

THE QUESTION OF SCHOLARSHIP IN SPECIAL EDUCATION

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The Context of Knowledge and Authority

How do we know what we know in Special Education? We speak and write about theories as if we have them, about hypotheses as if we understand them, about methods as if we agree about them, and about the limits of our knowledge as if we not only know lots of things but even know what we have yet to learn. Nevertheless, despite our journals, scientific societies, and universities--and despite our facade of knowledge and the trappings of wisdom--there's much in our field to commend the assertion that all isn't well with knowledge generation in Special Education. There's even evidence to fuel the assertion that we're barely a field, much less a scholarly discipline. For example, the student picks up the textbook or journal on learning disabilities and reads in one paragraph that possibly two or three percent of the school-age population has learning disabilities. And then, he picks up another textbook or journal on learning disabilities and reads that forty or fifty percent of the school children have learning disabilities. Once in a while, he'll find in the very same journal the two percent claim on the heels of the other, and hardly ever is there a proper explanation to the puzzled student. Is learning disabilities a rare condition? Is it so common that the exceptionality is not an exception? Could it be that it's both rare and common, rare in one community and common in another? Or could it be that its rarity is dependent on how it's defined, or who does the counting, or the purpose for which the counting is done? If those considerations aren't ventilated, the student's puzzlement remains unresolved. More often than not, the student concludes he's simply stupid, because the only other conclusion is that the field and its leaders are lunatic. There appears to be insufficient careful analysis in our

field, possibly one of the greatest "culprits" being my own ^{interest} field, mental retardation. Students are usually given too many books to read, and so they become superficial readers. Professors have too many journals to keep track of, and so they become superficial scholars. Authors are burdened with the idea that if they don't publish they aren't scholars, and so we have too many superficial writers and thinkers. Quite simply, people don't find the time these days to analyze carefully the papers they read and to construct carefully the papers they write. Consequently, there's a democratization in our field which, by its very egalitarianism, overwhelms rather than refreshes us. There is simply too much written that's wrong, repetitious, or trivial. And the rest of us have neither the time nor the interest in ferreting out the good from the bad. It can be argued that the field of Special Education has more books than ideas published, that there is everything except thoughtful analysis and reflection. This is all by way of saying that the field of Special Education is more action than wisdom, and what we need now is more wisdom. Not everything that can be known is equally worth knowing. Not everything that's known is useful. And it must also be remembered that not everything that can be used is worth using. Or to paraphrase Whitehead's 1929 classic, The Aims of Education, education is nothing if it is not useful. There are too many inert ideas being taught, too much dead knowledge proudly paraded, too many useless facts memorized (Whitehead, 1964).

Given the state of our field, it may be good to take stock of our knowledge base in Special Education and how we achieved it. That is, how much is there buried in our belief systems which is "true" and, furthermore, that we know is "true"? And from everything that's left, what is there among the debris which represents our values, and how concordant are those values with the shared values of society? From such an examination of epistemology, knowledge, and values, we may be able to lay out eventually a guide for scholars in our applied fields.

I am a teacher. I prepare teachers and other clinicians for work with disabled people, and I try to prepare scholars to describe and understand the disabled. Through the years, my research activities have involved me in most if not all of the quantitative and qualitative methodologies commonly used by social scientists. I am made to feel cheated (or guilty) if I don't regularly keep up with the archival journals in my area of specialization--mental retardation. Indeed, I think I make my own doctoral students and colleagues feel that way too. However, to tell the truth, I have been dissatisfied with the archival journals, many of the scholarly books, and almost all of the textbooks in Special Education. I also suspect that more than a few of my colleagues share my dissatisfactions.

