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PHILOSOPHY OF SCIENCE AND THE QUANTITATIVE–QUALITATIVE DEBATES:

Thirteen Common Errors

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ABSTRACT

One of the most important benefits of the qualitative–quantitative debate in evaluation has been the increased awareness it has brought evaluators about philosophy of science. But evaluators are rarely philosophers, and consequently their presentations of philosophical material may contain errors. This article highlights thirteen common errors of this kind, and discusses some implications of these errors for the quantitative–qualitative debate.

What are the most important accomplishments in the first 30 years of program evaluation? To me, the introduction of qualitative methods into evaluation should certainly be near the top of the list (Shadish, Cook, & Leviton, 1991). Without doubt, these methods have proven their utility to practicing evaluators, their distinctiveness to theorists, and their attractiveness to readers of evaluation results. Qualitative methods are here to stay, and evaluation is much better for it.

In the process of introducing these methods, many theorists have also introduced arguments from philosophy of science that explain why these methods ought to receive more attention. This philosophical material is as welcome as the methods themselves, helping us to think more clearly and completely about why we do what we do. For example, as a result of these arguments, most evaluators are more aware of the socially constructed nature of evaluative knowledge, of the philosophical ambiguities that necessarily surround any scientific methodology, and of

the near impossibility of justifying any particular method as being always and everywhere best.

Unfortunately, very few evaluators are philosophers by training. Most evaluators learned about these matters either by reading what other evaluators say about philosophy of science, or more rarely by reading original philosophical works themselves. In my own work, I have learned from both sources. In the process, however, I have also learned that reading primary sources in philosophy is a far more reliable source of what philosophers say than is reading what evaluation theorists say philosophers say. The discrepancies are often substantial. Hence the goal of the present article is to correct 13 common errors about philosophy of science that seem to be prevalent in the quantitative–qualitative debate today (see Phillips, 1990, for a similar work covering largely different ground).

I should begin, however, with a number disclaimers. First, I do not purport to present any original philosophical research in this article; my own quasi-philosophical views

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are outlined elsewhere (e.g., Shadish, 1994). Rather, I simply juxtapose some things said in the evaluation literature against divergent descriptions of the same matters in philosophy of science. I hope the juxtaposition will raise enough concern in readers' minds to reduce the frequency with which these errors occur. Second, my aim is not to criticize particular evaluation theorists. To the contrary, those theorists who have written about these matters should be congratulated for stimulating all evaluators to read and think more about philosophy of science. Indeed, since I am not a philosopher myself, I will undoubtedly make mistakes of my own in this article. But hopefully this article will still advance the conceptual debate by correcting some of the more serious and obvious errors, by pointing to questions that can be further explored in the interest of accuracy, and in the end by making fewer mistakes than it corrects. Third, these errors are most commonly found in debates between quantitative and qualitative evaluators, so I will refer to that debate to illustrate these errors. But the errors are not limited to that context. Rather, the errors are made by evaluators of nearly all methodological and theoretical ilk. These errors come from an equal opportunity demon, our own ignorance. Fourth, I apologize at the start for the jargon that is inevitably part of philosophical discussions. I try to keep the jargon to a minimum, and to explain it where it is used. But sometimes I introduce terms (like relativism, or constructivism) simply to identify certain alternatives that would take far more space than I have here to discuss fully. I hope I have struck a reasonable balance, however, so that the end result is understandable. For evaluators who want to know more, excellent introductions by philosophers are available (Bechtel, 1988; Brown, 1977; Laudan, 1992; Phillips, 1987, 1990).

Finally, I do not extend this discussion to showing how each error might carry implications for the practice of evaluation. Sometimes the implications are simple and obvious (there is nothing wrong with talking about and investigating causes); but sometimes they are complex and subtle (whether or not experiments are about confirmation or discovery, a theory of when we should use experiments would depend on many more factors that I can cover here). Indeed, we err if we think that our practice ought to be guided primarily by philosophy of science. Evaluation practice risks serious loss of contact with its political, social, and economic roots to the extent that it takes philosophy of science as a starting point for its practice. But as an academic, I can afford the luxury of speculating on these more philosophical matters. Which I shall now do.

COMMON ERROR #1: SOME EVALUATION THEORISTS ARE LOGICAL POSITIVISTS

Logical positivism is a phrase most evaluators recognize. But few evaluators know what it is; and there are many

misstatements of its key tenets. Indeed, it is not always clear that evaluation theorists intend to use the term accurately. Rather, the term has frequently become the linguistic equivalent of "bad," a rhetorical device aimed at depriving one's opponent of credibility by name-calling. This is particularly true in the quantitative-qualitative debate where some qualitative theorists are fond of labelling all quantitative opponents as logical positivists. But it is not limited to that debate. Logical positivism has become the favorite way to characterize the "status quo" whenever a "new paradigm" is discussed in evaluation, including those discussions that claim to be integrating quantitative and qualitative positions.

