings that he would not care to defend as part of a PhD thesis or a scientific article. When the scientifically-oriented clinician criticizes some clinicians as "unscientific," it is not this unavoidable decision making in the pragmatic context that is objectionable. One should be clear about that, lest one sound like an obsessional perfectionist purist. The trouble with some current clinical practices is persistence in approaches (whether diagnostic or therapeutic), despite clear *negative* evidence against their validity or efficacy. To say, "Well, the scientific evidence is not clear on this but I have to do something for this patient," or "I have to tell the judge something" is not, repeat *not,* the same as saying, "I don't care if the research evidence on the Minnesota Tennis Ball in Bushel Basket Projective Test shows it doesn't predict anything, I'm not in a laboratory, I'm in a clinic, and so I'll use it anyway." This latter is not only intellectually disreputable, it is unethical.

I am not suggesting that only scientific data in quantitative form warrant alteration in one's belief system and, hence, one's clinical practice. Accumulated clinical experience, including conversations with experienced colleagues, is an admissible source of "soft" evidence, as it was for many years in medicine. But, granting this, we should keep in mind how many theories and practices in old-fashioned medicine, before the rise of laboratory medicine, controlled experimentation, and the application of suitable statistics to clinical trials, turned out to be unwarranted and, in fact, killed a lot of patients. Nobody familiar with the history of medicine can reasonably hold that the mere statement, "Clinical experience shows that . . ." is a fully adequate answer to a skeptic, and it is arrogant to conflate "Clinical experience shows . . ." with *"My* clinical impression is . . .", when the very fact that the skeptic is putting the question suffices to prove that *different practitioners' clinical impressions have not satisfactorily converged.*

In my own case, I am not aware of any ideological commitment, personal loyalty, or financial inducements that influenced my psychotherapeutic views over the 45 years I have practiced. I think that the sole reason that my approach in trying to help people is today more "active" and cognitively-oriented and less psychoanalytic than it was in the 1950s is my strong impression that the psychoanalytic approach, while more fun for the therapist and sometimes for both parties, is less effective and far more costly and time-consuming than RET. I would prefer to have more hard data on this. But I take notice that the meta-analysis of Smith and Glass (1977) gives an edge to cognitive and behavioral therapy over psychodynamic therapy, which tells me that my clinical impression is at least not running against the best quantitative data available.

From my own practice, I take the following example: I have rarely been engaged in the treatment of psychotic schizophrenia, since I do not entirely approve of nonphysicians treating full-blown schizophrenics on an outpatient basis, although that is a highly debatable point. But I have spent several thousand hours working with decompensated but nonpsychotic schizotypes of the Hoch-Polatin variety. Reading May's (1968) book makes me doubt the efficacy of psychotherapy in florid schizophrenia, although the later meta-analysis by Smith, Glass, and Miller (1980) softens the blow somewhat. So, I am doubtful as to the propriety of taking patients' or taxpayers' money for psychotherapy with schizophrenics. As to the Hoch-Polatin syndrome, while I am not aware of affirmative evidence for the efficacy of psychotherapy with these patients, I do not know of any sizable body of *negative* data indicating that it does not "work." Since I have theoretical grounds for thinking that it should be possible for the psychotherapist, if he is suited for work with such patients, to help them stabilize, and since my clinical experience suggests connections between what I do and what happens, I do not consider it unscientific or unethical to work with patients of this type.

There isn't anything puzzling about this. It's simply the difference between two situations. In one, you oppose your subjective impressions, with all we know about how fallible they can be (getting a PhD does not cure us of bias or memory distortion or the familiar fallacies that are the origin of superstitions), to negative research data, even when the latter are appropriately gathered and analyzed in a sophisticated way. Whereas the other case is the *absence* of *affirmative* research data, where allowing oneself to make judgments on the basis of theory and clinical experience is acceptable.

