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Training in clear thinking should begin early. Accordingly, this classic appeal for clear thinking about psychology has been adapted for use in undergraduate instruction. While retaining the structure and flavor of the original, length has been reduced to 25% of the original. In addition, some difficult phrasing and vocabulary have been made more accessible. This is not an improved version - only one more likely to find its way into the undergraduate curriculum. It is an invitation to read the original and enticement to explore his many classic articles on psychometrics, psychodiagnosis, philosophy of science, clinical decision making, professional psychology, psychology and the law, and other topics. Some text (< 1%) has been added to create smooth transitions and to define difficult concepts. Quotations must be checked against the original article. - Everett Waters

Why I Do Not Attend Case Conferences

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I have for many years been accustomed to the social fact that colleagues and students find some of my beliefs and attitudes paradoxical (some would, perhaps, use the stronger word contradictory). I flatter myself that this arises primarily because my views (like the world) are complex and cannot be classified as uniformly behavioristic, Freudian, actuarial, positivist, or hereditarian. I also find that psychologists who visit Minneapolis for the first time and drop in for a chat generally show mild psychic shock when they find a couch in my office and a picture of Sigmund Freud on the wall. Apparently one is not supposed to think or practice psychoanalytically if he understands something about philosophy of science, thinks that genes are important for psychology, knows how to take a partial derivative, enjoys and esteems rational-emotive therapist Albert Ellis, or is interested in optimizing the prediction of behavior by the use of actuarial methods!

On the local scene, one manifestation of this puzzlement about my views and preferences goes like this: "Dr. Meehl sees patients on the campus and at the Nicollet Clinic, averaging, so we are told, around a dozen hours a week of psychotherapy and has done so almost continuously for almost thirty years. It seems evident that Meehl is 'clinically oriented,' that his expressed views about the importance of professional practice are sincere. It is therefore puzzling to us students, and disappointing to us

after having been stimulated by him as a lecturer, to find that he almost never shows up in the clinical settings where we take our clerkship and internship. We never see Dr. Meehl at a *case conference* (a round-table presentation and discussion by experts of a single patient's diagnosis and treatment). Why is this?"

The main reason I rarely show up at case conferences is easily stated: The intellectual level is so low that I find them boring, sometimes even offensive. This is in contrast with case conferences in internal medicine or neurology - both of which I have usually found stimulating and illuminating. I do not believe my attitude is as unusual as it may seem. I think I am merely more honest than most clinical psychologists about admitting my reaction. Witness the fact that the staff conferences in the Medical School where I work are typically attended by only a minority of the faculty - usually those who must be there as part of their paid responsibility, or who have some other special reason (such as invitation) for attending a particular one. If the professional faculty found them worthwhile, they wouldn't be so reluctant to spend their time that way. While we wait for adequate research on "What's the matter with the typical case conference," I will present here some of my own impressions. The first portion of the paper will be highly critical and aggressively polemic. (If you want to shake people up, you have to raise a lit-

tle hell.) The second part, while not claiming to offer a definitive solution to the problem, proposes some directions that might lead to a significant improvement over current conditions.

Part I: What's Wrong?

1. *Buddy-buddy syndrome.* In one respect the clinical case conference is no different from other academic group phenomena such as committee meetings, in that many intelligent, educated, sane, rational persons seem to undergo a kind of intellectual deterioration when they gather around a table in one room. The cognitive degradation and feckless vocalization characteristic of committees are too well known to require comment. Somehow the group situation brings out the worst in many people, and results in an intellectual functioning that is at the lowest common denominator, which in clinical psychology and psychiatry is likely to be pretty low.

2. *"All evidence is equally good."* This absurd idea perhaps arises from the "groupy," affiliative tendency of behavioral scientists in "soft" fields like clinical, counseling, personality, and social psychology. It seems that there are many professionals for whom committee work and conferences represent part of their social, intellectual, and erotic life. If you take that "groupy" attitude, you tend to have a sort of mush-headed approach which says that everybody in the room has something to contribute (absurd on the face of it, since most persons don't usually have anything worthwhile to contribute about anything, especially if it's the least bit complicated). In order to maintain the fiction that everybody's ideas are worthwhile, it is necessary to lower the standards for what is considered useful information. As a result, a casual anecdote about one's senile uncle as remembered from childhood is given the same time and attention as someone else's information based on a high-quality experimental or field-actuarial study. Nobody would be prepared to defend this rationally in a seminar on research methods, but we put up with it in our psychiatric case conferences.

3. *Reward everything - gold and garbage alike.* The tradition of exaggerated tenderness in psychiatry and psychology reflects our "therapeutic attitude" and contrasts with that of scholars in fields like philosophy or law, where a dumb argument is called a dumb argument, and he who makes a dumb argument can expect to be slapped down by his peers. Try this in a psychiatric case conference and you will be heard with horror and disbelief. Instead, the most inane remark is received with joy and open arms as part of the groupthink process. Conse-

quently the educational function, for either staff or students, is prevented from getting off the ground. Any psychologist should know that part of the process of training or educating is to reward good thinking versus bad, effective versus ineffective, correct versus incorrect behaviors. If all behavior is rewarded by friendly attention and nobody is ever non-reinforced (let alone punished!) for talking foolishly, it is unlikely that significant educational growth will take place.

