

Credentialed Persons, Credentialed Knowledge

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The vast experimental literature on human error agrees with history of medicine, folklore, and superstition in discrediting knowledge claims based solely on anecdotal impressions. Since clinical experience consists of anecdotal impressions by practitioners, it is unavoidably a mixture of truths, half-truths, and falsehoods. The scientific method is the only known way to distinguish these, and it is both unscholarly and unethical for psychologists who deal with other persons' health, careers, money, freedom, and even life itself to pretend that clinical experience suffices and that quantitative research on diagnostic and therapeutic procedures is not needed. Disputes about philosophy of science (e.g., logical positivism) are irrelevant to this issue, which is simply one of distinguishing knowledge claims that bring reliable credentials and others that do not.

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In the year 1487, there appeared a tome with the scary title *Malleus Maleficarum*, written by two Dominican monks, Kraemer and Sprenger. It was a technical work dealing with the important problem of how to diagnose a witch. Detailed specifications of psychological, physiological, and social criteria were laid down. It was recognized that there were false accusations and sometimes even false confessions of witchcraft. Although the authors did not write in terms of mental illness, it is obvious that discriminating true witches from what we would call "hysterics" or "schizophrenics" was part of the diagnostic task. During the next two centuries, thousands of persons were

hanged, drowned, burned alive, or crushed to death as witches on the basis of these diagnostic criteria. Kraemer and Sprenger were not fools or evil men; they were learned in the best technical know-how of their time and they were sincerely striving to do justice. Psychologists today do not believe that some persons have entered into a formal compact with Satan, whereby they are given supernatural powers and are able to kill people by sticking pins into their effigies. How are we to explain that vast socially shared delusion about witchcraft? The answer is that those authors, and others like them, were operating within an accepted theory and had identified symptoms and signs on the basis of extensive reading and "clinical experience." Everybody, learned and ignorant, believed in witchcraft, although we now know there is no such thing.

One might suppose that such misconceptions could only arise in a prescientific age, surely not since the Enlightenment. Is that so? In the early 1940s, cases appeared of a new pediatric disease in which a mass of cartilaginous material grew in the child's eyeball and ultimately resulted in total incurable blindness. This new disease, retrolental fibroplasia, was confined to premature infants, and a debate arose among obstetricians as to the efficacy of routinely administering large amounts of oxygen to prematurely born babies. Some said the pathology arose from not giving enough oxygen; others said it was from giving too much. Each side appealed to clinical experience as the basis for their strong opinion. The controversy was settled by combining statistics and experimentation. It was found that premature infants delivered by midwives in the mother's home, where oxygen was not used, never got the disease, and experimental studies of kittens prematurely delivered showed that they developed

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the identical eyeball pathology when oxygen was administered.

Surgeons in the early 1600s debated passionately about wound debridement, that is, the cutting away of flesh in the wound that had been contaminated by gunpowder and pieces of metal. It is hard to believe that this dispute continued even through the first World War, with a few holdouts against debridement. It was not until controlled statistical studies were conducted during World War II—after three centuries of dispute—that the controversy was finally settled. Today we find it hard to imagine that anybody would have thought it desirable to bind up a wound without debriding it. Such examples, which abound in the history of medicine, suffice to refute the idea that large numbers of professionally educated persons could firmly hold incorrect beliefs only before the rise of modern medical science. Historians of medicine inform us that before around 1890 almost everything that physicians did in treatment was either useless or actively harmful. For example, standard procedures included bleeding, purging, and blistering, the first two being harmful, the third irksome, and all useless.