One puzzling thing about psychology, when we contrast it with a field like medicine or engineering, is the frequently weak connection, or dubious translatability, of concepts at one level of analysis into another. We were exposed in beginning sociology to August Comte's famous "pyramid of the sciences," and the related vexed problem of *conceptual reduction.* Philosophers of science tell us that complete reduction of concepts to another level of analysis (as, for example, in a structural-compositional theory such as the DNA) is less common than used to be supposed. Nevertheless, there is a breath-taking beauty in the Crick and Watson discovery, and the same can be said for many other branches of the physical and biological sciences. We know what the liver is for, and how it works, and can formulate almost all of it in terms of microstructure and biochemistry.

However, the existence of impressive examples of conceptual reduction, where both the concepts and the laws have, if not a rigorous deducibility from one level to another, at least a strong quasi-derivation with suitable *ceteris paribus* clauses, does not mean that the applied scientist constantly translates statements at one level into those of another. When a heating engineer comes to my house to figure out what the matter is with the heating system in Minnesota weather, he talks in terms of BTUs and cubic feet of air moved and thickness of insulation and the like. He does not formulate his diagnosis or prescribe his treatment in terms of the kinetic theory of heat (writing equations for the mean kinetic energy of molecules, probability distribution of their velocities, etc.). If he has been out of engineering school for many years, he would have a tough time reconstructing it that way. Nevertheless, the concepts and laws of heat engineering are based pretty thoroughly in that kinetic theory of heat, just as what the physician does in working with your liver disease rests on the basic sciences of histology and biochemistry. For some reason not clear to me, a comparable derivability from the basic sciences to the concepts and laws we psychologists work with in clinical practice rarely obtains. This is so obvious and ubiquitous that we usually don't bother to mention it, we take it for granted. One rarely thinks about the relationship between, say, the internal consistency measures of reliability in a structured verbal personality inventory like the MMPI and the basic science of psycholinguistics. Translations between levels, even when fairly successful and persuasive, do not often lead to a reverse derivation back to the original molar level, in which our purported insight into the machinery, based upon this theoretical reduction, provides really new ideas.

Example: For some years I used Dollard and Miller's 1950 book on personality and psychotherapy as an ancillary text, and I do not feel apologetic about that since it was a good job of its kind. They attempted to formulate psychoanalytic therapy and the theory of neurosis in terms of learning theory. It can't quite be done, and I daresay Freud would not have been much impressed with it, but it was a heroic effort by two very able minds. I would, even today, look upon certain aspects of it as essentially correct. We speak psychoanalytically of proper dosage of anxiety, and connect that with the idea that interpretations ought neither to be too deep nor too superficial (not quite as empty a tautology as it sounds). We can formulate the interpretive process as extinction of two kinds of conditioning. The therapist's words, calling attention (as Fenichel says) "to what is available as a preconscious derivative and *just a little bit more*," has two concurrent effects that would appear opposed to each other unless we realize that they involve these two different conditionings. The interpretation of the defense, sometimes combined with an interpretation of the impulse warded off, tends to bring about experimental extinction of the defense as a form of instrumental conditioning, because the acquisition and maintenance of the defensive operant was based upon avoidance learning. If one cannot "successfully avoid" (on hearing the therapist's wording), this constitutes an extinction trial. However, the patient's impulse expression (including verbal, postural, gestural, and introspective occurrences) is not punished in the manner of childhood's significant figures, so that the underlying classical conditioning of anxiety is also undergoing some extinction. If interpretations are premature or clumsy or an adequate positive transference has not been established (I won't translate that into learning theory terms, but it shouldn't be difficult), the anxiety dosage is excessive and the attrition of the classical conditioning will not occur. On the other side, if the therapist is timid, the operant extinction side of this two-sided process fails. We speak then of "playing along with the resistance." Fenichel says the analyst must be like the surgeon. You cannot perform surgery if you are afraid to shed blood.