A corollary of the "reward everything" policy with respect to evidence and arguments is the absurd idea that, "*everyone is right - or at least, nobody is wrong*". A nice quotation from the statistician M. G. Kendall is relevant here: "A friend of mine once remarked to me that if some people asserted that the earth rotated from East to West and others that it rotated from West to East, there would always be a few well meaning citizens to suggest that perhaps there was something to be said for both sides and that maybe it did a little of one and a little of the other, or that the truth probably lay between the extremes and perhaps it did not rotate at all" (Kendall, 1949, p. 115).

4. *Tolerance of feeble inferences (e.g., irrelevancies).* The ordinary rules of scientific inference and general principles of human development, which everybody takes for granted in a neurology case conference, are somehow forgotten in a psychiatric case conference. I have heard professionals say things in a psychiatric staff conference which I am certain they would never have said about a comparable problem in a neurology case conference. Example: In a recent case conference the immediate task was to decide whether a particular patient was better diagnosed as schizophrenia or an anxiety reaction. Any well-read clinician would easily recognize that despite the patient's pervasive anxiety, the diagnosis should be schizophrenia (Hoch-Polatin's "pseudoneurotic schizophrenia" syndrome; Hoch and Polatin, 1949; Meehl, 1964). The psychiatrist presiding at the conference argued that the patient was probably latently or manifestly schizophrenic. He argued thus partly because - in addition to her schizophrenic MMPI profile - she had a vivid and sustained hallucinatory experience immediately preceding her entry into the hospital. She saw a Ku Klux Klansman standing in the living room, in full regalia, eyeing her malignantly and making threatening gestures with a knife, this hallucination lasting for several minutes. The presiding psychiatrist (and myself) felt that this would have to be considered pretty strong evidence for our schizophrenic diagnosis as against the anxiety-neurosis alternative.

At this point one of the nurses said, "I don't see why Dr. Koutsky and Dr. Meehl are laying emphasis upon this Ku Klux Klansman. After all, I remember having an imaginary companion when I was a little girl." Now suppose that this well-meaning nurse, whose remark was greeted with the usual respectful attention to "a contribution," had been attending a case conference on the neurology service. And suppose that in suggesting that a patient might have a spinal cord tumor the presiding neurologist had noted that the patient had lost bladder control. It would never occur to this nurse to advance, as an argument against the spinal tumor hypothesis, the fact that she used to wet her pants when she was a little girl. But somehow when she gets into a psychiatric case conference she forgets we are considering a seriously ill person and that if the diagnosis isn't spinal tumor it is certainly some other significant problem. And she overlooks the fact that a behavior can be entirely normal in children and entirely not normal in an adult. Equating a childhood imaginary companion with an adult's experiencing a clear and persisting visual hallucination of a Ku Klux Klansman is of course just silly - but in a psychiatry case conference no one would be so tactless as to point this out.

5. *Failure to distinguish between an inclusion test and an exclusion test:* Asked to decide between two diagnoses, schizophrenia and manic-depressive psychosis, a psychology trainee argues against schizophrenia on the ground that the patient does not have delusions or hallucinations. Of course this is just plain uninformed, because delusions and hallucinations are merely "accessory" symptoms, present in some schizophrenics but not all, and they are not part of the indicator family that "defines" the disease (Bleuler, 1911). Delusions and hallucinations (without indications of intoxication or disorientation) are absent in many schizophrenics. This is not an idiosyncratic clinical opinion of mine. It is a theory found in all of the textbooks, it is in the standard nomenclature, it is in Kraepelin and Bleuler, who defined the entity "schizophrenia". Having delusions or hallucinations is good evidence toward a schizophrenia diagnosis (inclusion test); not having them is not equally good evidence against the diagnosis (exclusion test). But when I point this out forcefully, the trainee looks at me as if I were a mean ogre.

6. *Failure to distinguish between mere consistency of a sign and differential weight of a sign.* Once the diagnosis has been narrowed to two or three possibilities, it is inappropriate to cite as evidence signs or symptoms that are nondifferentiating as between them. This is so obvious a mistake that

one thinks it would never happen; but some clinicians do it regularly. In distinguishing between a sociopathic personality, an acting-out neurotic delinquent, and a garden-variety "sociological" criminal, it is fallacious to argue that the patient was a marked underachiever or a high school dropout, in spite of high IQ, as grounds for a diagnosis of sociopathic personality. These are good reasons for eliminating some diagnoses and focusing on psychopathy, acting out neurotic, and garden variety criminality. But this evidence is of no use in distinguishing among them. This sign has lost its diagnostic relevancy at this stage of the investigation. This illustrates one of the shared features of case conferences in psychiatry, namely, the tendency to mention things that don't make any difference one way or the other. The idea seems to be that as long as something is true, or is believed to be true, or is possibly true, it is worth mentioning! In other medical specialties in order to be worth mentioning the statement must not only be true but it must also argue for one diagnosis, outlook, or treatment, rather than another.