In the field of clinical psychology, there are persisting conflicts about diagnostic and therapeutic procedures. Sometimes the disputes have become quite heated, involving claims of scientific incompetence and unethical practice. Millions of dollars in civil lawsuits, child custody proceedings, and criminal trials sending people to prison frequently hinge upon diagnostic procedures and psychological theories on which there is no scientific agreement. What is the basis for the knowledge claims we make? Let me assure the reader that I am not a pessimist about clinical psychology, in the teaching and practice of which I have made a comfortable and interesting living for over half a century. I think reassuringly of five “noble” intellectual traditions, which I am prepared to defend as not being faddish or ephemeral: psychometrics, applied learning theory, behavior genetics (currently the most exciting area of psychopathology), descriptive clinical psychiatry, and psychodynamics. I note in passing that only one of these five, applied learning theory, has its origin in the experimental laboratory. In 1996, clinical psychology celebrated its centennial year since the founding of Lightner Witmer’s laboratory in Philadelphia, and Division 12 of APA gave Centennial Awards to Hans Eysenck and me for our contributions to the field. Eysenck

and I disagree about two of the “noble traditions” I listed (descriptive clinical psychiatry and psychodynamics), and we have a somewhat different emphasis regarding a third (psychometrics). This illustrates the pessimistic side of my optimistic coin, namely, that we are still in a primitive state of knowledge when two prizewinners, elected by a high-quality committee to celebrate a centennial, can show 50% disagreement on such basic questions. I, of course, do not know who is going to turn out to be right, nor does Eysenck, nor does anyone else. But one thing I do know, and that is that our disagreement will not be settled by appeals to clinical experience, but by the systematic application of what I unabashedly call the scientific method.

The anecdotal method has long been discredited in the study of animal behavior. We clinicians should not deceive ourselves by denying that the phrase “clinical experience” is our honorific term for subjective anecdotal impressions as reported by people with MDs or PhDs. Scientific psychology began with the systematic study of error, growing out of astronomer Bessel’s interest in the personal equation in recording star transit times. The critical incident was the royal astronomer Maskelyne’s firing of his assistant Kinnebrook, because Kinnebrook’s readings differed systematically from his. Psychology classes used to include a lecture or two on the unreliability of human testimony, frequently with a demonstration of how fallible are people’s abilities to observe, record, and retain even simple event sequences. It is regrettable that today’s general psychology texts and lectures almost never go into this, because a strong, pervasive awareness of it is part of the conceptual equipment of any properly trained psychologist, clinician or otherwise.

When I taught introductory clinical psychology the students were eager to listen to a practicing clinician who had dealt with real flesh-and-blood mentally ill people, and I did not wish to dampen this enthusiasm. I started the class by recognizing this interest, telling them that I was convinced that I knew some things about the mind that my brethren in experimental psychology did not know. I told them that wherever I had quantitative evidence—whether experimental, or statistical from clinic files, or even quantitative tallies of my own observations—I would report such. But often I relied on my diagnostic and therapeutic experience, clinical lore from my mentors, books by the great clinicians, my own

experience on the analytic couch, and Beck's or Klopfer's Rorschach workshops. I then went on to tell them about witchcraft, and other examples similar to those above. I said I hoped that I had been sufficiently self-critical not to jump immediately to conclusions on the basis of a case or two, or to swallow everything that my clinical supervisors told me; but that nevertheless, while I hoped that most of the nonscientifically proven generalizations I offered them were correct, they could be confident that some sizeable minority were incorrect. The trouble was that, absent quantitative research, *I had no way of knowing which was which.*