Reason, Values and Prejudices

Of course, if I didn't have a problem with Science--better, with scientists--in the social sciences I wouldn't have the need to write a paper of this sort. But I think many people in this work have the same problem. Why? We look to Science to clarify our uncertainties, to discover truths, to invent solutions, to settle disagreements. In the hard and social sciences, controversy is supposed to be settled by reasonable professionals, applying their tools of science and scholarship to problems which are amenable to finite resolution. If it only could be so. But it isn't so in the social sciences. Science is nothing if it doesn't proceed from the application of satisfactory argument, buttressed by facts and other data. However, there are different ways to build a professional field, and Science is but one of these. People reason, and people express "values." And of course, people also have prejudices, selfish interests, and personal and professional imperfections (Gould, 1981). Hence, controversy arises, even in fields which are guided if not driven by Science. While Science itself may be theoretically immune to controversy, scientists aren't. While Science is built on facts and logical discourse, scientists are nurtured by

whatever it is that nurtures all people.

Even Nobel laureates will from time to time admit that there are "personal accounts" in the making of a great discovery, accounts which do not always enhance the perception of Science as pristine arguments, or the execution of Science as objective quest for the truth (Watson, 1968). Not only is there controversy, and not only are there personal agendas among the scientists, but from time to time there is fraud--even in our greatest universities and among our distinguished scholars (Broad, 1980a, 1980b, Hunt, 1981). While the laboratory itself may be pure, the scientist sometimes isn't. While the research protocol may be value-free, the scientist rarely is--especially in the social sciences, especially in our work.

Knowledge

Again, the question is asked: How do we know what we know? How do we know what to believe? A student reads in a textbook on exceptional children that one, two, three or seven percent of the school-age population have learning disabilities (Gardner, 1977, pp. 31-32). But another book on the problem claims that up to 40 percent of the school children have learning disabilities (Brutten, Richardson, & Mangel, 1973, p. 10).

Where is the analysis? Where is the judgement? Oftentimes, it is not present, and our work has suffered for it. Everyone's opinion is equal because everyone is equal. Everyone's scholarship is equal because too few of us are willing or able to make scholarly judgements. We have forgotten that it does make a difference when Seymour Sarason or Samuel Kirk says he doesn't understand a particular child he has examined as compared to the casual "examination" ^{by} of the next door neighbor who also doesn't understand that child. Judgement once ruled scholarship. Ironically, today, "scholarship" overrules judgement. So how do we know what we know? That's a question which probably is infrequently asked by the person

"doing" scientific work, even though it's a preoccupation of the philosopher of Science. Notwithstanding, it's a question which deserves to be asked by everyone connected with the business of solving puzzles. Unfortunately, many people in our field make out as if we're too busy to worry about "epistemological games." Some of us are so busy that we never took the time to genuinely learn what epistemology is. A few of us could hardly offer a nickel definition of the word. I've been driving at a definition since the beginning of this paper, and for me it comes down to: the bases of knowledge, where it comes from, what it means, what its limits are--i.e., How do we know what we know? I don't want to get into a deep discussion of epistemology as one of the three fields of inquiry, and I don't want to get into other deep technical questions of philosophy. But I do want to discuss the more practical epistemological questions which may have bearing on our work in Special Education.¹ Unfortunately, philosophers of Science appear to be almost the only ones who worry about those kinds of questions. People who are actually doing research (especially those in applied areas) seem sometimes to act almost mindlessly because they have a lot to do. Possibly, because nobody seems to listen to them very much, and because they're not always doing laboratory or field research themselves, philosophers of Science (who are mindful about how scholars unravel the truth) rarely have the opportunities to directly participate in the unraveling. It almost seems that the scientist behaves as if thinking doesn't always matter, and that ^{he} one needn't be thoughtful about what he's doing with the observations he's gathering. All of this is by way of saying that working scientists should develop greater respect for thinking.

For convenience, we may categorize epistemology in three ways: descriptive, analytical, and normative. Descriptively, people come to know about things in the process of explaining their similarities and differences: they take "pictures" of those things and figure out what the "pictures" portray. Secondly, analytical

¹I have been impressed with the works of Thomas Kuhn, especially his seminal book, *The Structure of Scientific Revolution*, (1970). Much of what follows may reflect this as well as the considerable influences of Donald Campbell, Seymour Sarason, and Frank Garfunkel.

epistemology is the work of the skeptic. Can we ever know anything? And if we can, what can we know? Analytical epistemology strives to unravel "knowledge"--which according to one philosophical formulation, is "true justified belief." For example, how can S know that P is true? One necessary condition for S to know that P is true is for P to be, in actuality, true. But that's not enough. S must also believe that P is true. Still, that's not enough! S must also be justified in believing that P is true. It could be that S believes that not only is P true, but Q is true, and R is true, and anything that's told to him is true. Consequently, even though P may be true, and even though S believes it, his belief isn't warranted. It's no more than belief; that is, S doesn't "know" that P is true. But for someone to know something, true belief isn't sufficient. It must be justified. Of course, something can be true even if no one believes it, or if no one has justification for believing it. But that's another matter.