7. *Shift in the evidential standard, depending upon whose ox is being gored.* A favorite tactic of case conference gamesmanship is to hold others to a higher standard than yourself. When you are presenting your own diagnostic case, you permit all kinds of weak inferences (mediated by weak theoretical constructions and psychodynamic conclusions); then when the other fellow is making his case for a different diagnosis, you become superscientific and behavioristic, making comments like "Well, of course, all we actually know is the behavior." It is not intellectually honest to hold up different people's opinions to different standards.

8. *Ignorance (or repression) of statistical logic.* A whole class of loosely related errors made in the clinical case conference arises from forgetting (on the part of the psychologist) or never having learned (in the case of the psychiatrist and social worker) certain elementary statistical or psychometric principles. Examples are the following:

a. *Forgetting Bayes' Theorem.* One should always keep in mind the importance of base rates - how common a disorder is in the population. If the disorder is extremely rare, you don't get very much mileage out of a moderately strong sign or symptom. On the other hand, when the disorder is rather common, you get mileage out of an additional fact, but you don't really "need it much," so to speak. The logic here has been outlined by Meehl and Rosen (1955) and applies in a clinical case conference just as strongly as in research.

b. *Forgetting about unreliability when interpreting score changes or difference scores.* Despite a mass of evidence, examples, warnings, and criticism, the practice of overinterpreting small differences in test scores remains far too common in clinical case conferences. Who cares whether the patient "scored two points higher this week" if scores are only accurate to within 10-20 points. And who cares if the patient "did well on the Block Design subtest but seemed to enjoy it less than Picture Arrangement"?

c. *Reliance upon inadequate behavior samples for trait attribution.* This is the error of believing that you can estimate the proportion of white marbles in a jar after sampling only a couple of marbles. It is particularly serious in clinical practice because, in addition to observing so little behavior, we have almost no control over the conscious or unconscious factors that determine which bit of someone's behavior we get to observe (or which part of our observations we actually remember). It is obvious that over a period of several hours or days of unsystematic observation, practically any human being is likely to emit at least a few behaviors that could be over-used to support almost any personality description or diagnosis.

(Note: Section d omitted here for brevity – Ed.)

e. *Failing to understand probability logic as applied to the single case.* This disability is apparently common in the psychiatric profession and strangely enough is also found among clinical psychologists in spite of their academic training in statistical reasoning. There are still tough, unsolved philosophical problems connected with the application of probabilities (which are always based on groups of people) to individual cases. But we cannot come to grips with those problems, or arrive at a workable decision policy in case conferences, unless we have gotten beyond the familiar blunders that should have been trained out of any aspiring clinician early in his training.

The most common error is the cliché that "We aren't dealing with groups, we are dealing with this individual case." True enough. But it is also true that not betting on the empirically most likely (or against the least likely) diagnosis or prognosis may help in a few individual cases, but it will almost certainly increase your lifetime error rate (Meehl, 1957). Clearly there are occasions when you should use your head instead of the formula. But which occasions they are is most emphatically not clear. The best evidence is that these occasions are much rarer than most clinicians suppose.

9. *Inappropriate task specification.* Nobody seems very clear about which kinds of tasks are well performed in the case conference context and which would be better performed in other ways. Case conferences are well suited to gathering and evaluating observations and interpretations from people who have seen a patient in different contexts and who bring to bear different kind of expertise – with the goal of making a diagnosis, treatment decision, or other practical *decision* that benefits the patient. I do not think case conferences are well spent listening to someone spinning out of complicated ideas about a patient's mental life on the basis of what we would have to consider "superficial" contact when, in any event, such information is neither qualitatively or quantitatively strong enough to guide important decisions.

10. *Asking pointless questions.* Participants in a case conference frequently ask questions the answers to which make no conceivable difference to the handling of the case. I have often thought that the clinician in charge of the case conference should emulate my colleague and professor of law, David Blyden. When a law student advanced a stupid argument about the case being discussed, he would respond with a blank stare and the question "And therefore?" This would usually elicit some further response from the student (attempting to present the next link in an argumentative chain). This would in turn be greeted by the same blank stare and the same "And therefore?" I daresay Professor Dryden made the law students nervous; but he also forced them to think. I suspect that one who persisted in asking the question "And therefore?" every time somebody made a half-baked contribution to the case conference would wreak havoc, but it might be an educational experience for all concerned.