While this is an illuminating way to look at the interpretive process and to understand semiquantitatively why proper dosage of anxiety is needed, I am not sure that this reduction, even if accepted as a complete account (which it probably is not), tells me much as a psychotherapist that I didn't know already when the process was formulated in psychodynamic language rather than learning language. My recollection is that almost the only place that Dollard and Miller's formulation generates something new by way of explaining the process is their semiquantitative derivation of the negative therapeutic reaction. I doubt that Freud would accept that explanation of it, although perhaps he ought to! So when it comes to suggesting new techniques, or criticizing standard psychoanalytic tactics as counterproductive, I don't believe you will find that anywhere in the book.

Consider rational emotive therapy. While Ellis, in his theoretical writings, refers to the classic studies of conditioning, perception, and the like, can it be said that the strategy and tactics of RET flow from the general psychology of cognition, perception, motivation, and learning? I think not. In his recent book, *Overcoming Resistance* (1985), which abounds with helpful tactical suggestions within the RET framework, I don't believe more than a half a dozen principles from a psychology of learning course are invoked. It makes you wonder what the appropriate pre-PhD training is for somebody who is going to practice either psychoanalytic or rational emotive therapy. It is at least arguable that a broad humanistic exposure to such thinkers as Epictetus and Buddha, or a reading of Bertrand Russell's *Conquest of Happiness,* is more relevant preparation for the practice of RET than a course in animal learning. It goes against my "basic sciences" grain to suggest that, and I suppose the reasonable suggestion would be to put in both and cut out some of the thin beer and baloney courses that students take, which shall be nameless.

Having voiced skepticism about the theory/practice relation, let me present some counter-instances, where one's theory matters, sometimes quite concretely and directly ("tactics"), sometimes in a sort of background guiding way ("strategy," "mind-set," "context of discovery").

Example: Operating within a fairly classical psychoanalytic mode, the session having begun with a dream, the associations suggest and (short-term clinical prediction!) corroborate the conjecture of a certain theme. But initially one cannot quite formulate this regnant theme as a *wish.* Choice of technical maneuvers toward the session's end will not flow deductively from dream theory, rather being "chosen" on the basis of personal style, intuition, one's own analyst as model, experience with this particular patient—the whole complex of conscious and unconscious factors that converge to produce interventions that Theodor Reik would call "tact" (versus clumsiness). Nevertheless, the selection of short-term tactics is, however weakly, constrained by the overall implicit strategy of seeking the unconscious wish, and may be quite different if the analyst accepts merely that "dreams tell us what the unconscious is cooking" (as my first analyst once said) rather than Freud's strong thesis of wish-fulfillment.

Example: I consider hedonic capacity a normal-range individual differences variable, probably polygenic, and have tried to pull together some clinical, psychometric, and experimental data in support of that theory (Meehl, 1974, 1987). If some patients with pleasure impairment get that way not because of impedance (anxiety, shame, rage) but due to a primary hedonic deficit, interview strategy relying on the classical impulse/defense model is inappropriate. How one discriminates clinically between secondary and primary hypohedonia is a complicated question to which I have no good answer, although I have offered a few suggestions. Some day we will do this with psychometrics and psychophysiology.

Example: Accepting the experimental psychologist's distinction between classical and instrumental conditioning (Skinner's Type S, "respondent" and Type R, "operant," like Thorndike's Associative Shifting versus Law of Effect) has a fairly direct impact on one's therapeutic strategy with respect to somatization. Striped muscle or sensory symptoms (the old conversion hysteria) I consider operant, maintained by Type R reinforcement, hence always having a social or intrapsychic *meaning* and *purpose.* Per contra, therapeutic exploration of a psychosomatic complaint with its putative aim in mind I consider useless and even irritating, since a stomach ulcer or psychosomatic asthma is maintained by Type S conditioning, or as a mere "physiological outflow" from an affective state-variable in Skinner's sense. "What is the patient expressing, or warding off, or gaining, by means of this ulcer?" is a bad question, the short answer being, "Nothing." Seeking its psychic meaning or goal is both theoretically unsound and a form of injustice, imputing nonexistent hidden motivations. Chronic, intractable somatic *concern* (with minimal somatic symptom-presentation) is again a different matter, and I am mindful of Bleuler's flat statement that almost all long-term textbook hypochondriacs are schizophrenic.