Thirdly, analytical epistemology slides into normative epistemology. There is a continuity between what goes on in scientific and ordinary evaluations. You know someone is telling the truth when he says, "Here's food." The remark meets all of the necessary conditions for a true justified belief. But what about the comment, "Nothing good for the mentally retarded occurs in segregated settings." Does that statement by itself meet criteria for a true justified belief? Remember, it isn't enough to really believe that the statement is true. One must be entitled to that belief; that is, one must have analyzed the belief and found it not wanting. Some people have commented that there is too much proclaiming in our field and not enough knowing. Maybe that's why people get on and off bandwagons.

There are rules which can be applied to aid us in systematizing our judgments:

- (a) A belief in a proposition is strengthened when it constitutes a good explanation of other propositions you already believe.

For example, while the assertion that food is on the table passes all normative conditions, claiming that the food is

better than the Queen of England's would fail.

- (b) One rule is better than another rule if, by following it, you will be led to more true justified beliefs and you would avoid more errors. If you have no rules, then you're as likely to make errors as gain knowledge, since by chance either condition can occur. From experience, you learn that some rules lead to knowledge more surely than other rules.
- (c) There are certain questions that have only one correct answer, while other questions can lead to several correct answers. How many people there are in a room has but one correct answer; but what causes learning disabilities can have many answers, or there may not even be one good answer available now. We sometimes "solve" problems in special education as if all questions have good answers, as if all questions are equally important, and as if all questions require scientific methodologies to determine answers.
- (d) In the real world of scientific and scholarly activity, there are few rules to go on. For better or worse (and more than by rules), the worker is left to rely upon his expert judgment. The good news is that we have opportunities to be self-critical about our scholarship. But the bad news is that there aren't many rules in the sense that there are algorithms available for us to get correct answers. Rather, epistemology has a limited but important usefulness in promoting thoughtful research.

The process of generating knowledge is almost always dependent on what's already known. Consequently, at any one time we know most of what needs to be

known by us. In that sense, Science is almost always fine-tuning knowledge. Of course, every now and then there is an astonishing discovery. Every now and then, it isn't merely an old road that's been paved, but a lost road that's been found or a new road that's been created. Every now and then, somebody discovers that the emperor is naked. Or every now and then, somebody invents something to replace the emperor. For a hundred years, people in Western society believed in institutions for the mentally retarded. Knowledge generation in the field was once aimed at describing, understanding, improving, and fine-tuning the institution. Of course, we rediscovered another road (and we even invented a few by-passes). Consequently, the knowledge generation today is aimed at finding suitable alternatives to the institution. But it isn't easy--even to fine-tune, much less to make a genuine discovery.

Most scholars fight over their "patron saints," so arguments in our field are often less concerned with what happens to mentally retarded people and more concerned with whether one follows, for example, Wolf Wolfensberger or Edward Zigler. Knowledge generation may be the business of individual scholars, but it is also socially determined. In every sense, it's a group venture--from conduct of the research to its dissemination and use. And furthermore, there always are antecedent realities. It isn't that everything is up for grabs or, conversely, that you can't change anything without in some minute way changing everything. It's more like what occurs in a good clinic: there is knowledge, wisdom, tradition, and luck; there are facts and their application to a problem; there are data and their reduction; and there is analysis and its interpretation. In the end, there are people who examine, interpret, and offer a judgment. In the end, good research, like good clinical practice, depends on who's doing it and why. There is always the "who" factor, but the neglected "why" question is of as compelling significance.