If one reads the evaluation literature, one can find almost as many descriptions of logical positivism as there are evaluation theorists. In part this is understandable, for the roots of logical positivism go back hundreds of years, and much depends on the philosopher to whom one refers (Bechtel, 1988). Still, the most common referent is usually to the Vienna Circle in Austria, and the Berlin School in Germany, in the early part of the 20th century. If one boils down their logical positivism to its essence, two characteristics are crucial. The "positivist" part of the label refers to the notion that knowledge should be based on direct experience; and the "logical" part invokes a commitment to theory development using a rigorous procedural language such as symbolic logic. Knowledge comes either from direct experience or indirectly from inferences from experience through the procedural language.

Most evaluation theorists describe the positivist part of the label accurately enough, although they often fail to give due credit to logical positivists themselves for their own disputes about the extent to which knowledge really can be grounded in experience. But few evaluation theorists even discuss the part of the label referring to logic. This part refers to a commitment to an axiomatic account of theory in which events are derived from laws (and other known facts) which are in turn derived from more general theories. Logical positivists most often held out first-order predicate logic as the ultimate scientific language for use in this process. But in spirit, they would accept any rigorous procedural language. Conversely, they generally distrusted ordinary natural language because ordinary words carry too many stray meanings to be rigorous tools of scientific inquiry.

If we were to acknowledge this part of logical positivism, then we would be compelled to admit that no evaluation theorist appears to be much of a logical positivist, either today or in the past. After all, when was the last time that you saw symbolic logic on the pages of an evaluation journal or book? When has an evaluation theorist professed a desire to axiomatize any theory or evaluation in terms other than ordinary natural language? Indeed, when has an evaluation theorist even contended that some theory could be rationally reconstructed using

such logic? When was this even an issue for them? In fact, all evaluations, and all evaluation theory, seem to be couched in ordinary natural language. And evaluators of all philosophical and methodological ilk routinely discuss inferences from the data that are not rigorously justified through any neutral procedural language. Let's face it, there are few if any logical positivists in evaluation theory (nor in evaluation practice, but more on that later).

COMMON ERROR #2: SOME PHILOSOPHERS ARE LOGICAL POSITIVISTS

If you can't find an evaluator who is a logical positivist, can you find a philosopher who is? For the most part, no. The particular set of solutions and strategies that logical positivism advocated disappeared from philosophy many years ago (Phillips, 1990). As Paul Meehl (1986) pointed out:

Logical positivism, in anything like the sense of Vienna in the late twenties, turned out not to be logically defensible, or even rigorously formulatable, by its adherents. It is epistemologically unsound from a variety of viewpoints (including ordinary language analysis), it is not an accurate picture of the structure of advanced sciences such as physics, and it is grossly inadequate as a reconstruction of empirical history of science. So it is dead. All old surviving logical positivists agree, including my friend and teacher Feigl, who invented the phrase "logical positivism" and was the first to introduce the approach in the United States in 1931. The last remaining defender of anything like logical positivism was Gustav Bergmann, who ceased to do so by the late 1940s. (Meehl, 1986, p. 315)

Such disavowals ought to alert us that something is seriously wrong with any argument that treats logical positivism as if it were a current philosophical opponent.

Of course, we should not be so literal as to overlook the senses in which the spirit of logical positivism lives on in modern philosophy. Some modern philosophical schools can trace their roots more closely to logical positivism than can others. These include some (but not all) modern philosophical analyses of causation (e.g., Salmon, 1984), formal semantic analyses of science (e.g., Van Fraassen, 1980), and ideal language analytic philosophy generally (e.g., Leinfellner, 1983). More abstractly, it helps to distinguish between the problems that logical positivists took as important versus the solutions they advocated for those problems. The particular set of solutions that logical positivists advocated — in particular, the combination of positivist empiricism with rigorous procedural language — disappeared long ago. The problems, however, remain of central interest to many current philosophers. For example, one can find many examples of philosophers who endorse the general goal of explaining the most phenomena with the fewest entities, or of

searching for methods that would yield definitive scientific answers. One can find philosophers who believe that we do not have an adequate account of science unless we have a formal account of the nature of explanation and confirmation in science. These philosophers may embrace at least some of the ambitious problems of logical positivism even if they discard the solutions.

But one must be careful in using this evidence to conclude that logical positivism still exists and so must be criticized, for two reasons. First, it means we are all subject to criticism for the sins of our philosophical heritage. For example, the solutions posed by certain deconstructionist theorists today could be seen as heir to older relativist or skeptic philosophies that have long been criticized as self-contradictory (all reality is personally created; so no single reality construction is better than another; my skeptical theory is just my construction; therefore, my skeptical theory is also no better than your realist theory). If we are to criticize our heritages because we are interested in the same problems, all of us become vulnerable. Second, to the extent that some current philosophers could be considered heirs to the logical positivists, we must again admit this reformulated logical positivism as a currently viable philosophical position. My philosopher friends tell me, for example, that there is now a movement to resituate logical positivism as part of the general Neokantian strain in early 20th century Austro-German philosophy, and that in this cultural context some younger philosophers do not mind being called logical positivists. But this strays far from the usual target of criticism — the Vienna Circle at the turn of the century. Sticking to that target and the solutions it posed, logical positivism is no longer a worthy opponent.