Example: A client complains of being "introverted," focusing on her distaste for office parties. Suppose one practices a mixed psychodynamic/cognitive therapy. Within this general frame, one's tactical approach to garden-variety social introversion (not schizotypy and not social phobia) will depend partly on theory. Since I accept the findings of Eysenck, Cattell, Gottesman, and my Minnesota colleagues Bouchard and Co. that the core of social introversion is a polygenic heritable trait (indirectly supported by analogous animal data), I do not ask, "What pathogenic defense gets in the way here?" but instead, "How do we desensitize the self-concept label 'introvert'?" and "How best cope with these irksome office parties without damaging her career?" So the theoretical position is here strongly influential, doubtless abetted by my not liking office parties either.

Example: A patient presents with symptoms of depression, anxiety, and social withdrawal. He has some vegetative signs of endogenous depression but not others; for example, he has terminal insomnia, "Meehl's eye-sign" (upper lid covers iris and sector of pupil, lower lid sags to reveal considerable sclera—I *think* I discovered that one) but no weight loss. Psychometrics are inconclusive. No plausible precipitating events are discernible. The family history reveals a sibling hospitalized for a classic manic episode, and an uncle who suicided in a severe depression. Best bet diagnosis: Depression, endogenous, in a genetic bipolar. The treatments of choice, based on the available quantitative outcome studies, are Beck's cognitive therapy, or tricyclics, or both. The deciding diagnostic consideration is the genetic theory of bipolar affective illness. An important management corollary is careful attention to the suicide risk (extraction of the usual "promise to call," perhaps inpatient care) since the quantitative research shows lifetime suicide risk of both unipolars and bipolars to be 1:6 (Russian roulette odds, not to fool around with); whereas "neurotic" or reactive depressions or depressions secondary to other psychiatric conditions have suicide risks only slightly above the general population.

These theory-to-practice examples involve substantive theory about personality, neurosis, genetics, and the several processes involved in healing by verbal means. What you think about them depends on what theories *you* hold, perhaps differing from mine, and I chose them with that intent. Since we cannot today say confidently who is right, our clinical practices may legitimately differ, depending on which theoretical horse we prefer to bet on. "Let 100 flowers bloom." Until the evidence is all in, we can agree to disagree, meanwhile hoping that our wrong bets will not harm the patient too much.

But we must not deceive ourselves. There is another class of "theoretical" matters where this delightful tolerance of different viewpoints is inappropriate and, in the extreme case, downright unethical. I have in mind those "theoretical" constraints that are *methodological* rather than *substantive,* that cut across theories and even disciplines, noncontroversial principles of logic, evidence, elementary probability theory, sometimes just plain arithmetic. I cannot offer hard quantitative data, but I have a strong anecdotal impression that the intellectual dissociation between what students learn in the academy and what they do in the clinic is here more pronounced than with respect to competing theories of the mind. It is also, in my opinion, more dangerous.

Example: At a Klopfer workshop 40 years ago, my colleague Grant Dahlstrom was troubled about the numerical instability of M: ΣC given the modest interscorer reliability of the determinants composing each side of the ratio. Upping M by one from, say, 2 to 3, and lowering Σ by similarly small amount wreaks havoc with the resulting ratio between two such small numbers. How then can the *Erlebnistypus* thus computed be other than extremely unreliable? The instructor smiled blandly and said, "Mr. Dahlstrom, a human being is more than a set of numbers." Such a reply to a simple scoring reliability question is, of course, absurd. It doesn't matter what theory of the mind, or of inkblot projection, I hold—Dahlstrom's problem stems from sixth grade arithmetic, and the reply he got was muddleheaded obscurantism. Other common examples involve ignoring Bayes' Theorem, which does not depend on any psychological theory but is a set-theoretical truth of high school algebra. Or the over-interpretation of WAIS patterns by neglecting the influence of subtest reliability and low subtest pairwise correlations. These things are not based on legitimate dissents from somebody else's psychological theory: they are just plain *mistakes,* bloopers in basic quantitative reasoning. I think they persist partly because of the poor way statistics is often taught, but also because the role models of preclinical professor and clinic supervisor are usually different persons. The professor teaching statistics is never a clinician, and uses no clinical problems to illustrate basic statistical reasoning. The clinical supervisor, alas, makes these mistakes himself! So it's easy for the fledgling clinician to think, in effect, "I had to learn all that boring stuff to pass prelims, but just as I thought all along, it has nothing to do with what we do here, out in the real world, caring for patients."

