

SECOND-ORDER RELEVANCE

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By beginning with a couple of anecdotes, I may seem to be contradicting myself, since I shall assume that training in scientific psychology (as in law, engineering, etc.) has an appreciable tendency to help people think better, at least on some topics and in some respects. To most psychologists, “thinking better” includes discounting “anecdotal evidence.” So let me emphasize that these anecdotes are mainly illustrative of this educational assumption, and are not being offered as strong evidence for it.

First anecdote: I am chairman of a search committee to find a new dean for the University of Minnesota Law School. At the first session of that committee, I was struck—as I have been frequently in lunch table conversations at the Faculty Club—with the incisiveness, clarity, directedness, and high relevancy level of my legal brethren’s discussions, even at such a preliminary stage. For example, when, on a (happily rare) occasion, one of the law professors began wandering a bit by anticipating an improbable contingency, he was cut off gently but firmly by another’s brief remark, “We don’t reach that issue.” Those of you familiar with appellate court opinions will recognize this locution as the way courts avoid talking about something they neither need nor wish to talk about, because it turns out that something else comes first and it is dispositive. My committee member was saying, by this standard lawyer’s language, “Look, let’s not waste our committee’s time talking about something that we may not have to talk about; but if we do, we will do it then.” Walking home after that meeting, I found myself favorably contrasting lawyers with psychiatrists. I served for several years on the executive committee of our Department of Psychiatry, and I cannot recall anybody (myself included) ever stopping a bootless, fruitless, irrelevant contribution (and there were many) by pointing out that “We don’t reach that issue.” In my experience, many psychiatrists (at least American-trained ones) have a tendency to see a committee meeting, or any administrative or clinical conference, as properly having the delightful free-associative features of a psychoanalytic hour or group therapy session (Meehl, [1973]). It is not to my purpose, even if we had the evidence,

to unscramble the self-selective factors from the educational impact factors that enable lawyers to “think better,” at least in certain contexts, and in several senses, for example, sticking to the point, avoiding issues you need not reach, separating evidentiary questions from others, distinguishing substance from procedure, locating the burden of proof, asking whether a certain presumption is absolute or rebuttable, distinguishing between arguments that are merely persuasive and arguments that are dispositive, separating questions of law from those of fact, interpreting ambiguous language by reference to less ambiguous, construing a rule or setting its application limits by looking to its purpose, testing a proposed rule by concocting imagined cases (“hypos”), and the like. I suppose most psychologists would take it for granted, extrapolating from the mass of research data on vocational interest tests (Campbell, 1971) that the characteristic lawyerlike mentation and discourse derive initially from the self-selection of a certain kind of mind for this profession, followed by elimination of those who cannot master the game, and the bent is developed further in survivors partly by the content of legal subject matter, and partly from the intensive, argumentative Socratic method of American law school instruction. A law student who cannot articulate his argument and finds each of his attempted unpackings met with the professor’s reiterated “and therefore?” is forced to get clearer about “What proves what?” and “What do these words mean?” and “What seems worth mentioning but isn’t?” Of course, like all virtues this one can become corrupted to a vice, as is suggested by Thomas Reed Powell’s ambivalent definition of the legal mind as one “capable of thinking about a topic, without thinking about another topic with which the first is inextricably entwined.”

My second anecdote has slightly more evidentiary value by usual psychologists’ standards because I have counted something. Upon the appearance of what are likely among the most widely used textbooks on clinical assessment, *Research in Clinical Assessment* (Megargee, 1966) and *Problems in Human Assessment* (Jackson & Messick, 1967), I had occasion to count the number of journal contributions reprinted in those books that were by Minnesota PhDs or current Minnesota faculty. I found they amounted to one-fifth in one volume and one-sixth in the other—a proportion of papers from one school that I doubt could be matched by any other psychology department in any selected area of specialization. This led me to make an informal tally of further references to Minnesota authors in the Jackson-Messick index, which gave me 43 names of Minnesota faculty or products (counting those I happened to recognize—there were doubtless many names unknown to me). Since none of the editors was trained at Minnesota or had any special contact with our faculty, I am inclined to think that our representation in those assessment texts stems from the scholarly merit of our productions, expressing what has been called—in some places derogatively, in others praisefully—“The Minnesota point of view.” In the assessment field, this is connected with such names as Donald G. Paterson (1892-1961), one of the great contributors to applied psychology in this country, and, through an apostolic succession derived from him, E. G. Williamson, J. G. Darley, and my own doctoral adviser Starke R. Hathaway, coauthor of the Minnesota Multiphasic Personality Inventory. I have seen similar informal computations showing the major role of “Minnesota products” as “PhD producers,” “academic administrators,” and—to a degree that has at times become a bit embarrassing—in dominant roles in APA affairs. Although I myself have departed intellectually (especially in my psychoanalytic interests and my theoretical concerns) from the core Paterson-Hathaway tradition, I continue to view it as one of the really great traditions of my profession and worthy, with some evolution, of preservation. I trust it will not seem excessively parochial or narcissistic if I take it as my exemplifying instance throughout the remainder of this article.