COMMON ERROR #3: MOST PRACTICING EVALUATORS (OR THEIR CLIENTS) ARE IMPLICIT LOGICAL POSITIVISTS

With a little argument like that above, one can usually get most opponents in a debate to admit that there are few, if any, evaluation theorists or philosophers who are logical positivists. But these opponents are often still reluctant to give up criticism of logical positivism, claiming that many evaluation practitioners or evaluation clients are *implicit* logical positivists. That is, the beliefs about evaluation held by such persons reflect assumptions that they may not recognize, but that are more similar to the tenets of logical positivism than to any other conceptual scheme. If so, the argument goes, we still need to correct their beliefs. The problem with this reasoning is that such assumptions may be implicitly consistent with a host of approaches to science, not just logical positivism. If they came from philosophy at all (which I doubt), it makes

much more sense to think these assumptions came from some philosophical influence other than logical positivism, for two reasons. First, these “implicit logical positivists” almost never display knowledge of many aspects of logical positivism that are closely tied to the position. If they were logical positivists at all, surely they would at least say that they aspire to axiomatize some theory, or they would make a pretense towards developing a rigorous procedural language! They don’t; they all talk in plain English despite the excess meanings. More importantly, such people *do* display features that are more consistent with other understandings of science. For example, their interest in practical problem solving (Does the program work?) is in many respects more consistent with pragmatism than logical positivism. The logical positivists were quite clear that science is distinct from its applications in technology; and they were not much interested in application. Pragmatists made no such distinction, and were deeply interested in application. So whatever assumptions clients might make about the evaluation process, we have little credible reason to attribute those beliefs specifically to any knowledge of or sympathies with logical positivism, and at least some reason to make the attribution to pragmatism.

Even more fundamentally, however, it is probably wrong to think that their assumptions about science have any philosophical roots at all. It seems much more plausible to think their views about science have sociological roots that we, as scientists, are responsible for fostering. Specifically, since WWII modern science has marketed a highly idealized and naive image of itself to the public and to the government. In that image, science is nearly infallible, always progressing, yielding ever increasing returns for each dollar allocated, and successfully self-monitoring and self-correcting. Such an image has historically been functional for scientists because it helps generate increased funding and independence, so we have a vested interest in convincing nonscientists of the validity of this image. It seems far more plausible to attribute naive assumptions about science to our marketing of this image than to some presumed causal connection to logical positivism specifically or philosophy generally.

Defining the problem as the philosophy of logical positivism rather than as the sociology of science causes another difficulty, as well. The definition of the problem carries in it the definition of the solution. If the problem is logical positivism, then the solution is educating evaluators and clients about what logical positivism is and why it proved to be such a bad idea. But if the problem is the marketing of a naive image of science, the solution is educating evaluators and clients about how science really works, warts and all. Viewed this way, the choice is clearer. Educating clients about philosophy is, with a few exceptions, probably not the best use of our time or theirs’. Conveying a more realistic image of what we

can do seems a much better idea. If we don’t do so, when obvious failures of that naive image inevitably occur (e.g., catastrophic failures at NASA, increased visibility of cases of scientific fraud and abuse), critics may take an increasingly cynical view of the credibility of the scientific community.

A footnote to these first three errors concerns the tendency to conflate the term logical positivism with the term postpositivism. The two terms describe philosophies that are similar in name only, and that describe very different philosophies of science (Phillips, 1990; Reichardt & Rallis, 1994). In fact, while logical positivism has a fairly clear referent in the Vienna Circle, the term postpositivism has been used in many different ways in the philosophical literature to describe many different philosophical successors to logical positivism. Consequently, it completely misses the mark to criticize as logical positivists those authors who identify themselves as postpositivists (e.g., Cook, 1985).

COMMON ERROR #4: REALISM IS DEAD

Those familiar with the philosophical literature will recognize immediately that this is wrong. A recent edited book on *Scientific Realism* by Leplin (1984), for example, displays vigorous, factious debate about realism by a number of philosophers from a number of perspectives. Perhaps the only agreement in this literature is that naive realism is not viable; that is, we do not directly perceive reality and report it uncontaminated by our theories. But many contemporary philosophers espouse other versions of realism, and their arguments are taken seriously even by those who disagree with them. If anything, the topic of realism has received new life in the last decade. Ironically, the new interest started with Hillary Putnam’s (1984) memorable thesis that realism is the only philosophy that does not make the success of science a miracle — ironic because Putnam stopped being a realist shortly thereafter. But an informative recent summary of issues is available in Laudan’s (1992) *Science and Relativism*, a dialogue between fictitious philosophers representing realism, pragmatism, relativism, and positivism. Realism is still an active and respected participant in the debate.

But the basic terms of this debate are rarely understood by evaluators who fail to distinguish between two very different issues in realism — ontological realism versus scientific (or epistemological) realism. Ontological realism debates what is “really real,” especially whatever real entities or structures might exist beyond appearances. Perhaps the best exemplar of this debate in the last two centuries is atomism, that is, the notion arising in both physics and some philosophy that physical appearances merely reflect an underlying atomic reality — that

atoms (and their constituent parts) are really real, not chairs or other things we appear to see. Of course, some philosophers deny the existence of an external reality independent of our constructions of it (Phillips, 1990, calls this relativism); but most advocate some version of ontological realism, if they talk much about it at all. Ontological realism is not much of an issue at all in philosophy of science. Most philosophers of science apparently lean towards the belief that some external material reality exists, to judge from the few times they address the question directly. This is true even of most social constructivists, a label that generally refers to constructing *knowledge about* reality, not constructing reality itself. Further, most philosophers of science seem to regard the matter of whether there is an external reality as ultimately quite difficult to prove one way or the other. Of course, there are some important ontological arguments here. Harre (1986) and Bhaskar (1978), for example, claim that experiments would make little sense if the structure of reality were not hidden from direct experience. So they want to determine what features of reality might be presupposed by those activities we call experiment (and science generally).