Science

What's the purpose of Science? To discover the most plausible stories about the world. All knowledge--true justified belief--is grounded in observation. All scientific stories result from the reduction of findings (data) obtained from observations. However, no observation is neutral, without bias, as no language exists without metaphor. Furthermore, there is always a discrepancy between a true observation and the recorded observation; and another discrepancy occurs when the recorded observation is reduced to manageable data; and further distortion occurs when those data are interpreted and discussed, and discussed. So what is Science?

Science is about the world. Probably, it is never all true and hardly ever divorced from other links to reality. There are many scientific methods, probably none which is precisely correct, but assuredly some which are better than others. Philosophers may not like what I'm saying, and neither will some sociologists. On one hand, some philosophers don't like to hear about the truth; and, on the other hand, some sociologists want to claim that everything is relative, that truth is in the eyes of the beholder and the beholden. Notwithstanding, Science can get at approximate truth; and Science does have methods which aid in that journey.

For convenience, permit me to claim that there are two kinds of Science: pure and applied. It may not be altogether naive to suggest that pure Science teaches us what it takes to discover facts. To further differentiate, we might also suppose that while the physical and biological sciences get at the truth about the world, the social sciences get at what the world believes. Whether belief is as "true" as "facts" is an argument which we should better leave to the philosophers and sociologists.

Why do people engage in scientific work? Why do people want to be known

as scientists? Over a long time, "scientific" came to mean a particular devotion to circumspection, attention to evidence, consideration of alternative explanations--in general, thoughtfulness in putting forth new ways to make sense (or better sense) of the world. In this view, "methodology" is inescapably a threat to the scientific spirit. ^{I'm} We're not saying that methods are bad, much less that "scientific methods" are bad. Rather, the point is that any method is potentially a way of excluding some understanding even as it makes some other understanding possible. This is even more true of specific theories, theory-systems or cosmologies. If we think of the great scientists and scientific breakthroughs, it should be clear that what is common to them is not the adoption of a superior method or theory; those methods and theories are always superseded by other ones. What is common to them is the abandonment of orthodoxy when more sense could be made another way. Great scientists have overriding curiosities. They know that the world is multi-lingual; when we think we have learned her language, at last, she will confound us by speaking in a new one. ✓

Today, to be scientific may have come to mean the adoption of a precise orthodoxy. Once again, I'm not saying that a particular orthodoxy is bad. The methodologies, theories, and language which define and are defined as Science have been powerful, illuminating and rewarding. I not only concede but affirm that the fruits of such ways of approaching the world will continue to be impressive. But there are other ways to understand the world. What is bad is the extent to which an orthodoxy prescribes the one and only way of understanding. Then it becomes a machine which puts us (or our minds) to sleep and prevents us from recognizing anything that is not its product. To a distressingly large extent, this is what happened in a vast part of the territory of Science. In the "behavioral sciences" and most of all in "education," we have established a thoughtless orthodoxy as the model for scientific inquiry. What is defined

as science is the mechanical insertion of "data" into an elaborate answer-machine which, with the advent of computers, has ceased to be even metaphorical. It has become almost inconceivable that any activity which does not involve the machinery could be research or scientific.

It cannot go without saying (it is not obvious) that a fair amount of what has been generated by the social science's rote behavior is worthless or meaningless. It is not obvious because the activity is so common, so orthodox, so universally regarded as the True Path. Too often today, the test of an investigation's worth is simply to judge whether it was carried out in accord with the methodological dogma--not whether the conclusion makes sense. In too many cases: the conclusion is nonsense or trivial; the question itself is often incoherent; and all too often, even when understandable, the "sensible person" would recognize that the question could neither be properly asked nor answered in the terms (language) of the ruling dogma.

So, if what I'm claiming is correct, why would anyone want to participate in this depressing game; why would anyone want to be known as a "scientist," in this sense? The answer can only be that it is the ruling dogma. In this rigid orthodoxy, status and approval can be achieved only by adopting the required rituals. And, the attractiveness of it is sustained by the (by now vestigial) connotations of the idea of Science--connotations of discovery and daring inquiry, of wresting structure and meaning from the complexity of the world. In the end, people do Science. And people have been known to fool themselves.