I then went ahead with a clear conscience to make statements based upon clinical experience lacking quantitative research. I told them about Meehl's depression eye-sign, that an important sign of major depression (as distinguished from neurotic, reactive, schizotypal, or chronic anhedonic depressions) was that the upper lid covers the upper part of the iris and a sizeable sector of the pupil, whereas the lower lid sags so that a large portion of the sclera is visible. I imitated schizophrenic speech, where there is often a discrepancy between the *latency* in responding and the *rate* of speech; whereas if a nonschizophrenic patient is depressed enough to have a long latency in responding to simple questions, the speech tends to be retarded as well. I talked about the psychopaths' freedom from constricted expression and the animal grace they often exhibit. I did my best to imitate the paranoid walk. I did not have good scientific evidence for any of these clinical impressions, although in the case of the psychopathic style, I could report that, on a bet with a charge nurse, I kept a record for a year of cases where I diagnosed the hard-core Pd (the MMPI 49' syndrome) at sight walking down the hall; in 13 cases so diagnosed, I missed only one. That kind of study is one that any clinician can do without a government grant, but we are somewhat lazy about doing it. Since almost no generalization about the validity of a diagnostic symptom, sign, or test score, or the efficacy of an intervention, claims 100% success, one must face the fact that such claims involve probabilities less than 1; hence they are inherently statistical. One doesn't say this because of a fondness for statistics. One must rationally recognize that there is only one known method of checking on a statistical claim, and that is to compute statistics! I also emphasized the ordinary language philosopher's distinction between "knowing *that*" and "knowing

how," pointing out that one reason we prefer to be treated by seasoned clinical practitioners is our hope that an experienced physician or psychologist will be somewhat better at knowing how to *do* something than a fresh-baked PhD or MD, although they might be equal in knowing *that* the research shows so-and-so to be the case.

Lore has it that Einstein, when asked by a journalist during a chaotic phase of controversy in quantum mechanics what was wrong with physics, replied, "Physics is too hard for the physicists." I confess to have thought that about some of the more interesting problems in psychopathology. But, I repeat, frequently we are simply lazy about tallying some simple observable events and attributes.

For example, when treating my first psychotherapy patient in 1942, I had been reading about Freud's urethral triad (see Freud, 1908/1959, p. 175; 1930/1961, p.90). I was intrigued when the patient reported a dream about a fire being put out by the fire department and all the rest of the session alluded in a variety of ways to the theme of ambition. I did not subscribe to the libido theory, but I was convinced that many of Freud's theoretical notions were unsound inferences from clinical correlations that *were* valid. I resolved to see whether the generalization suggested here was correct. In 50 years of practice, much of which was psychoanalytic, I never found an exception to the generalization that if a male patient (it does not tend to work with women) dreams of fire being put out by water (later I concluded that fire alone, or fire-linked things such as firemen's hats, fire engines, sirens, fire hoses, or fire hydrants would do), if I kept silent and did not interpret anything, the rest of the session would consist largely of material in the broad area of Murray's *n Recognition*. Had I ever heard an exception, I would surely have noticed it. My clinical laziness is shown by the fact that, rather than merely waiting to see the first exception, which never happened, I should have been tallying all of the positive instances so I could report a 4-fold table, but I cannot.

Surely we can sometimes learn about facts and their relations without conducting controlled experiments or computing statistics? Yes! I am not a scientific fanatic. I agree we know that the thunder occurs after the lightning, that a wine glass shatters when dropped on a tile floor, that if you regularly say cruel things to people, they will dislike you. But these common-sense, everyday observations about readily observable and closely connected physical events are not something it

needs a PhD to discern, warranting a professional fee for technical knowledge. Humankind has also “learned” a large number of *erroneous* relations about black cats, and witches, and petroleum dowers. We label these “superstitions”—the ones that *we* disbelieve. A clear message of history is that the anecdotal method can deliver wheat or chaff, and it does not enable us to tell which is which. In Martin Luther’s day everyone “knew” that the best way to teach a child arithmetic was to administer a painful whack on the knuckles following a mistake. I have elsewhere (in a discussion of psychoanalytic inference, Meehl, 1983) listed circumstances that warrant skepticism about nonquantified impressions from clinical experience. They include such things as large and variable time lag between allegedly correlated events, frequency of spontaneous change, a long list of variables with different weights and interactions, and inferred inner states and unobservable events such as past history.