On the whole, however, ontological realism is not where the action is in philosophy of science. The action is in epistemological realism generally, and more specifically in scientific realism. The former refers to whether we can have knowledge of what is real, and the latter refers to whether external reality constrains that particular form of knowledge that we call scientific theory. If there are atoms, do they affect our theories at all? If our theory postulates an atom, is it describing an ontologically real entity that exists in the way we describe it? Here the debate is between scientific realists versus antirealists (Phillips, 1990). Such scientific antirealism is quite different from ontological relativism — one can believe there is a reality, but that our theories are only partly or even not much at all affected by it.

Some evaluation theorists have suggested that neither version of realism is considered a viable position in philosophy of science. Ontologically, for example, they claim that reality itself does not exist until it is created or constructed by an actor. They further claim this position is widely shared, perhaps dominant, in the philosophical literature. It is not. Most philosophers probably hold some sort of middle ground. Philosopher Harold Brown (1977) elaborates:

The dichotomy between the view of perception as the passive observation of objects which are whatever they appear to be and perception as the creation of perceptual objects out of nothing is by no means exhaustive. A third possibility is that we shape our percepts out of an already structured but still malleable material. This perceptual material, whatever it may be, will serve to limit the class of possible constructs without dictating a unique percept. (p. 93)

Brown then gives an example from the hermeneutic literature where it has sometimes been argued that the existence of different textual interpretations should be taken as evidence of created ontological reality:

Again the parallel with reading is illuminating. A variety of interpretations of, for example, the *Critique of Pure Reason* have been proposed, but no matter how widely scholars differ on what is the correct reading of the text, no one can open the *Critique* and read the *Nichomachean Ethics* or *Moby Dick*. (p. 93; italics in original)

Those who deny the latter claim, who assert that one might indeed read the *Critique* as *Moby Dick*, are often referred to as skeptics. But we must distinguish naive from serious skepticism:

Scepticism of the sort “do I know this is a hand before me” is called “naive” when it would be better described as degenerate. The serious scepticism which is associated with it is not, “is this a hand rather than a goat or an hallucination?” but one that originates with the more challenging worry that the hand represented as flesh and bone is false, while the hand represented as atoms and the void is more correct. (Hacking, 1983, p. 141)

It is not the existence of reality that is usually in doubt, but the constructed representation of the reality. In terms we are more familiar with, scientific realism presents a construct validity problem.

COMMON ERROR #5: LOGICAL POSITIVISTS ARE REALISTS

With the distinction between ontological versus scientific realism in mind, we can now also address this misconception. In the sense of ontological realism, it is particularly ironic to say that logical positivists are realists. A crucial impetus to starting logical positivism in the first place was to combat what they saw as the excesses of metaphysical debates (Diesing, 1991). They regarded metaphysical questions about whether, for example, numbers actually exist as pseudoquestions resulting from misuse of language. Their very aim was to do away with such discussions in science, replacing them with a scientific language that referred only to observables and things constructed directly from observables using formal logic.

On the question of scientific realism, logical positivists usually distinguished between observable versus unobservable entities. They claimed that scientific theories could be realistic in the sense that they could reliably refer to observable experiences; in fact, most philosophies of science grant this sort of realism. But this sense of realism may be closer to pragmatism than to positivism, for it emphasizes a theory’s usefulness in predicting experience rather than whether those experiences reflect an underlying

metaphysical reality. Logical positivists were far more cautious on the matter of unobservable entities, things like atoms or quarks (or in social science, things like intelligence or satisfaction). On the one hand, logical positivists were impressed with the apparent empirical successes of some theoretical constructs in science that referred to unobservable entities in physics. So they did not want to reject out of hand the possibility that these entities might actually exist. This ambiguous position is perhaps best exemplified by Hempel (1965) who suggested that if you are not some sort of ontological realist, then it is not clear what the point of theorizing is. On the other hand, logical positivists tended to view claims about unobservable entities with skepticism. Although the following quote is taken from a colleague of J.S. Mills over a century ago, it captures the logical positivist's view of the metaphysical reality of unobservable constructs quite well:

Some hypotheses consist of assumptions as to the minute structure and operation of bodies. From the nature of the case these assumptions can never be proved by direct means. Their merit is their suitability to express phenomena. They are Representative Fictions. (Bain, 1870, p. 362; cited in Hacking, 1984, p. 169).

Clearly, if one had to choose whether logical positivists were realists, the fairest simple answer is that they probably were not — unless one insists on equating Fiction with Reality.