Authority

The picture I've initially drawn is of two camps--in some ways corresponding to the idea of Snow's "Two Cultures": The camp of doers, with their mainly applied research, technology and practical concerns; and the camp of thinkers, with

their theories and ideas.

Of course, in actuality the division is mistaken, misleading--and impossible. Moreover, it is implicitly demeaning of the participants in both camps. The reader is aware of the reputed depravity of the "mere philosophers" who "never do the research themselves." However, some of us in the "real world" camp are not quite as aware of the depravity of people who would call themselves "scholars" but can't be bothered by thinking. This writer is sure that both are vicious caricatures which one seldom encounters in reality. What do we, in fact, find? We find many philosophers who understand the doing of Science very well. (I'm referring here to people whose work relates to Science and the philosophy of Science). They understand Science not only in terms of principles and theories, but also in terms of apparatus, its uses and limits, and the politics and economics of doing experimental Science. It is absurd and false to say that one can know nothing about Science without doing it regularly. Not, incidentally, there are even philosophers of Science who are very good practicing scientists; but that is almost irrelevant to my argument here.

On the "practical" side, we find researchers with an excellent command of theory, its philosophical status and nature, and its imprisoning as well as illuminating nature. True, if you look around special education or psychology, the number of such people does not appear to be staggering. But, in a field like physics, or astronomy or (even) archaeology, such experimental scientists are numerous (if not typical).

The point here is that, if we divide the camps with the claim that the doers don't think because they are doing, and the thinkers don't do because they are thinking, we have isolated the wrong variable as an explanation of the problem--even though some doers don't think enough or thinkers do enough. The mindlessness I see in, for example, learning disabilities is not a consequence of practical

research activities. There are equally practical research scientists who do think and theorize at the highest level, who do not act mindlessly. The problem is much more complicated. There are thoughtful and productive scholars in both camps, and thoughtless and unproductive ones in both camps. Of course, there are still other types--for example, the "posturers," people who don't know very much about anything, but who have fallen into playing roles. In Socrates' day, they were called Sophists. Enough said about them.

And then there are most of the rest--people whose lives and work never even touch on theories or Science or scholarship. They claim to exist under the umbrella but not the commitment of the contemplative life--as professors, researchers, scientists, philosophers--and consequently have to keep up appearances, but they are essentially technicians. Presented with the invention of a wheel, they spend their lives making it rounder or smoother, putting pneumatic tires on it, or putting spokes in it to make it lighter--all good and honest endeavors. They do the replication experiments, standardizations, and extensions to analogous phenomena or populations. Their main contact with a theoretical structure is passive; they accept the theoretical status quo as a given, as the atmosphere within which they work but for which they are not responsible. If the theory needs adjusting, they assume someone else will take care of it, but they tend to trust that it is just fine the way it is. They also like to regard it--the status quo--as "obvious," so that anyone who is too concerned with it appears to be tilting at windmills, making waves, and wasting (everyone's) time. They are right! From their perspective and for the purposes of their work, concern with the theoretical is irrelevant. They aren't scientists, they aren't scholars, they aren't researchers. But neither are they useless, wicked or dishonest. They are simply technicians, people without whom the scientists would go crazy and make little progress.

If this taxonomy is not incorrect, then the picture of what's wrong with

special education and related fields could be understood somewhat differently than it has. It isn't that the people in special education are too hurried or compromised by career demands. Rather, there is a serious gap in the division of necessary labor; the workforce consists mainly of those we might better call technicians. But rarely is anybody in charge. And in this culture, our urging for an "examination of knowledge and values" has no significant appreciative audience. The technicians don't, won't, and can't (shouldn't?) care. To entice them to care will make them uncomfortable (Maybe the obvious isn't obvious!). It might force them to pretend to care and understand. But they won't keep it up for long or be very good at it. It's like gathering the nation's policemen to ponder some crisis of jurisprudence. They will act pompous and listen politely but wonder why they must listen to such far-fetched nonsense. After all, what will matter to them will still be the speed limit, good patrol cars and the practical details of enforcement. Without them a just society will be impossible. But don't look to them to redefine it. So too with all technicians.