My undergraduate adviser at Minnesota was Donald G. Paterson, one of the great men of applied psychology, a founder of what was called "student personnel work" in the 1920s and grew into counseling psychology. Pat used to complain that most psychologists didn't consistently *think* like psychologists outside the lab, clinic, or library. For instance, clinical or experimental psychologists often fail to reason about everyday questions of student selection and training, faculty incentives, administrative communication, and the like, in ways an industrial psychologist would take for granted in practicing his trade. I believe Paterson was right about this, and one place I see it is in the reaction of most academic psychologists to the idea of professional schools, especially to the PsyD degree.

Example: Reiteration of the research-production aspect of PhD training for practitioners continues, despite the fact that widespread allegiance to the "Boulder Model" for over a generation now has not raised the modal publication rate of clinical PhDs above zero. If something doesn't "work" after that long a trial, a scientific psychologist should be empirical enough to draw a conclusion.

Example: If almost all practitioners will be nonresearchers, but (one hopes) critical research consumers, why the emphasis on a research hurdle, as even most PsyD curricula require? Learning theory would suggest the best way to teach people how to do a certain thing is to have them practice that thing, not something else. The notion that spending 1000 hours painfully and unwillingly producing a doctoral thesis on some highly restricted topic will generalize to the subsequent research-consuming skills *and values* of a busy practitioner seems to me implausible on the basis of general psychology, not to say common sense and the anecdotal evidence. If someone I love goes for help to a practitioner, I would much prefer that the practitioner had spent that 1000 hours in another 900 of good clinical supervision and 100 of critical research scrutiny of the kind all Minnesota PhDs received in Paterson's famous Individual Differences class. Scrutinizing and dismantling research studies piece by piece and bone by bone over two quarters on a variety of topics, we acquired some pervasive, overlearned, and *highly valued* critical skills in reading the literature, such that whether or not he was creative, original, or research-productive at all, there were certain methodological bloopers that no Minnesota PhD would ever commit or allow to pass undetected. That makes more sense to me than the traditional dissertation or anything close to it. If we require a "big paper" at all, I would advocate a detailed case study (cf. Meehl, 1971).

I may say in passing that the empirically unrealistic *goal* of persuading most practitioners to publish research is itself not good psychology, since historians of science and statistical studies of the scientific communication network (e.g., *Science Citation Index*) show that by far the largest portion of published studies have negligible merit or impact. Thus, Myers' study (1970) suggests that, if the less visible 90 percent of publishing psychologists quit writing articles, the scholarly enterprise would be none the worse for it. But I find most academics uninformed as to these important sociological findings, and highly resistive to learning anything about them.

I can illustrate both the problem of integrating different levels of description or theory, and the task of putting together clinical impressions with scientific research from my personal experience as a practitioner interested in the theory of schizophrenia. In 1962, I published the sketch of a theory of schizophrenia and am currently writing a paper updating it (see also Meehl, 1972a, 1972b; Gottesman and Shields, 1982). Naturally, I am fond of my own theory, but its long-term merit is not the point here, but, rather, "how one's thinking works" on a tough problem like this one. As to levels, I think it a mistake to try to derive the soft neurology (e.g., ± dysdiadochokinesia) from psychisms, as some psychodynamic clinicians try to do with respect to the exaggerated tendon reflexes described already by Bleuler and Kraepelin. On the other hand, I think Bleuler, both in his 1911 classic and in the less widely known *Theory of Schizophrenic Negativism* (1910/1911), demolished "organicist" efforts to explain the motor phenomena of catatonia. The purposive, molar character of catatonic negativism, especially when we find it alternating in the same patient with command automatism, echopraxia, and echolalia, can only be understood psychologically and not in terms of some simplistic neurology of extrapyramidal tracts, reciprocal innervation, ballistic movements, and the like.

So here we have a situation where you can make two kinds of mistakes about levels. To integrate these levels of explanation is a problem of extraordinary difficulty, and some would consider it foolhardy to attempt. When I think about the neurology and psychology of schizophrenia, given the evidence of its being a genetic disease, I find myself moving from psychodynamics to speculative neurophysiology and back again. For example, the delusion of the end of the world, often found in the early stages of schizophrenic decompensation, can be interpreted as the symbolic and intellectualized expression of the patient's realization that he is undergoing a withdrawal of cathexis from the internal representations of social objects. The anhedonia, Rado's pain dependent pleasure, and Bleuler's cardinal trait of ambivalence, I subsume under the general heading of ambivalence combined with aversive drift, which is kind of an intermediate level psychism. But then, if I try to derive the anhedonia from the primary associative loosening, which Bleuler considers the root process of the disease, I can't do that psychologically. I have to move down to some speculative neurophysiology involving the positive and negative feedback from Olds (+) and Olds (–) reinforcement centers in the limbic system. This takes some fancy footwork, and there is always the problem of when to operate at the same level and when to shift levels or attempt a conjectural reduction. Furthermore, we must allow for the likelihood of a genuine mixture of causal dependencies, of the kind Bleuler suggests with regard to *some* of the motor phenomena of catatonia, where a possible neurological substrate, analogous to Freud's "somatic compliance" in hysteria, *goes along with* the higher order psychisms involved in the schizophrenic's several mechanisms for disengaging with the environment. The paradox of negativism with command automatism is resolved by viewing both as ways to minimize genuine interpersonal engagement, and to avoid inner conflict over what kind of commerce to have with the social surround. So here we explain by invoking psychisms. Yet perhaps those schizophrenics who develop the more dramatic aspects of catatonia, such as the waxy flexibility and analgesia, require to have some deviant neurological parameters along with the psychisms of autistic withdrawal. This mixing of explanatory levels strikes some psychologists as too complicated, which puzzles me because all you have to do is look at the causal arrow diagram of decompensated kidney function in organic medicine and ask yourself whether you really believe that the brain is simpler in its causal connections, or less "hierarchical," or less "feedbacky" than the kidney. Frankly, I think I do this sort of thing somewhat better than most persons theorizing about schizophrenia, because I am not hung up on the conflict between being a biotrope or a sociotrope, or some form of dualism in which one doesn't *really* believe that the mind is the brain in action.

The severest critics of such speculative theorizing are the arch behaviorists, but they don't bother me much. Despite the undoubted potency of Skinner's technology, which no informed person disputes, that doesn't tell me much about the overall theoretical adequacy of his formulations. Skinner consistently rejects both concepts and explanations of an institutional and social sort. He is impatient with those who try to understand the world (or change it) in terms of Keynesian economics or political theory, or even the individual social psychology of traits and attributions. He complains that these are all inappropriate levels of analysis. However, if a psychologist wants to move downward in the pyramid of the sciences, Skinner also complains. It is sinful to reduce or explain behavior in terms of cell assemblies or genes, or inner mental events, but it is also apparently wicked *not* to reduce jurisprudence or economics or political theory to the concepts of operant behaviorism. My ex-Skinnerian wife's pithy comment on this was, "Skinner is a reductionist or an anti-reductionist, depending upon whether you are moving down *to* or down *from* his preferred level."

In my thinking about schizophrenia, where I not only bounce around between explanatory levels but also evidentially between my clinical experience with the Hoch-Polatin syndrome, to the behavior genetics data, to the recent burgeoning of studies of schizoid soft neurology, it is discouraging to reflect that, in order to do it properly, there has to be too much expertise in too many areas in one head.

Some years ago, I circulated a memorandum among my Psychology Department brethren concerning the ephemerality of things that go on in the "soft areas" of psychology. One of my experimental colleagues, Professor Viemeister, who studies how the ear works (he belongs to the Acoustical Society but not APA) gave me some flak about how I could maintain my academic morale if I thought ideas in fields like clinical and social psychology were that ephemeral. By way of reply, I wrote him a memo pointing out that there are five "noble intellectual traditions" in clinical psychology which I am prepared to defend as *not* being faddy and ephemeral. They have been around for a half century or more, and while some of them are going better than others, they are all here to stay. Here is my list:

1. **Psychometrics:** It was our having a test of general intelligence that could be administered in an hour or so that brought the early clinical psychologists "into the clinic" in the first place. While there was some overtesting by standard batteries (for example, in the early days of the VA training program) that included instruments of negligible incremental validity, we can say that the WAIS, WISC, MMPI, CPI, SVIB, tests for brain damage, memory, and special disabilities are surely here to stay. Which components of the individual personality it is clinically useful to assess is a difficult question, upon which scholarly practitioners can disagree. Insufficient attention is paid to some distinctions I offered in the *Canadian Journal of Psychology* a quarter century ago (Meehl,

1959), involving successively higher hurdles for the justification of using a psychometric instrument that costs professional time and the taxpayer or the patient money. We first must distinguish between negligible and respectable validity coefficients and then between respectable validity and *incremental* validity; that is, learning something from the test over and above what you would routinely learn anyway. Finally, we demand *pragmatically important* incremental validity. The test must reveal something that really contributes to decision making about diagnosis, prognosis, and treatment. I hope we are now past the stage where we think the purpose of our test is to predict the verbal behavior of the psychiatrist, a rather pointless exercise, since the way to find out what he is going to write in the chart about a patient is to ask him! Construct validity is here to stay, although I have the impression that some are still not quite clear about it from either the philosophical or the statistical point of view.

2. **Applied Learning Theory** (operant contingency management, desensitization, and aversion therapy): It is interesting to note that this is the only one of the five great traditions where the primary origin of the concepts and methods is the experimental laboratory. Here we find mainly research on infrahuman animals being successfully applied in the clinic. While Skinner and Wolpe are the current big names, of course the tradition is older than that (e.g., one thinks of Knight Dunlap's beta method, Watson and Raynor's Albert, Guthrie's cue-alienation approach, or some early work going back to the 1920s on the treatment of phobias). I think whatever one's general theory of the mind may be, the technological power of learning theory applied to certain types of clinical problems can hardly be in doubt. That holds for clinicians like myself who prefer working with other methods but who nevertheless make referrals to (or sometimes work jointly with) behavior modifiers.

3. **Behavior Genetics:** This is the most exciting area in contemporary psychopathology, mainly developments of the last 30 years in the theory of the major psychoses, but also normal range individual differences, as in the twin studies of my colleagues Bouchard, Lykken, and Tellegen. Despite the invention of powerful mathematical techniques and the tremendous impetus to theorizing provided by molecular biology, the tradition is an old one, going back to such giants as Galton, Terman, Tredgold, and even Pavlov with his distinction between inhibitory and excitatory temperament in dogs as manifested in experimental neurosis. Freud must be included in this tradition, despite the anti-hereditarian bias of American psychodynamic clinicians. Anyone who thinks Freud didn't believe in the importance of genes in determining who falls ill of a neurosis has not read him carefully.

4. **Descriptive clinical psychiatry** (and the relevant aspects of clinical neurology): My experimental colleagues recognize the fact that I, as a clinician, know quite a few first-order descriptive facts about mental illness that come from neither the laboratory nor statistical analysis of psychometric data. There was a period