COMMON ERROR #6: CAUSATION IS DEAD

Some participants in the quantitative–qualitative debate claim that it has proven philosophically impossible to formulate a viable view of causation, that any further searches for such a view are hopeless, and that philosophers generally believe that in view of such difficulties the concept of causation ought to be dropped entirely from philosophical and methodological discussions. In contrast to such claims, anyone who looks in the philosophical literature of the last decade will find many mainstream philosophical works on causation and related topics (e.g., Eells, 1991; Humphreys, 1986a, 1989; Salmon, 1984, 1989). To be sure, philosophers and others vigorously disagree about many things regarding causation: whether or how a probabilistic theory of causation can be formulated and justified, how generalizations from specific causal claims can be warranted, how causal statements relate to scientific explanation, and how causal claims might imply dispositional ontologies about the latent forces that might give rise to an observed causal relationship. Equally sure, philosophers have not reached consensus on the answers to these vexing difficulties. But such debates are not signs that causation is dead. Just the opposite; they suggest that causation is still an essential concept in philosophy of sci-

ence, still engaging the attention of many mainstream philosophers, still advancing its program.

In fact, this is a case where those social scientists who write about causation may have much to offer to philosophy of science. Philosopher Paul Humphreys (1986b), for example, notes that causal claims made in the social sciences are probabilistic rather than deterministic, occur in a context with multiple causes rather than a single one, and use variables that imperfectly reflect some latent constructs thought likely to be the causal agent of actual interest to the researcher. By contrast, “if one then compares these causal claims with traditional analyses of causation which are available in the philosophical literature, those analyses begin to look almost willfully nondescriptive” (Humphreys, 1986b, p. 1). Those traditional philosophical analyses tend to be deterministic, and to concern one cause and one effect, both of which are taken to be perfectly available to the analyst (i.e., perfectly known and measured). The special issue of *Synthese* that he then edited (Humphreys, 1986a) brought together diverse social scientists from fields like causal modeling and quasi-experimentation to help construct and inform philosophical debates about how causality is conceptualized in these problematic social sciences. Far from being out of the philosophical mainstream, those social scientists who are writing about causation may well be in the forefront of that philosophical debate.

Perhaps the most compelling evidence that causation is alive and well comes from the writings of many qualitative theorists themselves. Harre (1986), for example, suggests that qualitative research is quite compatible with causation. And this kind of thinking finds its British pedigree in discussions of causation in the law (Hart & Honore, 1959, 1985) — discussions that reflect an almost entirely qualitative view of causation. However, even those who deny the viability of causation, and who studiously avoid using any version of the word, almost inevitably find themselves slipping into causal-sounding language. Is, for example, a concept like mutual reciprocal influence really successful in avoiding the concept of causation; or does it just avoid the word? Isn't such a term a way of sneaking causal concepts and functions back into the theory under a new guise? In fact, close examination of such terms invariably shows little new content that has not already been anticipated and incorporated in the relevant philosophical literature on causation. Causation, then, is not only alive and well in the philosophical literature, it is also alive and well in the social science literature generally, including qualitative literature itself. It remains viable because it has proven useful to those of us with both quantitative and qualitative leanings. We should not be embarrassed to use the term.

It is possible to place another interpretation on this error entirely. It may be that those who seek to drop terms like causation are (a) *rightly* pointing to the flaws

in simple billiard-ball conceptions of cause that are best viewed as dead for the social sciences, but (b) *wrongly* attributing this billiard-ball model to quantitative researchers who, in fact, do not hold the model either, and (c) *wrongly* assuming that quantitative researchers would disagree with conceptions of causation that include such things as mutual influence, multiple causation, or probabilistic causation. In this interpretation, quantitative and qualitative researchers may think they disagree about causation, but actually they agree on most of the relevant issues. This interpretation would explain how such critics could continue to use causal-sounding language while seemingly rejecting causation itself.

COMMON ERROR #7: LOGICAL POSITIVISTS ARE COMMITTED TO CAUSATION

Ironically, the same theorists who incorrectly reject the currency of causation in contemporary philosophy of science sometimes compound the error by incorrectly attributing a commitment to causation to logical positivism. But this attribution is also wrong. Some understandings of the word cause were unequivocally rejected by the logical positivists. For instance, they rejected the Aristotelian notion of final cause (the purpose that gave rise to the phenomenon). Such causes were unobservable entities that were rejected by logical positivists as not being the domain of science. In addition, the roots of positivism (e.g., Mach, 1914) emphasized that the role of scientific theories was simply to predict observable phenomena, to “discover regular patterns among our sensations that will enable us to predict future sensations” (Salmon, 1984, p. 5). Cause was (and is) not necessary to such prediction:

In the Positivists’ account, the concept of cause had no special status and one might well explain an event without knowing its cause. Some laws invoked in explanations might state causal relationships, but that was not required. In fact, *the Positivists had no resources for distinguishing causal laws from other generalizations embedded in the axiomatic structure of a theory.* (Bechtel, 1988, p. 39; italics added for emphasis)

Similarly, among the earliest attacks on Hempel’s logical positivism was Scriven’s criticism of it for its near total neglect of causation (Salmon, 1989). Many past and present philosophers are committed to causation; but logical positivists were rarely among them.