11. *Ambiguity of professional roles.* When the conference is not confined to one of the three professions in the team, there may arise a sticky problem about roles. For example, in mixed-group conferences I note a tendency to assume that the psychologist's job should be to present the psychological tests and that he is only very gingerly and tentatively to talk about anything else. I think this attitude is ridiculous. I can conduct a diagnostic interview or take a history as well as most psychiatrists, and non-test data are just as much part of my subject matter as they are of the psychiatrist's. Similarly, if a physician has developed clinical competence in interpreting Rorschachs or MMPI profiles or practicing behavior modification, I listen to what he says without regard to trade-union considerations.

12. *Some common fallacies.* Not all of these fallacies are clearly visible in case conferences, and none of them is confined to the case conference, being part of the general collection of sloppy thinking habits with which much American psychiatry is infected. I have given some of them special "catchy" names, admittedly for propaganda purposes but also as an aid to memory.

a. *Barnum effect.* Saying trivial things that are true of practically all psychiatric patients, or sometimes of practically all human beings this is the Barnum effect. (From the habit of circus side show announcers to entice customers with exotically pronounced but, in fact rather empty claims: "See the snake lady - she walks, she talks, she breathes the air around us" - of course she does.) It is not illuminating to be told that a mental patient has intrapsychic conflicts, ambivalent object relations, sexual inhibitions, or a damaged self-image! (Cf. Meehl, 1956; Sundberg, 1955; Tallent, 1958; Forer, 1949; Ulrich, Stachnik, and Stainton, 1963; and Paterson in Blum and Balinsky, 1951, p. 47.)

b. *Sick-sick fallacy ("pathological set").* There is a widespread tendency for people in the mental health field to think of themselves and their values as touchstones for evaluating mental health. This tends to link not only their ideas about how the mind works but sometimes their social role, and even to some extent their religious and political beliefs and values, with freedom from disease or aberration. Therefore if we find somebody very unlike us in these respects we see him as being sick. The psychiatric establishment officially makes a point of never doing this and then proceeds to do it routinely. Thus, for example, many family psychiatrists have a stereotype of what the healthy family ought to be; and if anybody's family life does not meet these criteria, this is taken as a sign of pathology.

c. *"Me too" fallacy* (the objection that "anyone would do that"). This is the opposite of the over-pathologizing "sick-sick" fallacy, and one might therefore suppose that clinicians fond of committing the "sick-sick" fallacy would be unlikely to commit the "me too" fallacy. I have no data on this, but my impression is that the same clinicians have a tendency to commit both. Perhaps the common property is not conservatism or liberalism in diagnosing pathology but mere sloppy-headedness.

I was first forcibly struck with the significance and seductiveness of the "me too" fallacy when I was a graduate student in clinical training. One of my first diagnostic workups was with a girl in late adolescence (a classic Cleckley (1964) psychopath) who was brought in for evaluation on a district court

order. The problem which brought her in was that she had "in a fit of pique" hit her foster mother over the head with a lamp base. One important thing to assess, from the standpoint of the court's inquiry, was the extent to which the patient could exert behavioral control over her impulses. One of the rules of our psychiatric service at the time was that patients could smoke only at certain times. This patient had come to the nurse wanting to smoke at another time. When told "No", she began pounding with her fists on the nurse's desk and then flung herself on the floor where she kicked and screamed like a small child having a tantrum. When this episode was discussed in the weekly conference with the junior medical students, the student physician told Dr. Hathaway, the clinical psychologist presiding at the conference, that he didn't see any point in "making a lot out of this tantrum" because, "after all, anybody might act the same way under the circumstances." The dialogue continued thus:

DR HATHAWAY: "How do you mean 'under the circumstances'."

MEDICAL STUDENT: "Well, she wanted a cigarette and it's kind of a silly rule."

DR HATHAWAY: "Let's assume it's a silly rule, but it is a rule which she knows about, and she knows that the tantrum is probably going to deprive her of some privileges on the station. Would you act this way under the circumstances?"

MEDICAL STUDENT: "Sure I would."

DR HATHAWAY: "Now, think a moment would you, really?"

MEDICAL STUDENT (thoughtful): "Well, perhaps I wouldn't, actually."

And of course he wouldn't. Point: Participants in case conferences too readily minimize recognized signs or symptoms of pathology by thinking, "Anybody would do this." But *would* just anybody do it? What is the actual objective probability of a mentally healthy person behaving just this way? Perhaps you might feel an impulse or have a momentary thought similar to that of the patient. The question is, would you act out the impulse as the patient did, simply because experienced it?

d. *Uncle George's pancakes fallacy.* This is a variant of the "me too" fallacy, once removed; rather than referring to what anybody would do or what you yourself would do, you call to mind a friend or relative who exhibited a sign or symptom similar to that of the patient. For example, a patient does not like to throw away leftover pancakes and he stores

them in the attic. A mitigating clinician says, "Why, there is nothing so terrible about that - I remember good old Uncle George from my childhood, he used to store uneaten pancakes in the attic."