Before continuing, I must obviate some easy misunderstandings. First, I am of course aware as a psychologist that in our profession the ancient notion of a sort of “general training of the mind” is looked upon with disfavor, although perhaps today somewhat less so than was true in the 1920s during the great debate over transfer of training. But from the fact that the study of Latin does not improve the rigor of somebody’s reasoning about politics, it does not follow that there is no such thing as training a person into certain habits of thought, or perhaps I should better say habits-cum-attitudes of thought, by combining emphases on methodological generalization *and* diversified specific substantive content, producing a kind of “disciplined mind.” I do not myself have much doubt that something like

this can happen, although, like most psychologists, I tend to assume that we do it rather inefficiently and haphazardly at all educational levels. When I took Paterson's great course in differential psychology (back in 1939), he spent most of the class time critically analyzing studies of individual differences on a wide variety of substantive topics, ranging from whether redheaded people have bad tempers to the origin of the observed ethnic IQ differences and the usefulness of multiple factor analysis for improving practical psychometries (Paterson held, rather little). I find it hard to conceive of any capable student listening attentively to those 60-odd hours of Donald G. Paterson taking research studies apart bit by bit and bone by bone, and ever being quite the same afterward. Even the few constitutional and dedicated "bubble heads" I knew (bubble heads didn't tend to take graduate work at Minnesota, at least in my day) were unable to emerge unscathed by this educational experience. To express a personalistic probability, I would bet, at 10:1 odds, \$1,000 of my own hard-earned money that hardly any Minnesota student who went through that course—which everybody, regardless of his PhD specialty, was expected (and eager) to take in those days—could commit certain kinds of errors in experimental design or data interpretation, no matter how strongly his substantive bias might tend to seduce him. To this day, I continue to come across articles by psychologists containing methodological mistakes of the sort that I was cured of by Donald G. Paterson in the beginning of my junior year (see, e.g., Meehl, 1969, 1970a, 1971a, 1972; Meehl & Rosen, 1955). I hasten to add that this Patersonian skepticism and criticality also had its adverse effect—which I hope is not a necessary correlate but which I readily confess tends empirically to be associated—namely, a kind of theoretical nihilism and a tendency on the part of students to think that among the most horrible sins you could possibly commit would be not to have a perfectly representative sample, or to underestimate the influence of selective migration or assortative mating on a correlation coefficient. (A Minnesota MA from the late 1930s came back from Harvard labeling us as "dust-bowl empiricists," which slap he felt we took rather too good-naturedly.) William James once poked fun at William Kingdon Clifford's *Ethics of Belief*, in which the latter had said, "It is wrong always, everywhere and for anyone to believe anything whatsoever upon insufficient evidence," by commenting that apparently in Clifford's morality the worst thing a man could possibly do would be to make a mistake by believing something false. Between the statisticians' two kinds of errors, I think that the typical "Paterson product" of the 1930s and 1940s was phobic about errors of the first kind, and insufficiently willing to speculate, extrapolate, and "let his hair down" to permit a little bit more of the creative process to go on. But as my political scientist colleague Herbert McClosky is fond of reminding me, "Everything has its price tag."