COMMON ERROR #8: THE EXPERIMENT IS ABOUT CONFIRMATION, NOT DISCOVERY

In the quantitative–qualitative debate, the experiment has often been the battle ground. The experiment is often

taken as the epitome of quantitative methodology, so much so that criticisms of it are taken to be sufficient criticisms of quantitative methods generally. The experiment tends to be portrayed as quantitative rather than qualitative, and as about confirmation rather than discovery. Both portrayals are wrong. On the latter point, the essence of experimentation is to discover what happens when we intervene in a system. If we knew what would happen, we wouldn’t have to experiment; and most fields (including evaluation) are full of experiments that, one way or the other, resulted in the unexpected. The history of science is littered with examples of experimentally based discoveries:

The development of new and improved experimental apparatus and laboratory procedures . . . could also lead to totally unexpected discoveries, such as the Leyden jar in 1745, which set off an entirely new train of ideas and experiments. (Hackman, 1989, p. 58)

As Gooding (1989) put it, “People act upon Nature to generate new possibilities for observation” (p. 191). Over 200 years ago, Priestly (1775) made a similar observation about the discovery function of early experiments about electricity:

Hereby the laws of her action are observed, and the most important discoveries may be made; such as those who first contrived the instruments could have no idea of. (Priestley, 1775; quoted in Hackman, 1989, p. 42).

Indeed, one of the most celebrated episodes in experimental science is Galileo’s extensive use of programs of experiments as the centerpiece of his approach to science in the 1600s. Yet even there an historical analysis reveals that “experiment does not by any means play a positive confirmatory role to establish the claims of Galileo’s theory; on the contrary, it proves incapable of corroboration” (Naylor, 1989, pp. 117–118). Hacking goes so far as to observe:

Must there be a conjecture under test in order for an experiment to make sense? I think not The physicist George Darwin used to say that once in a while one should do a completely crazy experiment, like blowing the trumpet to the tulips every morning for a month. Probably nothing will happen, but if something did happen, that would be a stupendous discovery. (Hacking, 1983, p. 154)

The beauty of experimenting is that we cannot fully control what happens after we intervene. Inevitably we discover new things as a result.

Of course, it would be wrong to suggest that experiments only discover, never confirm. In fact, experiments do many things, of which discovery and confirmation are only two: “Experiment has many uses apart from supporting or refuting knowledge claims: active observation,

invention, the construction of models, imitation of natural phenomena, or the design of instruments to extend the senses" (Gooding, Pinch, & Schaffer, 1989a, p. xv). To which we might also add the goal of falsification in experimentation. When we portray the goals of experimentation more narrowly than this broad list, we do it an injustice.

COMMON ERROR #9: EXPERIMENTS ARE INHERENTLY QUANTITATIVE

But the reader may object that experiments constrain discovery by the dependent variables they measure. Historically, this may be an accurate observation; but logically, numbers are not intrinsic to the experiment. If experimenting is about intervening, nothing in the notion of intervening requires that either the intervention or the outcome be measured quantitatively (although they can be so measured, of course). One of the earliest experimental efforts in science — in fact, celebrated by some as the start of experimental science (Drake, 1981) — is reported in Galileo's 1612 treatise *Bodies That Stay Atop Water, or Move In It*. Galileo conducted a series of experiments to explore the variables that cause an object to stay on top of water or move (sink, rise) in it. In many of these experiments, no quantitative measurements were taken, and the report of the experiments rarely includes numbers; but the process was clearly experimental. Similarly, Naylor (1989) examined subsequent experimental work reported in Galileo's *Dialogue on the Two World Systems* and noted that it is remarkably devoid of quantitative work: "The only argument in the *Dialogue* that makes any call on quantitative issues is his tidal theory — which proves a remarkably poor fit with observation" (Naylor, 1989, p. 127).

Isaac Newton's experiments with prisms and light diffraction were also primarily qualitative in nature. Their essence was to examine how the color of light changed under different interventions. Over successive refractions, Newton held that "the apparent changes [in colour] would become smaller by repeated refractions, because the simpler colours would arise at every step" (Newton, quoted in Schaffer, 1989, p. 83). The examination of these apparent changes was qualitative. Hacking also discusses Newton's related work on the light interference phenomena that came to be called Newton's rings, noting that "the first quantitative explanation of this phenomenon was not made until more than a century later, in 1802" (Hacking, 1983, p. 156).

In the social sciences, Muzafer Sherif's famous Robbers Cave Experiment (Sherif, Harvey, White, Hood, & Sherif, 1961) was similarly mostly qualitative. At a summer camp, boys were divided into two groups of eleven each. Within-group cohesion was fostered for

each group separately, and then intergroup conflict was introduced. Finally, conflict was reduced using an intervention to facilitate equal status contact in the context of working on goals that required the cooperation of both groups. Much (but not all) of the data in this experiment, both process and outcome, was qualitative. It is easy to run a qualitative experiment, or a partly qualitative experiment. Instead of traditional measures, or in addition to them, add traditional qualitative methods of observation. Nothing in the experiment prevents this; and we would undoubtedly gain much by doing so.