The underlying premise in this kind of objection seems to be the notion that none of one's own friends or family (especially if they were undiagnosed and unhospitalized) could have been mentally ill. Once this premise is made explicit, the fallacy is obvious. The proper conclusion from such a personal recollection is, of course, not that the patient is mentally well but that good old Uncle George, whatever may have been his other delightful qualities, was mentally aberrated.

e. *Multiple Napoleons fallacy*. This is the mush-headed objection that "Well, it may not be 'real' to us, but it's 'real' to him." It is unnecessary to bring a philosopher into the case conference before one can recognize a distinction between reality and delusion as clinical categories. So far as I am aware, none of them would dispute that a man who believes he is Napoleon or has invented a perpetual-motion machine is crazy. If I think the moon is made of green cheese and you think it's a piece of rock, one of us must be wrong. To point out that the aberrated cognitions of a delusional patient "seem real to him" is a complete waste of time. The statement "It is reality to him," which is philosophically either trivial or false, is also clinically misleading. Nevertheless I have actually heard clinicians in conference invoke this kind of notion, as if the distinction between the real and the imaginary had no standing in our assessing a patient.

f. *Crummy criterion fallacy*. It is remarkable that eighteen years after the publication of Cronbach and Meehl's "Construct Validity in Psychological Tests" (1955) and so many other treatises on the meaning of tests that clinical psychology trainees (and some full professors) persist in a naive undergraduate view of psychometric validity. Repeatedly in clinical case conferences one finds psychologists seeing their task as "explaining away" data from psychological tests rather than genuinely integrating them with the interview, life-history, and ward-behavior material on the patient. If data from a well validated test such as the MMPI indicates strongly that the patient is profoundly depressed or has a schizoid makeup, do we really have a problem if it doesn't agree with the global impression of a first-year psychiatric resident? No, and yet we often find the psychologist virtually apologizing for the test. Now this is silly. Even from the armchair, we start with the fact that an MMPI profile represents the statistical distillation of 550 verbal responses which

is considerably in excess of what the clinician has elicited from the patient in most instances.

The methodological point is so obvious that it is almost embarrassing to explain it, but I gather it is still necessary. Point: If a psychometric device has been empirically constructed and cross-validated in reliance upon the average statistical correctness of a series of clinical judgments, including judgments by well-trained clinicians, there is a pretty good probability that the score patient reflects the patient's personality structure and dynamics better than does the clinical judgment of an individual contributor to the case conference - even if he is a seasoned practitioner, and certainly if he is a clinical fledgling. The psychologist who doesn't understand this point is not even in the ball park of clinical sophistication. Discrepancies between psychometric tests and clinical observations raise important questions: What speculations would we have about discrepancies of this kind? What kinds of research might we carry out in order to check these speculations? Are there identifiable types discrepancies for which the psychometrics are likely to be correct, and others for which the clinical observations should prevail? I do not assert that one never hears these important metaquestions asked in the case conference; but you can attend a hundred conferences without hearing them raised a dozen times.

g. *"Understanding it makes it normal"*. This is a psychiatric variant of the notion that understanding behavior makes that behavior ethically permissible or "excusable". I once heard a clinical psychologist say that it was "not important" whether a defendant was legally insane, since even if he was sane his homicide was "dynamically understandable" (and therefore excusable). As for T. Eugene Thompson, the St. Paul lawyer who cold bloodedly murdered his wife to get a million dollars from life insurance, this psychologist argued that "I suppose if I knew enough about T. Eugene Thompson, like the way his wife sometimes talked to him at breakfast, I would understand why he did it." I gather that this psychologist (a Ph.D.!!) believes that if T. Eugene Thompson's wife was sometimes grumpy in the morning, he was entitled to kill her.

h. *Assumption that content and dynamics explain why this person is abnormal*. Of all the methodological errors committed in the name of dynamic psychiatry, this one is probably the most widespread, unquestioned, and seductive. The "reasoning" involved is simple. Any individual under study in a clinical case conference comes to be there, unless there has been some sort of mistake, because he has psychiatric or medical symptoms, gross social in-

competence (delinquency, economic dependency), or extreme deviations in characterological structure. In addition, simply because he is a human being - namely, he has conflicts and frustrations - there will be areas of life in which he is less than optimally satisfied, aspects of reality he tends to distort, and performance domains in which he is less than maximally effective. There is nobody who can honestly and insightfully say that he is always efficient in his work, likes everyone he knows, is idyllically happy in his marriage and his job, that he always finds life interesting rather than boring, and the like. If you examine the contents of a mental patient's mind, he will, by and large, have pretty much the same things on his mind as the rest of us do.