When a clinician says, “I can tell for certain, as the patient walks into the room, that she has been sexually abused as a child” (a preposterous claim to make without very strong evidence), it is not like a layperson who says, “I notice that the thunder always comes after the lightning.” I cannot understand PhDs in psychology who ignore a body of research going back to Bessel’s discovery of the personal equation, studies of perception, memory, judgment, inference, and hypothesis testing, showing the great variety of sources of error in these processes—studies numbered not in the hundreds, but in the thousands, done by clinical, counseling, social, and cognitive psychologists. If I insist upon the validity of Meehl’s depression “eye-sign” and reject the need to research it, I am claiming genius immunity from the failings to which all human beings, with or without advanced degrees, are *known* to be subject.

One dishonest tactic is to say, “I am in a clinic, not a laboratory, so scientific rules just don’t apply.” Knowledge credentials are not tied to a building called a laboratory, containing white rats and Skinner boxes, tended by academics in white coats. Ignoring a skeptic’s request for evidence by invoking the buzzword “clinical” amounts to saying that the patients’ cognitive distortions can be studied, and those of their relatives, but that I, the clinician, am immune from study. This is may be convenient for me, but it is irrational and irresponsible.

Consider a concrete, scary, clinical example: Major-depressive disorder has the highest suicide risk of any psychopathology. I was taught that the

lifetime rate for these patients is around .15, which is nothing to fool around with, being Russian roulette odds. Recent data make it at least .20, perhaps .25; whereas the suicide rate for neurotic, reactive, chronic anhedonic and other symptomatic depressions is only slightly higher than the general population figure. The MMPI and the Rorschach, however valid they may be for inferring a depressive mood, do not accurately distinguish among these diagnoses. Suppose that we are discussing whether it is safe for a patient to be treated on an outpatient basis. I invoke Meehl’s purported major-depression eye sign in support of my diagnosis. You tell me that your clinical experience does not confirm mine. What is our scientific and moral situation here? We are, absent research evidence, in the same position as two little boys, one of whom says, “My dad can beat your dad,” and the other says, “Cannot,” “Can so,” “Cannot”—a standoff; except here the stakes are higher, involving deprivation of human liberty and a frightening probability of death. If I insist that my anecdotal impression must prevail, I am not being merely arrogant and unscholarly, I am being immoral. Disputes about positivism or operationism, behaviorism or psychodynamics, projectives versus structured tests, or even clinical and statistical prediction, are all red herrings. What is involved here scientifically and morally has nothing to do with these divisions. It is simply the distinction between a knowledge claim that brings good credentials and one that does not. “I feel very sure” is a fact about Meehl’s biography, it is not a knowledge credential. Long before the era of logical positivism, behaviorism, or biometrics, the essential point was forcibly made by Francis Bacon:

The human understanding, once it has adopted opinions, either because they were already accepted and believed, or because it likes them, draws everything else to support and agree with them. And though it may meet a greater number and weight of contrary instances, it will, with great and harmful prejudice, ignore or condemn or exclude them by introducing some distinction, in order that the authority of those earlier assumptions may remain intact and unharmed. So it was a good answer made by that man who, on being shown a picture hanging in a temple of those who, having taken their vows, had escaped shipwreck, was asked whether he did not now recognize the power of the gods. He asked in turn: “But where are the pictures of those who perished after taking their vows?” The same reasoning can be seen in every superstition, whether in astrology, dreams, omens, nemesis and the like, in which men find such vanities pleasing, and take note of events where they are fulfilled, but where they are not (even if

this happens much more often), they disregard them and pass them by” (Bacon, 1620/1994, pp. 57-58).

It goes back to W. Stanley Jevons (1974/1958), one of the first scholars to write a whole treatise on the scientific method, that men mark where they hit and not where they miss. If I am dogmatic about Meehl’s eye sign, especially if there is countervailing statistical evidence from a controlled study, I am not only having irrational, prideful overconfidence in my brilliant clinical talents, I am ignoring cognitive research and the dismal record of superstitions about four-leaf clovers, black cats, and witchcraft.