So here, too, we must acknowledge that experiments can serve many functions, both quantitative and qualitative. Gooding, Pinch, and Schaffer (1989b) conclude the same thing when discussing "the range of experimental determination which Galileo allows his actors to employ; some trials work as refutation of rival positions, others are written as the qualitative establishment of a matter of fact, and yet other seem to imply specific quantitative estimates of the behavior of moving bodies" (p. 7). Again, we should not portray it more narrowly.

COMMON ERROR #10: EXPERIMENTERS ARE NAIVE REALISTS OR NAIVE POSITIVISTS

Critics sometimes claim that experimenters naively believe that the experiment is the royal road to truth, simply and directly revealing reality; and they often associate such naivete with positivism, even though we saw earlier that this does not accurately portray logical positivism. Contrast this characterization with the view of Donald Campbell, one of the foremost advocates of experimentation in evaluation. Far from grounding the experiment in positivism, Campbell (1988) likens the experiment to tribal divination rituals in which:

Caribou hunters roast a shoulder blade on the fire and use the cracks resulting to choose the direction the hunting party should take. The details of the ceremony contain many features designed to prevent human hunches from influencing the outcome, thus providing an uncontaminated channel through which the supernatural powers can speak if they will. Similarly, Norwegian fishermen once located new fishing sites by building a shoreline and island map of sand in a pan, filling it with water, and watching for the first place where bubbles rose. (pp. 501–502)

It's a mighty big leap from roasting shoulder blades and observing Norwegian water bubbles to logical positivism and naive realism. Human history, construction, and interpretation are ever present in tribal divination rituals, of course; but humans don't have much control over where the bone cracks appear or where the bubbles first rise. In this latter, very narrow sense, the experiment

allows a role for nature to speak. To be sure, the very idea of allowing nature any role at all in science will still be objectionable to some radical critics. Such differences are grist for the mill. But tribal divination rituals can hardly be fairly characterized as naive realism or as logical positivism.

Campbell (1982) also describes experiments as arguments, not demonstrations of fact:

A dialectical perspective does more justice to the history of experimental physics than does an image of the experiment as a window through which nature is seen directly. At each stage the “experimental variables” and the “outcome measures” are never “defined” for out-of-context or all-context meaningfulness. Instead, they are historically and dialectically indexical, acquiring their transient meaningfulness in the context of previous experiments and theories. In this important sense, *experiments are arguments* in a historical dialectic for the physical sciences and perhaps potentially for the applied social sciences. (pp. 120–121; emphasis in original)

It is difficult to see how such arguments could be fairly characterized as logical positivist or naive realism.

COMMON ERROR #11: QUANTUM PHYSICS SHOWS THERE IS NO REALITY

Certain findings from modern quantum physics are sometimes cited as evidence that there is no physical reality. Indeed, such a position is characterized by some evaluation theorists as one that even physicists endorse. This is misleading at best. Quantum mechanics has challenged some of our traditional understandings of reality, but certainly has not led most physicists to deny there is a reality. In fact, contemporary physicists say something quite different:

Most physicists do not pursue the logic of the quantum theory to the ultimate extreme. They tacitly assume that somewhere, at some level between atoms and Geiger counters, quantum physics somehow ‘turns into’ classical physics, in which the independent reality of tables, chairs and moons is never doubted. (Davies & Brown, 1986, p. 31)

According to quantum mechanics, it is now thought to be wrong to speak of individual particles as if they were unique and discrete entities in the manner of, say, rocks or tables. Also, at the level of such entities as electrons our observations do interfere with and perhaps even help create the particular form that the entity will take — from within a very limited number of possible forms that are usually known ahead of time in general form. But physicists do not view these molecular entities as created from nothing but one’s will, nor do they believe that people can somehow create anything at all — say an egg or a dictionary — from whatever gives rise to an electron.

What they do believe is something like this:

Quantum mechanics implies a world in which individual particles of matter do not really exist in their own right as primary entities. Instead, only the collection of all particles treated as a whole, including those that go to make up the measuring apparatus, has the status of ‘reality’. (Davies, 1984, p. 48)

Quantum mechanics does not deny reality; rather, it denies an isomorphism between reality in quantum mechanics with reality as understood in Newtonian mechanics. Moreover, this denial applies to the particle level in quantum mechanics, not to the macro-level world in which humans have evolved. While the results of quantum mechanics should caution us against accepting any simple versions of reality, it certainly does not require us to accept the position that all reality is created by the perceiver.

Finally, nothing about this view from modern physics is more consistent with qualitative approaches than with quantitative approaches. After all, evaluators rarely evaluate physical particles, so the connection between arguments about physical particles and evaluations has a great deal of superficial implausibility. In fact, it is particularly ironic that some qualitative theorists generalize from particle physics to social programs while simultaneously maintaining that there is no such thing as generalization. But if we are to make that generalization, then we must remember that it was the use of quantitative methods in physics that was partly responsible for identifying such phenomena as Heisenberg’s uncertainty principle and Schrodinger’s cat in the first place. The success of those methods in physics would seem to support the continued validity of quantitative methods in evaluation, as well.