The seductive fallacy consists in assuming, that the conflicts, failures, frustrations, dissatisfactions and other characteristics he shares with the rest of us account for the medical, psychological, or social aberrations that define him as a patient. But by and large, the research literature on retrospective data for persons who have become mentally ill shows only rather weak and frequently inconsistent statistical relations between purportedly pathogenic background factors and mental illness (e.g., Schofield and Balian, 1959; Frank, 1965; Gottesman and Shields, 1972). I do not object to speculating whether a certain event in the patient's past or a certain kind of current mental conflict may have played an important role in producing his present pathological behavior or phenomenology. I merely point out that most of the time these are little more than speculations. The tradition in case conferences is to take almost any kind of unpleasant fact about the person's concerns or deprivations, present or historical, as of course playing an important causal role.

(Note: Section i omitted here for brevity - Ed.)

j. *The spun-glass theory of the mind.* Every great intellectual and social movement seems to carry some "bad" correlates that may not, strictly speaking, follow logically from society's acceptance of the "good" components of the movement but that psychologically have a tendency to flow therefrom. One undesirable side effect of the mental hygiene movement and the over-all tradition of dynamic psychiatry has been the development among educated persons of what I call the "spun-glass theory of the mind." This is the doctrine that the human organism, adult or child, is constituted of such frail material, is of such exquisite psychological delicacy, that rather minor, garden-variety frustrations, deprivations, criticisms, rejections, or failure experiences are likely to result in major traumas.

Example: A pre-adolescent male with a prostitute mother and a violent, drunken father, living in marginal economic circumstances in a high-delinquency neighborhood, rejected by his parents, his peer group, and the teachers in his school. He had been treated by a therapist because of behavior problems and morbid fantasies. The treatment was considered successful and this was to be his last interview before discharge. Shortly before the seminar was scheduled to be held, the social worker informed us that we really could not go ahead with the interview as planned - because it was scheduled for a different room from the office in which the child was accustomed to being interviewed. She felt that to interview him in this "strange situation" (= different office) might have a traumatic effect and undo the successful achievements of the therapy. This is the spun-glass theory of the mind with a vengeance. Here is this poor child, judged well enough to return to a very harsh environment; yet, despite the "successful" psychotherapy, he is considered so fragile that these therapeutic achievements could be wiped out by having an interview in a different office! I submit that the best way to describe that combination of views is that it is just plain silly.

(Note: Section k, omitted here for brevity - Ed.)

l. *Neglect of overlap.* This one is so trite and so much a part elementary statistics instruction that it shouldn't need mention. But it persists in journals and case conferences. The question before us here is the *application* of a statistically significant difference to real decision making tasks. Suppose I have devised the Midwestern Multiphasic Tennis-Ball Projection Test which I allege to be clinically useful in discriminating schizophrenics from anxiety-neurotics. Let us suppose that we have run the test on a carefully diagnosed sample of 100 schizophrenics and 100 anxiety-neurotics. And let us suppose we succeed in achieving a "statistically significant difference" between the two groups at the $p = .01$ level (about par for the course in most journal articles of this sort). A little arithmetic shows that the ratio of the mean difference between the two groups is about .37 standard deviations. Entering normal curve tables we find that at best using the test to decide between schizophrenics and anxiety-neurotics would give a measly 7 percent improvement over what we could achieve by flipping pennies. From my perusal of the current clinical literature I think it not an unfair exaggeration to say that a considerable number (perhaps the majority) of the psychological test criteria urged upon us for clinical use are close to worthless. A scientific cost accounting of their role in the

decision-making process would usually not justify the expense to the patient (or the taxpayer) in the use of skilled clinical time required to administer and score the instrument and to present it in evidence at the case conference.

(Note: Sections m,n,o omitted here for brevity – Ed.)

p. Double standard of evidential standards. I have no objection if professionals choose to be extremely rigorous about their standards of evidence. But they should recognize that if they adopt that policy, many of the assertions made in a case conference ought not to be uttered because they cannot meet such a tough standard. Neither do I have any objection to freewheeling speculation; I am quite willing to engage in it myself. You can play it tight, or you can play it loose. What I find objectionable in staff conferences is a tendency to shift the criterion of tightness so that the evidence offered is upgraded or downgraded in the service of polemical interests. Example: A psychologist tells me that he is perfectly confident that psychotherapy benefits psychotic depressions, his reason being that his personal experience shows this. But he rejects my similar impression that shock therapy can also be useful – arguing that he has never seen a single patient helped by shock therapy. When challenged with the published evidence indicating that shock therapy can be quite effective, he says that those experiments are not perfect, and further adds, "You can prove anything by experiments." (Believe it or not, these are quotations!) I confess I am at a loss to know how I can profitably pursue a conversation conducted on these ground rules. He is willing (1) to rely upon his casual impressions that psychotherapy helps patients, but (2) to deny my similarly supported impression that shock treatment helps patients, and (3) to reject the controlled research on the subject of electroshock - which meets considerably tighter standards evidentially than either *his* clinical impressions or *mine* - on the grounds that it is not perfectly trustworthy. It is not intellectually honest or, I would argue, clinically responsible thus to vary your tightness-looseness standards when evaluating conflicting evidence on the same issue.