William James wrote of ‘over-beliefs’ in religion, but they exist in all areas of life, including science. We cannot live without them, so we cannot be faulted for holding them. To marry, to divorce, to have children, to invest money, to choose a graduate school, to vote for a politician—all these actions rest on beliefs that go beyond the evidence. Spending 50 hours writing a research grant proposal requires an optimistic egocentric over-belief. What is forbidden is deceiving oneself and others as to epistemic status, pretending that something is not an over-belief but is fully credentialed. The moral-scholarly principle involved here is the same one Lakatos (1970) invokes concerning a scientist’s persistent adherence to a degenerating research program: It may sometimes be rational to do so, but what is impermissible is falsifying the record—pretending that the program is not degenerating. I happily confess to a big over-belief in Freud’s list of defense mechanisms, a moderate over-belief in Meehl’s schizotaxia theory, a credentialed, rational belief in my taxometrics, and (some would say) an underbelief in the Big Five. I trust that all these are influenceable by evidence. When a clinician advises, prescribes, or testifies concerning *other* people, whose liberty, money, health, or even life is at stake, the ethical problem becomes acute. I do not have the answer, but I do know that some practitioners (and professors!) take it far too lightly.

I discern three levels of inadequacy in our uncredentialed clinical knowledge. In the first, a sizable amount of research has been conducted and is sufficiently uniform to draw a conclusion, and yet practitioners largely ignore it. An example is the equality or superiority of formal (i.e., algorithmic or mechanical) modes of data combination over informal subjective judgmental combinations, the clinical–statistical controversy. My book on the subject (Meehl, 1954/1996) dealt mainly with theoretical, that is, philosophical, and mathematical considerations; but there were 20

studies that showed, somewhat surprisingly, that clinicians do not do better than even a nonoptimal regression equation or actuarial table. Most textbooks ignored either these facts or the theoretical analysis with which I accompanied them. Some attributed to me the view that objective tests (such as the MMPI) were superior to the interview or history as a basis of prediction. I never said this or anything that implied it, and I do not believe it; the extent to which that view is attributed to me testifies to the unscholarly habits of our profession. The recent meta-analysis by my colleague William Grove finds some 136 empirical comparisons of formal and informal modes of data combination for predictive purposes (see Grove & Meehl, 1996). Only 5% of them show the informal one to be superior, a sufficiently small proportion to be plausibly attributed to sampling error. I am unaware of any controversy in psychology or sociology in which the data are as massive, varied, and uniform as this. Nevertheless, the majority of clinicians continue to act as if these data do not exist, and the majority of textbooks misinform the student that the controversy “remains unsettled.” The most scandalous example of practitioners ignoring large bodies of consistent research data is continued use of tests that have been thoroughly studied over many years and shown to be of negligible validity. I leave readers to pick their own examples.

A second case is when we have adequate research methods but they have not been used to answer important questions. We use the DSM diagnostic categories despite it being doubtful that there are many genuine types, taxa, or disease entities in psychopathology. On the available evidence, it is likely that the best way to characterize most persons coming for psychological help is dimensional rather than typological, and there has been no adequate scientific showing that the typological (sometimes called “the medical model,” a phrase that I never use) is for some unknown reason preferable to locating an individual in a multifactorial space of relevant dimensions. There are several mathematical procedures, some of which have been around for a long time, for determining whether a certain purported type or taxon is genuine or not. We have cluster algorithms, inverse factor analysis, latent class analysis, mixture analysis, and—a method which I am bold enough to say has definitively solved the problem—my taxometric method, which involves several nonredundant independent procedures (Meehl, 1995). There is little justification for continuing

to revise the psychiatrist's system of rubrics instead of using powerful quantitative methods to decide which of them correspond to anything that really exists.