COMMON ERROR #12: THE SOCIAL SCIENCES ARE RADICALLY DISCONTINUOUS FROM THE NATURAL SCIENCES

Critics of quantitative methodologies often claim that quantitative methods may be appropriate for the natural sciences but are inappropriate for the social sciences because the latter are qualitatively different from the former in kind. Indeed, the claim is sometimes made that the social sciences are not really sciences at all but rather a part of the humanities more akin to, say, history than biology. One argument in support of such a position is that the social sciences fail to yield the kind of consistent, predictable results that the natural sciences yield. But Meehl (1986) notes many social science findings are just as predictable as natural science findings:

Skinner points out in *The Behavior of Organisms* (1938, 41, 419) that the curves obtained from a single organism in the operant conditioning chamber (“Skinner box”) are smoother and more reproducible than many of the curves obtained by

medical students in their introductory physiology lab course. The verbal report of a sophomore, experiencing for the first time a negative afterimage, is reproducible enough so that you can afford to bet \$10,000 at 100-to-1 odds that a subject pretested for having normal color vision and not insane will report that what he sees after presentation of a red circle and being asked to fixate a distant gray wall is a large, faded blue-green circle. (p. 316)

Conversely, it is easy for social scientists to overestimate the consistency of findings in the natural sciences. Hedges' (1987) used meta-analysis to compare reviews in physical science (including physics) with those in social science. He found that "the results of physical experiments may not be strikingly more consistent than those of social or behavioral experiments. The data suggest that even the results of physical experiments may not be cumulative in the absolute sense by statistical criteria" (p. 443).

Similarly, the socially constructed nature of social science knowledge is often cited as an essential differentiating feature of the social sciences. But the social construction of knowledge in the physical sciences has been a dominant theme in modern science studies of all sorts (Shadish & Fuller, 1994). Philosopher Patrick Heelan (1983), for example, asserts that constructed interpretations of visual images are at the heart of many measuring devices in natural science:

Visual perception — and by analogy, all perception — is hermeneutic as well as causal: it responds to structures in the flow of optical energy, but the character of its response is hermeneutical, that is, it has the capacity to 'read' the appropriate optical structures in the World ('texts'), and to form perceptual judgments of the World about which these 'speak'. (pp. 181–182)

In at least some important respects, then, the physical sciences are just as social as the social sciences.

Another claimed discontinuity is that the social sciences study phenomena that are more complex than those studied in the natural sciences. But is this really so? Relativity theory and quantum mechanics are truly complex and difficult theories to appreciate; and physical reality is correspondingly complex in many ways. What theories in the social sciences are equally complex and difficult to grasp fully? Might it be that occurrences at the quantum level are as complex as any social or behavioral events?

This is not to deny that important differences do exist between social and physical sciences. The point is simply that important similarities exist, too, similarities that often tend to be ignored in the rush to justify a new methodology. By overstating the case for the differences between social and natural science, we become vulnerable to counterarguments. For instance, we might claim that because social sciences are radically different from natural sciences, social sciences need a radically different methodology. But when we are forced to acknowledge the similarities

between the two, that claim becomes vulnerable. Better to ground our claims in more defensible arguments.

COMMON ERROR #13: VEXING PHILOSOPHICAL PROBLEMS REQUIRE RADICAL SOLUTIONS

Every philosophy of science has its problems. Those problems need to be addressed, so that we should never be satisfied with the status quo. And sometimes those problems are so many and so severe that they cause us to reject the philosophy as inadequate, as happened with logical positivism about 50 years ago. But most philosophical problems simply reflect our imperfect understanding of the world in which we live, and the inevitable limitations of any single philosophical or methodological approach to science. For example, a belief in ontological realism is admittedly difficult to justify in a strictly rational way because so many of its key assumptions are virtually impossible to test. Similarly, philosophers still don't agree on what constitutes the logic of causal inference. Methodologically, experiments do intervene into the natural course of things, and many times that is not desirable or even of interest. These problems admittedly cause us to have philosophical headaches, and they admit to no easy solutions. But the mere existence of difficult problems does not require that we start from scratch, abandoning every older concept for radically new and different ideas alleged to be in the "vanguard" of current thinking. As philosopher Jerry Fodor (1986) recently observed:

It is a curiosity of the philosophical temperament, this passion for radical solutions. Do you feel a little twinge in your epistemology? Absolute skepticism is the thing to try. Has the logic of confirmation got you down? Probably physics is a fiction. Worried about individuating objects? Don't let anything in but sets. Nobody has suggested that the way out of the Liar paradox is to give up talking, but I expect it's only a matter of time. Apparently the rule is: if aspirin doesn't work, try cutting off your head. (p. 1)

Fodor goes on to call such extreme overreactions "grotesque." I agree. If history has taught us one thing, it is that every philosophy of science has its problems. The newer the philosophy, the less apparent those problems may be; but they will become evident with study over time. Anyone who tells you otherwise isn't telling you the whole truth.

DISCUSSION

I have stressed throughout this article that these 13 "common errors" are erroneous because they are incorrect portrayals of modern philosophy of science. But many are

also errors in a second sense. They attribute naive philosophical positions to quantitative researchers — for example, quantitative researchers are logical positivists who don't know logical positivism has died, or quantitative researchers believe one can directly perceive reality and produce unbiased reports of it, or quantitative researchers have a billiard ball model of causation. But we have no reason to think that quantitative researchers are so naive. