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Psychology: Does Our Heterogeneous Subject Matter Have Any Unity?

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My title's question, 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, and at times has taken on crisis proportions where the question seemed to be "Are we about to fall apart?" In the mid-1960s I chaired an ad hoc committee of APA on a controversy precipitated by an embarrassing legal wrangle in New York, where the distinguished experimental psychologist Gibson was prosecuted for giving a lecture without having bothered to become a licensed psychologist, and I was a vociferous but ineffectual member of the Clark "blue ribbon committee" (Committee on the Relation Between Scientific and Professional Aims of Psychology, 1967)—my ad hoc committee's successor. But I do not propose today to discuss licensure, politics, 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. It goes without saying that these theoretical issues are related to such mundane matters as money, and the relationship goes both ways, 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, an open secret such that the distinguished chair of that task force, Dr. Robert Spitzer, has been known to give 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 which we are not competent 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 the Psychology Department. I lunch frequently with my brethren in the field of experimental psychology. Except for the fact that, like me, they know a little 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 "bootstrapping" in taxometrics. It is possible to find two competent and highly productive 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 behavior geneticist studying the transmission of schizophrenia be able to converse with an expert on the electrochemical processes in the retina of the walleyed pike?

The Minnesota Psychological Association is celebrating its half century mark, but I have passed my two-thirds century mark, which, if I understand the mores of our culture, permits me a certain amount of Old Oaken Bucket delusion whether or not I join the Grey Panthers. I promised Sue Rydell that I would try to do something here other than lament the good old days or talk about the giants in the earth. But I'm afraid that I believe, at age 66, 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 to students about this, or even to some of our junior faculty, I get the impression that our attitude in this matter 45 years ago must strike them as terribly naive on the part of reasonably bright people. But, friends, just think of the great books that appeared in the decade 1935-1945, during which period 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,* 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, but 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 were 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 like chemistry or physics, these exciting developments did make 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.

Despite having had a lot of fun doing psychology (both with rats and patients) and having been given more ego rewards by the profession than I deserve, in a certain sense I have to say that at age 66 I am a somewhat disappointed man. For instance, I took it for granted when I was a graduate student that by the time I reached my present age I would have a pretty good idea just how much of the corpus of Freudian theory was true and how much was not. While I have learned some things since then, considering myself today to be roughly a 40% Freudian whereas in the 1950s I said I was a 60% Freudian, it is quite obvious now that I will go to my grave without knowing even approximately what that percentage should correctly be.

Whether connected with these great books or not, it is a social fact that in discussions in the old psychology building among the teaching assistants that sometimes went on till after midnight with White Castle hamburgers and Coca-Cola, there was not the bimodality that we observe today in clinical students between those who were practitioner oriented and those who experienced strong cognitive passions. All of us were both. I have often been asked by the present generation of clinical students to explain this change, and I am at a loss to do so. I do not even know whether it's a bad thing, except that I am conscious, as a professor who is also a practitioner, that clinical students with zero theoretical passions are, even when basically bright, slightly boring to me.

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 epoch of grand theories (Hull, Tolman, Guthrie and Co.) was the psychologist's obsession to be more like physics, which led him to take Newton or Kepler 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,* of which classical mechanics or thermodynamics would be examples, and whose paradigm is systems of differential equations telling us how certain variables change over time in relation to others. Then we have what may be called *structural* or *compositional theories,* which tell us what something is made out of, what kinds of substances or parts it is composed of, 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 kinds of substances out of which other more complicated substances are made, and the theory of DNA. Thirdly, 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. It was wrong of us to focus only upon functional dynamic theories as if the other two types were somehow less interesting or respectable, and 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 on the development of cognition are historical. Each of these kinds of theories has its own criteria of evaluation and, in a sense, even 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 the connecting of 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, there was at least one common factor present in all of the psychology faculty—scholars with such very different interests and 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 which 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 a century ago. One mark of a good psychologist is to be critical in evaluating evidence. In this respect she would have a mentality somewhat like an efficiency expert, a prosecuting attorney or detective. I have heard of some psychological testimony in courtrooms locally in which this critical mentality appears 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 if I employ a diagnostic procedure which has been repeatedly shown to have negligible validity to make life and death decisions about people and collect the patient's or the taxpayer's dollar for doing so. One of the deepest, most pervasive dimensions that separate psychologists in these matters is the famous Russell-Whitehead distinction between the simple-minded and the muddle-headed. 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 simple-minded psychologists have a hard time discovering anything interesting, whereas muddle-headed ones discover all sorts of interesting things that are not so. The simple-minded have a tendency to be hyper-operational, too closely tied to rigid standards of evidence (often based upon misconceptions of both philosophy of science 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 muddle-headed is less in their preference for certain classes of explanatory concepts than it is in their often 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 thing that makes me have a slight preference for the simple-minded. 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. Simple-mindedness as a methodological orientation is, in some cases, curable. But I have never known a muddle-head to get well. Muddle-headedness 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; and while the simple-minded 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 muddle-head, because muddle-headedness itself immunizes its victims from critical objections. You can't make him bothered by the fact he thinks sloppily, because part of muddle-headedness 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 Ph.D. 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 she is objecting to, and she should be clear about that lest she sound like some sort of obsessional perfectionist purist. What is objected to by some of us in examining some current clinical practices is the 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, "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 some quantitative form will warrant an alteration in one's belief system and hence one's clinical practice. One's accumulated clinical experience, including conversations about clinical questions 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 modern laboratory medicine and 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 due to 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 at least I can 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 experience at least is not lining up *against* the best quantitative data presently available.

From my own practice I take the following example: I have rarely been engaged in the treatment of florid schizophrenia, since I do not entirely approve of non-physicians treating full blown schizophrenics on an outpatient basis, although that is a highly debatable point. But I have spent many thousands of hours working both supportively and intensively with semicompensated or decompensated but nonpsychotic schizotypes of the Hoch-Polatin variety. The quantitative outcome research on full blown schizophrenia is sizable and consistent, and in fact even May's book (1968) taken by itself, given the professional quality and expertise of the planners and supervisors in that project, makes it hard to deny that florid schizophrenia is not appreciably influenced by psychotherapy, at least in any respects that we know how to detect. I would consider it doubtfully sound practice for a psychologist to undertake to treat full blown schizophrenia if he takes money for so doing or if he leads the family to believe that this is going to influence the course of things. So that if I were to engage in such practice, I would have a conflict between my clinical impressions (if I thought I was helping these people) and what the scientific evidence showed. That would be a case of persisting in doing something despite adequate adverse evidence.

But as to the Hoch-Polatin syndrome, while I am not aware of affirmative evidence for the efficacy of psychotherapy with these patients, the point is that I do not know of any sizable body of *negative* data indicating that it does not "work." And since I have theoretical grounds for thinking that it should be easier for the psychotherapist if he is suited for work with this type of patient to help them stabilize on the sane side of the equilibrium point, and since my clinical experience (admittedly not scientifically quantified) leads me to believe I can often see the connections between what I do and what happens, I consider it not unscientific and not 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 Ph.D. does not cure us of bias or memory distortion or the familiar fallacies which are the origin of superstitions), against 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, in which allowing one to make judgments on the basis of theory and clinical experience is all right.

One puzzling thing about psychology when we contrast it with a field like medicine or engineering (and I know enough about those disciplines not to exaggerate their rigor, displaying the psychologist's scientific inferiority complex) 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 at one level of analysis (as, for example, in a highly successful 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 your house to figure out what's the matter 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.), and 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, or to anybody else I have listened to or read on this subject, 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.

For example, for some years I used Dollard and Miller's 1950 book on personality and psychotherapy as an ancillary text in a clinical psychology course, and looking back 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 Hullian 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 to a large extent correct. For example, 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 in terms of two kinds of conditioning (here I do some Skinnerian stretching of Hull) namely, that the therapist's words, putting into language as Fenichel says "what is available as a preconscious derivative when attention is called to it, and *just a little bit more*," has two simultaneous or concurrent effects which would appear to be opposed to each other unless we realize that they are directed at 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, and if one cannot "successfully avoid" given the therapist's wording, this constitutes an extinction trial from the standpoint of the experimental psychologist. However, inducing the patient's wording in response together with postural, gestural, and introspective occurrences is not punished in the manner of childhood's significant figures, so that the underlying classical or Pavlovian conditioning (Skinner's conditioning of Type S) also is 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 respondent conditioning will not take place. But 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 interpretative 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. I haven't looked at the book recently, but 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, and 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 in his theoretical writings Albert Ellis makes reference 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,* 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 is the appropriate pre-Ph.D. training 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 more reasonable suggestion would be to put in both and cut out some of the thin beer and baloney courses that people take, which shall be nameless.

I can illustrate both the problem of integrating different levels of description or theory, and the task of putting together clinical experience 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 monograph updating it for Millon's new journal [Meehl, 1990]. Naturally, I am fond of my own theory, but its long term merits is not the point here, rather "how my thinking works" on a tough problem like this one. As to levels, I think it a mistake to try to derive some of 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, before him, even by 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), completely demolished "organicist" efforts to explain the motor phenomena of catatonia. The purposive and 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 overwhelming evidence of its being a genetic disease (a clinician who doesn't accept that conclusion is not in the ball game), I find myself moving from psychodynamics to speculative neurophysiology and back again. For example, the delusion of the end of the world, found in the early stages of schizophrenic decompensation, can be interpreted (I think rightly) 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. Again, 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 amazing 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. I like my psychologist wife's observation (she is an ex-Skinnerian of sorts), when we were discussing over our martinis the fact that Skinner consistently rejects both concepts and explanations of an institutional and social sort, that he is impatient with people who try to understand the world (let alone change it) in terms of Keynesian economics or political theory, or even the individual social psychology of traits and attributions, complaining that these are all inappropriate levels of analysis; whereas, if somebody wants to move downward in the pyramid of the sciences as a psychologist, Skinner complains. It is sinful to reduce or explain behavior in terms of cell assemblies or genes, but it is also apparently wicked not to reduce Keynesian economics or Marxian political theory to the concepts of operant behaviorism. My wife's pithy comment on this was, "In other words, Skinner is a reductionist or an anti-reductionist, depending upon whether you are moving up or down from his preferred level."

In my current thinking about schizophrenia where I not only bounce around between explanatory levels but also evidentially between my experience of thousands of hours of psychotherapy with the Hoch-Polatin syndrome, to the behavior genetics data, to the recent burgeoning of studies of the 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. Avoiding false modesty, which is not among my vices, I think it fair to say that not many clinical psychologists combine as much firing line experience with academic research as I do, or have acquired as much competence in psychodynamics, learning theory, animal behavior, genetics, mathematical statistics, and philosophy of science as I have. But, alas, I feel scientifically guilty in my theorizing because of my relatively poor knowledge of the latest state of neurophysiology, and my abysmal ignorance of biochemistry. Some people think that this problem can be handled by interdisciplinary teams, but I have not been much impressed with the concrete examples of that. I believe that the ideal is to have all of that stuff in one head, and that's too much to ask, even of a fake Renaissance man like myself!

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:

I. 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. The noble tradition of psychometrics is associated with Minnesota psychology as much as any place in the world, and while there was some over testing by standard batteries (for example, in the early days of the V.A. 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* [Meehl, 1959] a quarter century ago, 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; then between respectable validity and *incremental* validity, that is, learning something from the test over and above what you would routinely learn anyway; and 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.

II. 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, and in fact 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. I distinguish this tradition from the kind of thing involved in the Dollard and Miller book. That is, I am not considering here translations of other theoretical and therapeutic modes into learning concepts, but rather the direct application of learning principles in the treatment process.

III. 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, Doll, Goddard, 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 must never have read him.

IV. Descriptive Clinical Psychiatry (and the relevant aspects of clinical neurology) are parts of a great tradition which I hope is still strong in Minnesota, although the neuropsychiatry minor in the Medical School that was formerly required of all doctoral candidates in clinical psychology has been liquidated, some think unwisely. I am pleased to report that my experimental colleagues willingly recognize the fact that a clinician, trained as I was, knows 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 not quite so impressive results in taxometrics (where I predict almost quantum leaps in methodology during the next decade), 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 most of 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 grounds that reliability considerations have led to the elimination of some important clinical signs. I would mention, for instance, Bleuler's *ambivalence* as one of the primary symptoms of schizophrenia, dropped in contemporary psychiatry because of the difficulties of assessing it reliably. But that opens up an area of controversy that I do not have time to go into here. There is no reason to apologize for descriptive psychiatry, although it is often more art than science in the strict sense. Anyhow, I assume we all believe in the value of "descriptive taxonomic sciences" that exist outside of the mind area (e.g., freshman geology, botany or comparative anatomy) and hence I count clinical syndromes as legitimate scientific concepts, if they are reasonably reliable as applied by skilled persons.

V. 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 non-clinical colleagues who are minimally identified with the classical tradition freely employ 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 am willing to go out on a limb and 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 I think that 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 familiar with the history of medicine before it rooted itself in the basic sciences, and before it 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 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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