A third and more discouraging category of inadequately credentialed knowledge arises when we do not have appropriate research methods for investigating it. My former colleague, Stanley Schachter, once asked why, since I was seeing psychotherapy patients 10–15 hours a week and I had done experimental work on latent learning in the rat and statistical studies of the MMPI, I had not done any research in psychotherapy. My answer was, "It's because I don't know how." In the early 1950s, Kenneth MacCorquodale and I did a detailed analysis of the verbal behavior of a patient of mine who was a roaring psychotherapeutic success, his social introversion score (that was his presenting complaint) dropping 2.5 standard deviations in 25 sessions. While we got some of the usually alleged changes in such things as distribution of verb tenses, verb/adjective ratio, and positive and negative words, we found ourselves asking whether we had learned anything new about the psychotherapeutic process. How had this man been helped by my gentle ministrations? Despite our tedious and costly protocol analyses and calculations, MacCorquodale and I agreed that we did not learn anything interesting and we did not publish our findings. I decided that either I was not clever enough or the methodology was not developed enough for me to study psychotherapy profitably, and that was the last time I tried. Carl Rogers came to the same conclusion with lots more data. It may be that our general knowledge of emotional conditioning and cognitive processes is inadequate, and I am sure that our knowledge of quantitative psycholinguistics is inadequate, and these are crucial auxiliary theories necessary for a satisfactory theory of how psychotherapy works when it does. It may be that a new sort of statistics (e.g., a variant of graph theory or path analysis) must be devised. That is discouraging because a statistician who has not had clinical experience with the therapeutic session is, I think, unlikely to come up with anything appropriate, whereas most seasoned practitioners do not know enough mathematics, not to mention philosophy of science, to do so.

Having mentioned philosophy of science, I cannot resist a digression. Some clinicians with obscurantist motives, who do not wish to take intellectual responsibility for credentialing their alleged knowledge, take illegitimate comfort from

the death of logical positivism as a philosophic movement. Associated with this is a tendency to brandish Thomas Kuhn's (1962) book the way some TV evangelists brandish the Bible. (Such persons never mention what Kuhn has to say about the social sciences in his book, that they are in such a primitive state that they have not even reached the stage of having a criticizable paradigm!) The distinction between knowledge that brings credentials with it and purported knowledge that does not *has nothing whatever to do* with logical positivism as a discredited philosophic movement. The distinction between scientific evidence and superstition began to be made even before the age we call the Enlightenment, long before the logical positivists appeared on the scene. The point I am making here has nothing to do with positivism, phenomenalism, or the distinction between scientific realism and instrumentalism, nor between behaviorist and cognitive understandings of the mind. None of these distinctions, important as they are in their own right, have anything to do with the difference between knowledge claims that bring credentials and those that do not; between penicillin, which works, and venesection, which does not; between petroleum geology that gets one profitable drilling in six tries (it turns out, statistically it is worth it) and dowsing, which does not. Clinicians who sidestep the issue of knowledge credentials by positivist-bashing are deceiving themselves with a philosophical red herring.

Here is an example of a conjecture, arising from my 10,000 hr of psychotherapy practice, which, if I were a young man with a huge research grant, I would not know how to go about researching. Setting aside the major mental disorders of genetic biochemical origin (such as schizophrenia and major affective disorders) and phobias based upon conditioning (best treated by desensitization), let us consider that type of patient or client that occupies much of practitioners' time. In the last 20 years of my private practice, almost all of my clients were academics or business executives who had William Schofield's YAVIS syndrome, that is, young, attractive, verbal, intelligent, and successful (see Schofield, 1986). Philip S. Holzman (personal communication, 1990) has said that psychoanalysis is not the treatment of choice for patients with a diagnosable mental illness, but rather should be viewed as a growth opportunity for the worried well, and I agree with him about this. In that YAVIS group, I discern three kinds of difficulties. Some clients present almost pure instances of each, although most are mixed.