What I think is needed is a genuine community--that is, one bound together by mutual respect and needs. Such a group would require the inclusion of both doers and reflective thinkers. There are people who devote themselves to theory building and others who test it. Those who create theory must respect the part the implementers play in the world, as the implementers must likewise respect the wisdom and importance of the theoreticians. But again, such dichotomies don't actually exist in real life. And what immediately arises from the thought of such a culture is the problem of authority. That's why this is such a troublesome idea, particularly in this country; we confound authority with the idea of power. Most of the academic reforms of the last decade were aimed at softening the power of professors, administrators and other educational leaders. But in the process, we did away as well with whatever legitimate authority should have continued to exist. Perhaps

I'm making a distinction which is not natural to our language. The notion of "bowing to authority" has carried a negative sense far longer than immediate memory. But the distinction must be made. There is that which can be expressed no better than with the term, "legitimate authority," before which it is not only permissible but wise and sensible to bow. Despite occasional disaffection, the authority of Einstein in physics is acknowledged and respected by our greatest physicists. It does not diminish them. Rather, it enables them to proceed to greater achievements. On the other hand, the idea of power in the academic world infects the doctoral process and threatens the roots of the scholarly enterprise. The difference between authority and power here is the difference between saying "because I know" and saying "because I say so." One thing that has befallen us is that we have ceased to believe that anyone knows. There is little if any authority, and the toiling technicians are left to their own sense of orthodoxy to carry them through life.

One aspect of this atrophy is a lack of understanding of what it is to know something. In our field, knowing exists only as a strawman to be righteously brutalized on ceremonial occasions. This is because "to know," so the current version goes, is to have a grip on something eternally immutable, unchangeable, unassailable and permanently universally valid. Of course, then, when someone slips and says he knows something, he becomes instantly ridiculous. But, except for rare technical purposes in philosophy or religion (some of which have been truly ridiculous), the idea of knowledge has never been understood in that way. I believe that the strawman has become so pervasive that virtually everybody in our line of work would profit by a six month's or year's study of the meaning of "to know." It would take that long. And one mustn't think that "justified true belief" gets to the bottom of it. It's "merely" a part of it. What counts as "justified?" How do you know it's true? What happens when your justified belief is false? So, to "examine what we know" is a premature start. We should first get straight: what we can look for

as "knowledge;" and in what contexts; and what we want to know; and why we want to know it. As part of that enterprise, we must establish a prerequisite world-view in which our knowledge can carry its proper weight and guide our actions. As Socrates might have said, knowing that one doesn't know something is oftentimes knowing a great deal. Possibly, that's why there is more power in our field than authority, more doing than knowing.

Misplaced Precision

What is the social scientist doing when he presents infant age data to the nearest hundredth of a month or the mean I.Q. score of a group carried out two decimals? Usually bad mathematics and bad science. It is usually wrong because researchers feel obliged to be too right. The ethos of our scholarly journals requires a facade of numerical precision. So, in creating the facade, even otherwise sensible researchers trick themselves into saying things which are ultimately less accurate than the layman's reckoning which they seek to transcend. Unless we know the birth hour and minute of those infants, we shouldn't aggregate the ages of such a group as if we have such information. And are I.Q. scores carried out two decimals more accurate than giving them to the nearest five or even ten points? Test reliability or validity do not clarify such matters. In almost all situations of this kind, the response to such "precision" should be "so what?" If mathematical rigor were really the object in these cases, it would demand that researchers not only master the statistical methods which are central in their reports but that they would show greater respect for elementary principles. It should be evident that the most elegant manipulation cannot generate more significant figures than the raw data contain. Since such principles are not difficult to learn and apply, the inference must be that rigor is not the point. In my judgment, the point is to ape the manners of "hard" Science. And the consequence

is a betrayal of what any science must rest on--carefully fashioned appropriate tools, thoughtfully used, in the service of human judgment.

What I am driving at revolves on the idea that many researchers in special education have misunderstood the nature of scientific inquiry. They have been dazzled by the precise measurements, exact formulas, and tidy theories of the physical sciences; and they have consequently proceeded to emulate these as though they were the heart of Science. Yet, a Nobel prize-winning scientist briefly if at all mentions any of these things in his description of scientific discovery. His work is, "To see what everyone has seen and, then, to think what no one has thought." The demand for formal uniformity in special education research--ranging from tables of "exact" numbers to APA-prescribed prose--is virtually a guarantee that not only will we wind up thinking what everyone has thought, but also overlooking what someone might have seen. What I have been discussing can also be taken as an indication that science and methodological technique have become a religion--"scientism," rather than science. That view, however, incorporates a mistaken derogation of religion. One of the major concerns of religion has always been with the danger when worship tends to become mechanical; ceremonies can drift into ritual, and devotion can degenerate to compliance. A pertinent part of Martin Luther's charge was that the pursuit of knowledge--whether divine or early--cannot be routinized without becoming a meaningless travesty.

Suggestions for the Applied Scholar

Since I am not able to actually instruct a person on how to develop scholarly judgment, I'll offer some suggestions (really ideas) to ponder. Remember, more than one smart person has advised that the only advice someone can give someone else is not to quickly take advice.

- (a) Being unable to observe something does not necessarily

prohibit one from learning about it. For example, what's "in the head" is important, even if unseen.

- (b) Scientific knowledge depends on observation and experimentation. But the nature of the observation and the methodology of the experiment is up for grabs, as long as they meet the canons of science: efforts at validity and honesty--(especially honesty).
- (c) Never entirely trust a methodologist to determine your problem, nor the philosopher of Science to do your Science. Most philosophers of Science are to scientists what most literary critics are to literary creators, and what most research methodologists are to researchers. Some people can truly pull off more than one role successfully, but there aren't too many of those around. But even if they are around, never entirely trust someone else to determine your problem.
- (d) We can agree that there are problems we'll never get right. But fortunately, we don't agree on what those problems are. So there's enough for everyone--for all the scientists, and other scholars, methodologists, and other technicians.
- (e) Maybe there is an ultimate truth. Maybe there isn't. Maybe if we looked hard enough we would find it. Maybe it's only that each perspective offers a window to truth, and that no single perspective can ever be the whole truth. Maybe one view is right, and maybe the other. But maybe they're both right. Maybe we should search

for the truth as if it's there. But maybe we should also be interested in not only how people agree on what's there, but how they differ. Maybe we should take obvious differences as seriously as we take obvious agreements. This is by way of saying that the practicing scholar might well think about theories as approximations and, hence, about truths as approximations.

- (f) While knowledge may depend on observations and not theoretical formulations, can knowledge be created from values? Not necessarily. Certainly not invariably. "Blacks are equal," asserts the educational reformer. "Schooling is good for children," concludes the educational sociologist. "Women are equal," is the consensus belief of the masses. If X is the value, isn't Y truth quick on its heels? Not necessarily.
- (g) When is a theory acceptable? When it's tested and not disconfirmed once or twice? That idea seems to have infected many graduate students, if not all of their professors. A theory is acceptable when there are X primary hypotheses and a significant variety of auxiliary hypotheses which support it. Furthermore, a theory stands up when it isn't in violation of past "successful" theories.
- (h) The more expert we are--the more we know--the more ambiguous we may be about what we know. For example, someone like John Money knows a great deal about human gender, much more than most other people. Notwithstanding,