in American clinical psychology when descriptive psychopathology of a nosological kind was denigrated, for reasons that did not hold water either empirically or philosophically. Now, I see some danger, despite the impressive results in behavior genetics, and less impressive results in taxometrics, that quarrels with our medical brethren about some of the strange creatures catalogued in DSM-III may have resulted in a certain anti-nosological backlash among nonphysicians. I hope this does not continue, since trade union hassles are not a good scientific reason for defending or opposing constructs. Despite the emphasis on objectification and statistical data combination in my writings, I firmly believe (with my mentors Hathaway, Schiele, McKinley, and my analyst and analytic supervisor Glueck) that there is no substitute for extensive and intensive clinical experience with patients in learning how to look, listen, reflect, and inquire. (Even DSM-III itself and some of its associated instruments, I would criticize *not* on the grounds usual with psychologists but, rather, on the grounds that reliability considerations have led to the elimination of some important clinical signs.) There is no reason to apologize for descriptive psychiatry, although it is often more art than science in the strict sense. Besides, I assume we all believe in the value of "descriptive taxonomic sciences" (e.g., freshman geology, botany, or comparative anatomy). So we can count clinical syndromes as legitimate scientific concepts, if they are reasonably reliable as applied by skilled persons.

5. **Psychodynamics:** I persist in my belief that Freud discovered some important things about the human mind. A problem in giving historical credit for this great tradition is that certain portions of Freud's ideas have become presuppositions of most educated persons, so that Freud loses out because some of his ideas are taken for granted by persons who would not label themselves "Freudian" or even "neoFreudian." For example, some of my nonclinical colleagues who are minimally identified with the classical tradition freely invoke the defense mechanisms in talking about students and colleagues: "He tends to project a lot" or "I think that she has a reaction formation against her power drives" and the like. Whatever may be the fate of classical or modified psychoanalytic technique as a mode of intervention, I predict that many of Freud's basic ideas about how the mind works will still be around in the thinking of psychologists a century from now.

I think that these five noble traditions contain permanent elements of truth, although they differ in how firmly they are currently evidenced by what we consider hard data of an experimental or statistical sort. Taken together—and we have not worked as hard at integrating them as we should have—they constitute a body of genuine knowledge in clinical psychology, methods and concepts that are interesting and intellectually respectable, and for which we need not apologize to psychologists engaged in laboratory work at the basic science level. Putting it another way, as an educated, supervised, and fairly seasoned clinician, I am convinced that I know quite a few things about the human mind that an intelligent, thoughtful layman of matched I.Q. simply does not know.

I am aware that I have wandered around a good deal in this talk, and merely touched on those aspects of the problem of integration in our field that stand out in my mind as most important. I think we should accept the fact that the problem of hierarchical reduction of concepts in psychology is probably always going to be more difficult than it is for the physiologist, biochemist or engineer, and learn to live with that. One wishes that experimental psychologists or academic personologists not engaged in clinical practice would be more sympathetic to the decision situation presented by the pragmatic context. On the other side, clinicians should remember that, while we can say, “I collect my data in the clinic file, and form my theoretical impressions in the therapy session, rather than in the laboratory,” and while not everything that’s important to notice in this world can be subjected to meaningful quantification at a given point in time, and while one is primarily committed to helping this individual rather than formulating a theory of the mind—all of which things I believe as firmly as a full-time practitioner without academic connections or research interests—none of these truths can free the clinician from recognizing the distinction between knowledge that brings its credentials with it and purported knowledge that does not. No fair-minded person who was familiar with the history of medicine before it rooted itself in the basic sciences and developed a quantitative research tradition could fail to see that being a bright, perceptive person with helping impulses and having seen a lot of sick people is no guarantee whatsoever that you will not do all sorts of useless things—which is what most of medicine was before, say, 1850—and in fact will do all sorts of positively harmful things, such as did venesection. I have always been ambivalent about the Boulder model and I still am, partly because the relationship between the basic sciences and clinical practice is so much more tenuous for us than it is for a physician treating a biochemical malfunction due to liver disease. But if de-emphasis of the Boulder model comes to mean that clinicians no longer recognize the distinction between knowledge that brings its credentials and purported knowledge that does not, or that they forget the fallibility of human judgment and memory that is present in all of us, that would leave me doubtful as to whether psychologists have any credentials better than those of palmists and faith healers.

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