The first suffer from problems of psychodynamics, classical or not. I still believe, despite all I have learned from Albert Ellis, that for such persons a broadly psychoanalytic approach (sometimes close to classical, although not intended to go on for 10 years) is the treatment of choice. These persons have difficulties arising from the impulse defense conflict and the basic aim of therapy for them is attrition of the defensive system by the procedure of interpretation. We enable the ego to tolerate less distorted derivatives. I confess that part of my reason for continuing to hold this increasingly challenged view is the benefits I received from my personal psychoanalysis. Of course, given my general views about credentialed knowledge, that cannot constitute by itself a good reason. A second kind of client suffers primarily from holding what Ellis calls "irrational" life postulates. Although the intensity and rigidity with which such people cling to these postulates has, of course, a life history and a psychodynamic side, the important therapeutic intervention is to smoke out these crazy postulates and teach the client to challenge them rationally. A third kind of client, in addition to psychodynamic hang-ups and clinging to foolish beliefs, suffers mainly from development of inadequate, ineffectual, and counterproductive instrumental behaviors. The effective interventions are contingency management, desensitization, or aversive conditioning. Freud's basic mistake, in replying to someone suggesting that there should be synthesis after analysis, was to assume that when the libido is "freed up," it will proceed to attach itself to realistic, mature objects. That is because he conceived of the mind as a repository of memories, feelings, and desires, and did not appreciate the extent to which it is a repository of habits. There is no theoretical reason from learning theory (or from common sense) for believing that just because you have brought about attrition of some rigid defenses against knowing what your impulse life or the external world is really like, or have quit saying that everybody has to love you or that you have to do everything perfectly, *therefore* a set of effective habits in work and love will be available "at high strength," to use Skinnerian language.

It is my impression that these three kinds of disorders have a tendency to reinforce one another for reasons that are quite obvious with a little thought, and that each one of them can stand in the way of an effective healing of the other two. But I also think that the strength of the causal influences is markedly asymmetrical. I believe that unre-

solved psychodynamic patterns impair the ability to challenge irrational life postulates more than the other way around. I believe that psychodynamic constraints and persisting irrational life postulates impair the ability to explore more effective behaviors more than the other way around. That suggests the notion of an ideal sequential integrated psychotherapy, which would begin with psychodynamic exploration, be followed by rational-emotive challenging, and terminate with behavior modifying approaches. I have experimented with combining the first two of these to such an extent as, for instance, letting the development of a puzzling hang-up in rational emotive therapy lead to putting the patient on the couch and proceeding psychoanalytically for a while. The difficulty is that the psychoanalytic mode of listening and speaking is so very different from rational emotive therapy that it is hard for the therapist to do both. Whether a single therapist could learn to do such a sequence well, I do not know, but I am inclined to doubt it. The point is that here is a conviction that I can only support by my clinical experience and certain theoretical arguments, but for which I have not a shred of scientific proof. Whether we have presently available methods of quantitative analysis of interview protocols that would, even with huge amounts of money and captive psychotherapists, enable us to check on these conjectures, I do not know, but I am inclined to doubt it.

Judging by the widely received views of 1945—the year of my doctorate—clinical psychology seems an unpredictable, even erratic, discipline. Most clinicians then believed that the most powerful interventions were psychoanalytic or nondirective therapy, that drugs were of little value, that projective methods were vastly superior to structured tests, that clinical judgment was more accurate than regression equations, that genes were unimportant because psychopathology arose from bad child-rearing practices, that the soft neurological signs in schizophrenia were psychosomatic and without etiological relevance, that examining patients' beliefs merely encouraged intellectualization and was therefore useless, that antisocial behavior was basically neurotic and hence must be treated accordingly, that the contribution of experimental psychology to psychodynamics and psychotherapy was Hull's learning theory. All of these opinions have turned out to be incorrect. Ours is a funny field, indeed.

A centennial impels forecasting, but I am

mindful of the wise adage that it is safer to remain silent and be thought stupid than to speak and remove all doubt. However, relying on Popper's view that science is a history of corrected mistakes, I shall rashly record a few predictions.

(a) Utilization of confirmatory factor analysis and taxometric methods will result in a diagnostic system that distinguishes taxa from multifactorial location, with the latter predominating.