Part II: Suggestions for Improvement

The preceding discussion has admittedly been almost wholly destructive criticism. I don't really mind it much when my colleagues or students ignore me or disagree (interestingly) with me - but I become irritated when they bore me. It is annoying to walk across campus to the University hospital for a case conference only to be served such intellectual delicacies as "The way a person is perceived by his

family affects the way he feels about himself - it's a dynamic interaction," or "Schizophrenia is not like mumps or measles." However, having expressed some longstanding irks and, I hope, having scored a few valid points about what is wrong with most case conferences in psychiatry or clinical psychology, I feel an obligation to try to say something constructive. Not that I accept the Pollyanna cliché that purely destructive criticism is inadmissible. This has always struck me as a rather stupid position, since it is perfectly possible to see with blinding clarity (and usefully point out) that something is awry without thereby being clever enough to know how to cure it. Whether the following proposals for improving the quality of clinical case conferences are sound does not affect the validity of the preceding critical analysis. I invite the reader who does not find himself sympathetic to my proposed solutions to look for alternative solutions of his own.

The first suggestion that comes to the mind of anyone whose training emphasized measurement techniques in psychology is to upgrade the psychometric training of those involved. Obviously this is not something one can go about accomplishing directly by administrative fiat. We can't pass an ordinance requiring of the cosmos that more people should have superhigh IQ's! However, several top schools (Minnesota included) have in recent years opted for a marked reduction in size and goals of their Ph.D. clinical psychology training programs, which has permitted the imposition of tougher "scholarly standards."

More difficult to assess, and therefore more subject to my personal biases, is the question of value orientation, what "turns people on." In my graduate school days, those of my peers who went over to the University Hospitals to work on the psychiatric ward and with Dr. Hathaway on MMPI development were students having both a strong interest in helping real flesh-and-blood patients (not to mention the fun of wearing a white coat!) and intense intellectual curiosity. But most observers agree with me that strong cognitive passions (and their reflection in highly scholarly achievement and research visibility) have, alas, a distinct tendency to be negatively associated with a preference for spending many hours per week in service-oriented, face-to-face patient contact.

Conversely, when the particular psychology department has a reputation for an "applied emphasis," and when the criteria of selection become somewhat less scientifically or intellectually oriented, then one finds an increasing number of trainees in the program who are really not "turned on" by the life of

the intellect. These students, admirable as human beings and doubtless well-motivated healers, find themselves somewhat bored, and in some cases actively irked, by abstract ideas.

As I said above, I am somewhat old-fashioned in these attitudes. I believe there is no substitute for brains. I do not believe the difference between an IQ of 135 - perfectly adequate to get a respectable Ph. D. degree in clinical psychology at a state university - and an IQ of 185 is an unimportant difference between two human beings (cf. McNemar, 1964). Nor do I believe a person, even if basically bright, can be intellectually *exciting* unless he is intellectually *excited*. It astonishes me that so many persons enter academic life despite having what, to all appearances, is a rather feeble capacity for becoming excited about ideas. This aspect of the case conference problem - the fact that many of its participants are not first-class intellects in either ability or values - is obviously not curable by any modification in format. However, without being unkindly elitist, we might try to convey (gently but firmly) the message that if you don't have anything worthwhile to say, you should probably shut up. The current practice, based upon a kind of "encounter group" orientation to case conferences, seems to assume that everybody should get into the act regardless of how bright he is or what he knows, either clinically or theoretically. I view this attitude as preposterous on the face of it. The plain fact is that what most people have to say about anything complicated is not worth listening to. Or, as my medical colleague Dr. Howard Horns put it in a lovely metaphysical witticism, "Most people's thoughts are worth their weight in gold."

If it is argued that you can't prevent people who have nothing significant to contribute from talking without being cruel or discourteous, I submit that this is empirically false. I point to case conferences in other specialties like neurology and internal medicine, where, so far as I have observed, there is no social discourtesy or cruelty manifested by those in charge; but the general atmosphere is nevertheless one which says, in effect, "Unless you know what you are talking about and have reason to think that you are saying something really educational for the rest of us or beneficial to the patient, you would be well advised to remain silent. Mere yakking for yakking's sake is not valued in this club." I have rarely had to listen to trivia, confused mentation, plain ignorance, or irrelevancies when I have attended case conferences in internal medicine or neurology, or the clinicopathological conference on the medical service. If an atmosphere of decent intellectual

scholarly standards can be created and maintained on those services, it should be possible to approximate the same thing in clinical psychology and psychiatry.