The two opposite errors to which psychologists, especially clinical psychologists, are tempted are the simpleminded and the muddleheaded (as Whitehead and Russell labeled each other in a famous dinner exchange). The simpleminded, due to their hypercriticality and superscientism and their acceptance of a variant of operationalist philosophy of science (that hardly any historian or logician of science has defended unqualifiedly for at least 30 years), tend to have a difficult time discovering anything interesting or exciting about the mind. The muddleheads, per contra, have a tendency to discover a lot of interesting things that are not so. I have never been able, despite my Minnesota "simpleminded" training, to decide between these two evils. At times it has seemed to me that the best solution is sort of like the political one, namely, we wait for clever muddleheads to cook up interesting possibilities and the task of the simpleminded contingent is then to sift the wheat from the chaff. But I do not really believe this, partly because I have become increasingly convinced that you cannot do the right kind of research on an interesting theoretical position if you are too simpleminded to enter into its frame of reference fully (see, e.g., Meehl, 1970b). One hardly knows how to choose between these two methodological sins. One thing I can say in favor of the simpleminded is that I have seen several cases of it get cured, by personal experience of psychoanalysis or by exposure to sufficiently bright, rational, and articulate intellects of opposite persuasion, or by just getting older, securer, and more "relaxed." Simplemindedness is (not being correlated with stupidity among academicians) a curable condition. But I have, alas, never seen a

muddlehead get well. I am inclined to believe that this condition has a hopeless prognosis.

Second, I would not want to identify intellectual incisiveness with hypercriticality, as if the essential feature of the scientific mind as applied to a subject matter like psychology were to be “the spirit that denies.” (Aristotle says early in the *Nicomachean Ethics* that the educated mind will insist on precision insofar as the nature of the subject matter permits, and who am I to quarrel with Aristotle?) I had a bit too much skepticism drilled into me in the course of my Minnesota education, and had slowly and painfully (on the couch and in my own practice as a psychotherapist) to free myself of it. There are certain tensions within our profession, arising mainly from the expansion of clinical psychology, that have tended to polarize “being scientific” against being, shall I say, “imaginative” or “open to new speculative possibilities.” But I confidently anticipate that the next generation of young psychologists will have sufficient exposure to the writings of contemporary historians and logicians of science such as Popper, Feyerabend, Lakatos, & Co. (cf. Lakatos & Musgrave, 1970, and references cited therein) so they will not suffer from this needless polarization. I could exemplify it in a nutshell: If as a psychologist you think it somehow incoherent to be intellectually respectful, yet skeptical, yet emotionally turned on by both factor analysis and psychoanalysis, there is probably something rigid about your cognitive structure.

Third, I would not want to give the appearance of thinking (which I definitely do not think) that long-term educational impacts on “mental habits-cum-attitudes,” such as “officially” intended by training in engineering or philosophy or psychology or law, are not empirically researchable or that we should go blindly ahead in reliance on anecdotal or inadequate statistical data. It is almost a cliché today that one oddity of higher educational decision making in the behavior sciences is the low interest shown in applying behavior science methods to research our own educational outcomes or the efficacy of competing training techniques. On the other hand, it is important to recognize that education presents a pragmatic decision context, like law, politics, applied economics, medicine, or psychotherapy, and hence it is (intellectually and morally) licit to make practical decisions, including decisions involving large amounts of time, energy, and money, pending adequate research to answer the big educational questions by usual “scientific standards.” To draw an analogy: I might, as a clinician, prefer to have better validation for the MMPI code type available to me in diagnosing the instant case; I might even have a sincere intention to research it myself over the next few years, but the patient sits here today and his problem cannot be “put on ice” in the meantime. It is in that kind of context that an inadequately documented second-order relevance is defensible as an aim of doctoral education in psychology.

Fourth, I am aware that some of our more “bodacious” undergraduates need instruction that the word “relevant” is not a one-place predicate (like “green” or “copper”) but is a *relation* word. Personally, I do not engage in serious discussion with students who object to someone’s studying the composition of the stars or the question as to whether the raccoon can count because it is not “relevant,” unless they show at least some minimum intellectual clarity about the concept of relevance itself, and a recognition of the elementary point that a given subject matter varies in its relevance to a number of different human purposes, short term and long term, and with empirical assurance of its relevance varying from possible to probable to damned near certain. On those ground rules, I do not countenance anybody’s dogmatic definition of “relevance” as “tending immediately and strongly to put a stop to the Vietnam war, to integrate the races, or to eliminate pollution.” Like most of our students, I consider the Vietnam war to be one of the wickedest and stupidest adventures our political leaders have ever engaged us in. But I think it absurd to make its liquidation the touchstone of whether a university course, curriculum, or academic department should be considered respectable on “relevance” grounds. Further, I am not impressed favorably with students (or faculty) who, on the one hand, make a major issue (as I do) of freedom, autonomy, openness, antiauthoritarianism, and the like, and then proceed to inform me—frequently with a strident note of dogmatic and self-righteous moralism—about the nature of my moral obligations, though it turns out on inquiry that they have never read Immanuel Kant, never heard of Grotius, and could not provide me with a sophomore-level exposition of the difference between rule-utilitarian and act-utilitarian ethics. I am puzzled by a person who reads me the moral

riot act without even bothering to inform himself at an undergraduate level about the great intellectual traditions of ethical and political theory. I hold that people who are passionate about their right to “do their thing” should be willing to let me do mine, unless they can make a reasoned case against me.

Finally, my defense of second-order relevance should not be construed as an opposition to the training of psychological or social practitioners for first-order relevance “at the firing line.” I am in print strongly to the contrary (see Meehl, 1971b), and the main reason a practitioner Doctor of Psychology degree is not being offered at Minnesota is that the graduate dean and I have been unsuccessful in persuading my colleagues of its merits. Nor am I one of those academicians who considers it sort of a natural right of the intellectual to pursue his interests in the ivory tower at the taxpayer’s expense (“ivory tower” is no dirty word for me—I love it there, and plan to stay there). My views about higher education are somewhat radical and include the notion that the immediate, highest probability, longest term, and largest amount beneficiary of higher education is the student who receives it. From which I conclude, absent persuasive counterarguments, that support of training to the MD or PhD or similar degree by the taxpayer is a form of regressive taxation, is not really in the “equalitarian tradition,” and that some other system would probably be fairer, as well as more effective from the student-motivation aspect. (Correlated with this view is an openness to ideas about faculty salaries and department sizes being more directly tied to student demand and satisfaction, although these notions admittedly involve dangerous side-effect and unforeseeable counterproductive possibilities.)

In specific, consider an institution like the University of Minnesota. It turns out that on cost accounting its clearly *instructional* functions, the entire faculty teaching payroll could be met without a dime from the legislature—who would then have to provide only bricks, mortar, and civil service personnel—if we were to *double* the student fees. (Don’t shriek “Horrors!” Wait a moment.) As has been suggested by a number of economists, we could make doubling student fees socially feasible by creating a large national student loan bank. In case anyone argues that this would scare away students from disadvantaged classes or ethnic groups, we can figure it out actuarially so that the student does not have to pay back the loan *unless and until* he starts making professional money, at which time he pays a flat percentage rate on his income for *X* years. Considering the financial killing that many professionals make (and even college professors aren’t doing too badly these days), the loan bank would before long have money coming out of its ears.

After what may seem an excessively long and discursive, but I trust relevant, “introduction,” I turn now to second-order relevance as I conceive it. In the present context, I take the term “relevance” as short for “applied relevance,” meaning by the latter (roughly, but good enough for our purposes) *bearing rather directly on a problem of applied psychology*. An ambiguity of the generic term “applied psychology” arises when a psychology department gets to questioning what sorts of jobs its PhD products will be doing after they get out. The phrase applied psychology can be taken to designate either a *class of problems* or a *working context* (i.e., how, where, paid by whom, doing what specific tasks the applied psychologist spends his time). I use the phrase applied psychology to designate the problem class. The problem class is defined by its member problems being of immediate, practical social importance, in the sense that a hard-nosed legislator would say,

This is a bad problem: we have to try to solve it or at least ameliorate it: we are spending the taxpayer’s money on this problem because people or social institutions are “hurting” somehow—whether in disease, or money, or social friction, or wasted talents, or psychological distress, or whatever—and something needs to be done.

Such a hard-nosed legislator might feel pretty reluctant to subsidize a flock of college professors whose main concern is to reconstruct the development of the Etruscan religion, or to analyze the philosophical mind–body problem, or to settle the Chomsky–Skinner controversy.

The other meaning of applied psychology—designating the work situation—is, unless I am mistaken, always subsumable under the preceding meaning. We can divide applied psychology (= a generic term) into two subgroups. “First-level applied psychology” means working on a practical social problem “on the firing line.” That cliché expression designates the first-level practitioner as one who deals with the concrete educational, social, medical,

psychological, economic, or technological problem presented by an individual help-seeking “client” (whether patient, student, business firm, government agency, or whatever). The practitioner is attempting to ameliorate *that client’s problem now*. I would not require in all cases that he be dealing face to face with the person (or the individual representative of an institution) who is “hurting.” This would be an inconvenient basis to draw the semantic line. I would say that when I consulted at the Veterans Administration Mental Hygiene Clinic for 10 years supervising trainees in psychotherapy, I was doing first-level applied psychology even though I was not myself talking with the patient. As long as I am dealing with a concrete problem presented by the trainee, who asks me not “What do you think about Freud’s theory of libido, in general?” but “What’s the matter with this guy?” or “What should I say when the patient makes this kind of remark?” I am doing first-level applied psychology. The individual patient’s problem is the focus of what I am directly helping the trainee to do. And, of course, on the client’s side, we find a correlative expectation of, and desire for, help. He may, to be sure, realize that some “file research” or “clinical trials” are going on, with persons like himself as subjects. He may even be altruistic about serving as a “guinea pig.” But what he mainly comes in for—and is, given the social context, entitled to demand—is help for himself.

“Second-level applied psychology” does not intend to deal directly with the help-seeking consumer’s individual or group problem. It works on the *applied problem class*, by research or teaching. A clear showing of the difference is that if a Veterans Administration patient suicides, and as a result of his suicide the trainee and I cook up a new psychometric theory and technique of assessing internalized aggression based on the case material, we have failed in our job as first-level applied psychologists, but we have succeeded in our job as second-level applied psychologists (since assessing suicide risk is an applied psychology problem). A clinical psychologist who gets a PhD from Minnesota’s Psychology Department and then hangs out a shingle in full-time private practice of psychotherapy or takes a job as director of a community mental health center is engaged in first-level applied psychology. But a clinical psychologist who, after getting his PhD from our Department, becomes a professor of psychology teaching personality theory and psychometrics and psychotherapy and behavior modification, while an equally clear case of applied psychology in the generic sense, is engaged in second-level applied psychology. (Of course, one and the same person can shift roles, as I do myself.)

I see no a priori reason why a department cannot train both kinds of applied psychologists. But the Minnesota faculty is increasingly convinced that for some reason (not entirely clear) we do not do both effectively. I think we have one of the best clinical psychology training programs in the country, but I am not very happy with it. It seems to me that in our efforts to “do both” with a large number of students, we are turning out PhDs who fall between the two stools. (Sheer size is arguably a major element of this problem; see, e.g., Gallant & Prothero, 1972.) They are not, by and large, first-class intellects of the sort I would expect either to scintillate in the classroom or advance the science by their contributions to the research literature. But neither are they, by and large, professionals to whom I would feel comfortable sending a mentally aberrated friend for diagnosis or treatment. It distresses me, for example, when after four years of post-BA training to be a clinical psychologist, a candidate cannot (on a preliminary oral) list for me the major symptoms of a psychotic depression, or is unaware that the suicide risk with this diagnosis is roughly 3,500% what it is for people in general, and many times higher than for other psychiatric diagnoses (being competed with only by schizophrenia and alcoholism). It seems to me that a PhD in clinical psychology who cannot handle this kind of clinical question is simply incompetent to perform his professional task. Yet when I shift gears hopefully and ask this clinically marginal candidate to discuss the mathematics of factor analysis, or the methodological problems of construct validity, or the conceptual nature of a “personality trait,” or the taxonomic problem of loose syndromes in behavior genetics, it turns out that he cannot do an intellectually respectable—not to say halfway exciting—job on that more “academic” kind of question either. It almost seems as though such students were (as they complain) spending too much time on the books to learn enough about patients, yet too much time with patients to become scientists or scholars. No doubt this is partly because we, the faculty, waste

some of their time teaching them a bunch of “garbage” that is part of the clinical armamentarium but should be dispensed with—because we do not have sufficient courage to turn out a clinical psychologist who says, “No, I don’t claim any expertise with the Icelandic Tennis Ball Projective Test, because the research evidence indicates that it has negligible validity.”

It seems to me that with limited resources and with one of our major morale problems among students being the excessively large number of them (which they say makes them feel as if they were still undergraduates), it is rational for a psychology department to ask whether it can do a better job of *one* of these two kinds of training than most other places can. That seems to me just good utilitarian reasoning, not to say ethical reasoning in some stronger sense. I am reminded of Skinner’s famous crack about Albert Schweitzer, that “He wanted to help mankind—one at a time.” I daresay there are many more would-be psychologists who are reasonably good at listening sympathetically to disturbed patients than there are applicants who are as bright, informed, clearheaded, and passionate about ideas as my psychology department colleagues, these latter being also clinically sophisticated from plenty of “dirty-hands, real-life” clinical practice. From my six years as a member of the American Board of Professional Psychology, I am morally certain that too many clinical psychologists are being turned out who have never been taught to think scientifically about much of anything or even to value clear thinking as a highly desired personal or professional goal. So I have concluded, until further notice—postulating that the “old Minnesota tradition” is worth preserving, especially in a professional field that seems to be becoming increasingly “touchy-feely” while showing a decline in respect for the work of the intellect—that if we have a special kind of talent and attitude in our faculty (a “scarce resource”), we should concentrate on using it to the fullest, not sacrificing educational resources to a task that lots of other places can do as well or better. Let schools *X* and *Y* grind out the tea-leaf artists and layers-on-of-hands, because we know how to do something (a) harder, (b) rarer, but (c) more valuable in the long run.

The job description for a second-level applied psychologist would be that he spends most of his time either (a) teaching (in the classroom and seminar sense, not in the clinical supervision sense) students, who themselves may become either first-level or second-level applied psychologists. But a “second-level applied training program” is one aiming to *produce* the second-level applied psychologist; or (b) conducting, supervising, or evaluating (as on an NIMH panel) research that has a direct (or confidently foreseeable indirect) relevance to a practical problem. The subjects of such research may be clients or patients or students or various social groups with a problem; it may even be that in the conducting of his research the psychologist has face-to-face contact with these subjects; nevertheless, the defining goal of his activity is not primarily to help these particular persons or groups—although he may help them inadvertently, he may even agree to try to help them as part of the “package deal” for using them as guinea pigs—but rather to find out something about a relationship. As for public relations, I divide my public into those whose opinions I almost have to listen to—whether or not I find them rationally persuasive (to wit, the State Legislature)—and those to whom I listen only if I find their credentials adequate and their reasoning convincing. (This attitude admittedly requires a certain amount of inner-directedness that I gather is somewhat on the decline in academia; but we all have to act according to our lights.) I do not think that the average state legislator is so completely “immediate-practical impact” oriented that he is unwilling to help support a kind of graduate training chiefly aimed at figuring out the scientific facts and theories *bearing on* a practical problem, rather than concentrating on turning out hewers of wood and drawers of water. I think the behavior of our Minnesota legislature provides some empirical support for this view of the legislator’s value system. I have for 17 years spent much of my time in a University division called the Psychiatric Research Unit, which has always been given adequate legislative support. (During one biennium it was the only administrative unit in the entire University, including the often favored agricultural campus, for which the legislature appropriated the full amount of funds we had requested, raising the appropriation for psychiatric research back to our request from where the Board of Regents and the Governor had lowered it.) Informed members of the legislative committee know that in our Psychiatric Research Unit, studies are being conducted on topics

ranging from the brain waves of monozygotic twins, through the Skinner-box schedules of addictive drugs in chimpanzees, to development of new taxonomic search methods for loose genetic syndromes in mental disorder, and even “armchair analyses” of philosophical problems arising at the interface between clinical psychology and the criminal law. I could emphasize to any legislator, with force, sincerity, and logic, that the most important thing to do about schizophrenia (or poverty, or delinquency, or suicide, or divorce, or alcoholism) is to get a more adequate causal-theoretical understanding of these phenomena—in the same way that the most important thing to do at this time about cancer and multiple sclerosis is to find out “how they work.” I am convinced that the average legislator is not too shortsighted to understand this position, assuming it is presented cogently by someone who has thought it through, who has had practical clinical experience on the firing line (a purely “academic” knowledge of mental disorder will not do) and who can come on strong because of his own personal conviction and be articulate in presenting the case.

I do not know exactly how to classify the administrator-psychologist who neither sees patients nor supervises the clinical personnel who are seeing patients but who nevertheless determines, or at least strongly influences, the policy and ideology of a clinical facility by a combination of hiring practices and his educative impact whether formal (e.g., in-service training as in lectures by associated university faculty) or informal (e.g., coffee break). But for defense of a psychology department’s concentration on second-order relevance, how you classify such an individual does not matter too much. I am rather inclined to consider him in the first-order applied group, even though my above definition of working context does not put him clearly “on the firing line.” In any event, a department oriented toward second-level relevance as its major or sole educational commitment need not apologize if some subset of its PhD product ends up in such positions. The state of Minnesota could save hundreds of thousands of dollars of taxpayer money and greatly improve patient care by such administrators fostering adoption of clinic policies that are, I urge, easily defensible in the present state of the evidence. Elimination of useless clinical data collection by complex, time-consuming assessment devices of low validity and which purport (validly or not) to assess dimensions of questionable bearing on what happens or what is done to the patient anyway, is a favorite form of clinical psychologists’ boondoggling, taxpayer-supported at both local and national levels. Or, again, a psychologist who has been trained scientifically to respect evidence rather than ideology will know that there are some (genetically determined) psychotic depressions that are “endogenous,” not known to be significantly influenceable by psychological or social intervention, and for whom the treatment of choice is (first, because conservative) one of the antidepressant drugs, and failing that, electroshock therapy as almost the sole “specific therapy” in psychiatry. (There are thousands of clinical psychologists who will not accept these facts because they have been trained improperly and have acquired mental habits that do not enable them to reeducate themselves.) Again, I know a woman whose way of handling her semipsychotic spouse has been influenced permanently for the worse (not to mention her own guilt feelings) by some “fuzz-brained family therapist” who managed to convince her that *she* was her husband’s trouble, despite the fact that the husband was considered “very peculiar” before he met her, and his family tree included an impressive number of obviously aberrated persons, including a couple of suicides and state hospital patients diagnosed “dementia praecox.” (This illustrates my thesis that it is very dangerous in the United States to be the relative of a mental patient because you will almost certainly get blamed for it.) There are all sorts of costly clinical activities going on which are at best inefficacious and at worst countertherapeutic, which maintain themselves in a helping profession that has insufficient scientific rigor and which, unfortunately, functions “in the team” with two other professions (psychiatry and social work) that have even less. Arguing along these lines, I think I would not have much trouble convincing an intelligent member of the legislature that training more Minnesota PhDs for second-level applied psychology is a socially defensible enterprise.

In conclusion, while tritely urging “more and better research” on our doctoral educational outcomes, including the subsidized evaluation of innovative training methods, and while retaining my long-standing belief in the desirability of a fully “professional practitioner”

doctorate in clinical psychology, I nevertheless argue that it is socially defensible in the meantime for a first-class psychology department to adopt explicitly a policy of selection, training, and job placement oriented wholly to second-order relevance. I would insist, of course, that the PhD products of such a program receive sufficient clinical experience to develop a nonbookish “feel for what real patients are like.” But the primary aim of their PhD learning experience would be to produce scientific clinical psychologists who can think more clearly and research better in the broadly defined domain of “applied psychology problems” than seems typically the case. Such PhDs should have mastered thoroughly the basic behavioral substantive sciences (learning theory, child development, behavior genetics, neurophysiology, differential psychology, psychodynamics, social psychology) along with the investigative tools (statistics, experimental design—with a suitable skepticism about the received Fisherian tradition—psychometrics, and perhaps a dash of up-to-date philosophy of science) and, hardest to characterize and inculcate but as important as the substantive and “tool” knowledge and skills, those “attitudes” that—if my largely (but not wholly) anecdotal impressions are correct—tend to distinguish most engineers, lawyers, and scientifically trained psychologists from educated persons generally and from those “woolly” clinicians who never learned to discriminate between their acts of faith (perfectly all right in themselves) and such beliefs-cum-techniques as bring respectable evidentiary credentials with them.

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