(b) A few conditions, such as schizophrenia, manic-depression, panic disorder, and hard-core psychopathy, will be *defined* by their genetic etiology, so that neurological and psychophysiological indicators will play a major role in diagnostics. (Even on today's evidence, it is irrational to give hallucinations a heavier weight than soft neurology in diagnosing schizophrenia.)

(c) Despite the valuable meta-analytic findings of Smith, Glass, and Miller (1980), I expect that therapeutic strategies will turn out to be differentially appropriate over diagnoses, whether taxonic or dimensional.

(d) The assessment procedure will become relatively more statistical, and the sequence of trying alternative interventions (e.g., antidepressive drug of choice, how long to try it, which one to try next) will be based on an optimizing sequential decision-tree.

(e) Direct alteration of basic pathogenic parameters (e.g., anxiety proneness, hedonic capacity) by medications and genetic engineering is a near-certainty, although how soon is an unknown.

(f) As for our many pre-scientific practices, I believe that if we do not take strong steps to clean up our act, some smart lawyers and sophisticated judges will either discipline or discredit us.

Applying scientific method to human behavior is surely not easy, but it is the only way to credentialed knowledge; given the primitive state of our theory and technology, the worst strategy would be to follow the advice of obscurantists and positivist-bashers to wallow in subjectivity. Young clinicians today confront a difficult task, but that helps to make it interesting. I wish them luck, and I hope they have as much fun trying as I have had.

NOTE

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REFERENCES

- Bacon, F. (1994). *Novum organum* (P. Urbach & J. Gibson, Eds. and Trans.). Chicago, IL: Open Court. (Original work published 1620)
- Freud, S. (1959). Character and anal erotism. In J. Strachey (Ed. and Trans.), *Standard Edition of the Complete Psychological Works of Sigmund Freud* (Vol. 9, pp. 167-175). London: Hogarth Press. (Original work published 1908)
- Freud, S. (1961). Civilization and its discontents. In J. Strachey (Ed. and Trans.), *Standard Edition of the Complete Psychological Works of Sigmund Freud* (Vol. 21, pp. 64-145). London: Hogarth Press. (Original work published 1930)
- Grove, W. M., & Meehl, P. E. (1996). Comparative efficiency of informal (subjective, impressionistic) and formal (mechanical, algorithmic) prediction procedures: The clinical/statistical controversy. *Psychology, Public Policy, and Law*, 2, 293-323.
- Jevons, W. S. (1958). *The principles of science*. New York: Dover. (Original publication 1874)
- Kraemer, H., & Sprenger, J. (1970). *Malleus Malaeficarum*. New York: Blom. (Original work published 1487)
- Kuhn, T. (1962). *The structure of scientific revolutions*. Chicago, IL: University of Chicago Press.
- Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 91-195). Cambridge: Cambridge University Press. Reprinted in J. Worrall & G. Currie (Eds.), *Imre Lakatos: Philosophical papers. Vol. 1: The methodology of scientific research programmes* (pp. 8-101). New York: Cambridge University Press, 1978.
- Meehl, P. E. (1983). Subjectivity in psychoanalytic inference: The nagging persistence of Wilhelm Fliess's Achensee question. In J. Earman (Ed.), *Minnesota studies in the philosophy of science. Vol. 10, Testing scientific theories* (pp. 349-411). Minneapolis: University of Minnesota Press.
- Meehl, P. E. (1995). Bootstraps taxometrics: Solving the classification problem in psychopathology. *American Psychologist*, 50, 266-275.
- Meehl, P. E. (1996). *Clinical versus statistical prediction: a theoretical analysis and a review of the evidence*. Northvale, NJ: Jason Aronson. (Original publication 1954)
- Schofield, W. (1986). *Psychotherapy: the purchase of friendship* (2d ed.). New Brunswick, NJ: Transaction Books. (Original publication 1964)
- Smith, M. L., Glass, G. V., & Miller, T. I. (1980). *The benefits of psychotherapy*. Baltimore, MD: Johns Hopkins University Press.

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