John Money takes a great deal longer to decide who is and who isn't male or female. The man in the street knows instantly who the male is and who the female is; and if there is any doubt in his mind, he knows how to quickly resolve that doubt. John Money requires more than physical observation to make such a determination. He may need powerful microscopes to look at the twenty-third pair of a person's chromosomes. It isn't necessarily so that the more we know about a problem the more we are certain about it. People who know a great deal about mental retardation oftentimes have much more difficulty deciding who is mentally retarded than the man in the street. That man in the street knows for sure who is mentally retarded when he sees someone with Down's syndrome or hydrocephaly or cerebral palsy. The specialist in mental retardation may require excruciatingly detailed tests before he would agree that any one of those people is mentally retarded because, indeed, there are people with Down's syndrome, hydrocephaly, or cerebral palsy who aren't mentally retarded. Is the fetus a human being? How many facts does a scientist need to make that determination? Empiricism isn't dead, but it is limited insofar as the applied scholar is concerned. Possibly, that's why it may be all right for philosophers to argue about approximate truth; but clinicians and other applied scholars don't have to argue on that score. Or as one scientist said to this writer years and years ago, a good method (even if we don't know why it works) is much better than a barrelful of

good hypotheses which are verified, but inapplicable to the amelioration of suffering or the enhancement of life.

- (i) The researcher must always compromise between the size of his sample and the number of variables he can control. If he has a large number of subjects to examine, then variable size will suffer; and if he must deal with many and complex variables, his sample will need to be that much more restricted.
- (j) Some potential experiments offer the possibility of such dangerous consequences that we should avoid the temptation to get into them. That's one of the arguments embedded in the nuclear energy controversy. But we also have examples in our own work, possibly the various school voucher systems being one of these.
- (k) On the one hand, we discuss our literature with almost utter disregard to both the canons of Science and whatever else it is that makes for common sense. Most literature reviews are indiscriminate, non-evaluative, and unrevealing. The textbook reports that Author A claims mental retardation exists in 16% of the population, but that Author B claims that mental retardation exists in only 1% of the population. Then there is silence on the issue. What is the reader to make of all this, especially since the professor (as well as the textbook) neglects to resolve a discrepancy of such magnitude?
- (l) On the other hand, the problems in our field may be too value-laden to be left simply to the control of scientific traditionalists. Our philosophers and sociologists of

Science can tell us what we don't know, but they can't tell us what we should believe. Consequently, we may be tempted to be discouraged with science qua science, but we should never give up on scholarship. What might be the shape of that scholarship? For one thing, we could look for exemplary models to study, rather than spend so much of our time on comparing A to B or B to C when none of those models is very good to begin with

Discussion

I've laid out a list of suggestions, which you can take or leave. My greater purpose has been to interest you in the idea that we must be more mindful of the pitfalls of Science better--especially ritualistic Science (which isn't Science). It will be difficult for us to change in this regard. Embedded in the human condition is the belief that one doesn't need to change. Ordinary individuals resist change like the plague. And scientists are no different. "Getting at the real truth" means that one now has lies. Who knows but that our ways of doing business in the social sciences has more to do with our inability to give up our lies and our incapability in truly understanding this pervasive characteristic of human beings--resistance to change.

What then should special educators be concerned with? We must corner the values market, and let the pure scientists and technicians own the "truth" debate, the exotic methods, and the gadgets. If one angel can stand on the head of a pin, how many angels can stand on the head of a pin? There is no intent here to insult philosophers. Indeed, I believe this may be an important theoretical problem for philosophers of Science. But it's a trivial problem for scholars in Special Education. While even pure scientists know more than the data they've collected in

support of a theory (that is, even pure scientists rely on earlier studies), applied scientists too have those earlier studies in their baggage--but also have as well a set of values, and an agenda to make the world better.

That brings me to the heart of my argument. We hear time and time again that scholars are in the business of seeking truth. Of course they are. But why? The "why" is the important question. As a colleague recently remarked to me, scholars seek truth because that's what they do best. But they, like everyone else, are in the business to make the world a better place. That's everyone's job, even the scholar's. For handicapped people who seek equal rights, who seek to be free, to have the stigma washed from their souls as well as bodies, it's not enough for us to demand that they, the handicapped, change. It's not even enough for the majority of the people to change. If all these things we've been arguing for are to come true, the society has to change--fundamentally and pervasively. That isn't going to happen if our scholarly arguments are only about real or perceived truth. But it also isn't going to happen if we don't have scholarly arguments, if our research remains in the wretched, narrow, thoughtless, muddled state it's in now.

I've revealed more about myself than you may have wanted to know.

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