One of the vices of the present system is that so much time is thus spent (frequently because of inadequate preparation plus the inefficient oral presentation of history material) that in a conference scheduled to last an hour and a half, by the time we are ready to see the live patient, whoever is in charge of the conference and interviewing the patient is so uncomfortably conscious of how little time remains that the interview is almost *pro forma*. I have sat through conferences in which the first hour was spent in oral presentation of a *melange* of piddling and disconnected facts (including, say, that the patient had a great-uncle who died of cancer - the patient never having known his uncle); the patient then came in for ten minutes, leaving twenty minutes for a discussion of diagnosis, dynamics, and the treatment. This is simply absurd.

For educational purposes, I think experience shows that no conference should be scheduled to run less than an hour and a half, and I myself would advocate two hours. Colleagues warn me that people's attention can't be held for two hours. I agree that you can't hold their attention with a bunch of poorly prepared third-raters doing a deadly presentation of meaningless material. But some changes (inspired by my comments above) in who is talking and what we are talking about should make it possible to hold people's interest for two hours. Most of us find we can run a two-hour seminar provided we run it right (that means, incidentally, not listening weekly to student literature reports!); and I therefore believe that two hours is feasible for good case conferences.

Analogously to the seminar situation, anybody who has been around academia very long, and who remembers how he felt as a student, is aware that students do not much enjoy listening to each other. Admittedly student presentations serve an educational function for the presenter, although I see no reason for assuming that a first-year trainee, who has never attended any sort of conference before, is "ready" to begin his active learning process by presenting a case. In any event, we sacrifice a good deal of other students' valuable time when we force them to listen to an incompetent and boring presentation by somebody who really isn't ready to do it.

At the very least, I would suggest a change in the way we assign major responsibility for presentation to advanced students, faculty, and near beginners. I recognize that this is predicated on the old fashioned

idea that full professors with twenty years of teaching, research, and clinical experience should, on the average, be capable of serving as educational models for beginning clinicians.

In either the present or the revised system, one must allow sufficient time so that discussion of the diagnosis, dynamics, and treatment can be carried on at a respectable level of intellectual depth. Questions like "Is there such an entity as schizophrenia?" or "Should the construct 'sociopathic personality' be defined mainly by psychological-trait criteria, or by life-history criteria (such as delinquency and underachievement)?" cannot be discussed meaningfully in five minutes. Many important questions which would presumably be part of the function of a clinical case conference are far better left undiscussed than discussed in a superficial, dilettante fashion. Nothing is more offensive to a first-class intellect than to have to listen to third-raters converse about an intrinsically fascinating and complicated topic.

Of course this suggestion involves not merely suitable changes in case conference procedures, and an enforcement of constraints on the use of time in certain ways by the participating personnel. It also requires that we address more refractory problems, including the need for departments of psychology and psychiatry to recognize that there is a scholarly and intellectually exciting way to discuss complicated subjects.

Part III: Concluding Remarks

This paper is a polemic. If some of my judgments seem harsh, I remind the reader that a psychiatric case conference involves the welfare of patients and their families, that we deal with the physical or psychological pain, the "success" or "failure," the incarceration or liberty, the economic dependency, and sometimes the life or death, of human beings for whom we have accepted some measure of responsibility.

I have tried to indicate that we face some special methodological difficulties in the psychiatric and psychological fields, difficulties so complicated and recalcitrant as to present major problems even for first-class scientists and practitioners. And I have tried to offer at least the beginnings of a constructive plan for bringing the reinforcement schedule and cognitive feedback of the psychiatric case conference somewhat closer to those which prevail in the clinicopathological conference that has been so successful as a teaching device in the nonpsychiatric fields of medicine.

Addendum

As this volume was going to press, my psychiatric colleague Dr. Leonard L. Heston commented, on reading the manuscript, that I have overlooked to mention follow-up as a useful device for improving case conferences. I am at a loss to understand my omitting this important alternative, except for the fact that my mental set was so strongly oriented toward solving the problem of providing fairly quick differential reinforcement, of the kind that the internist receives when at the end of a conference the pathologist presents his quasi-criterial report on what the tissue showed. We could, for example, have the clinicians participating in a psychiatric case conference speak, for the record (we could even tape-record the conference which might in itself tend to reduce some of the garbage generated!), and compare their comments with evidence obtained through follow-up of the patient in the forthcoming days, weeks, months (sometimes even years) later. Over time this would provide useful reinforcement of clear thinking and also reveal the limits of what is possible. It would also undermine the uncritical, affiliative, "buddy-buddy" ethic that makes case conferences so unproductive (and boring!). If you go along with the kinds of analysis I have been commenting upon, you'll be wrong, and we'll see it. As they say, the prospect of being hanged concentrates the mind wonderfully. Dr. Heston's suggestion is extremely important, and my discussion of the problem would be seriously defective without mention of it.

Finally, I take this opportunity to add that since my scholarly psychiatric colleagues Drs. Leonard Heston and Neil Yorkston are now running a new weekly clinical case conference which is being inched up steadily to clinically and scientifically respectable standards, the title of this essay has become out-of-date for its author, since I am attending their conference regularly, with enjoyment and profit.

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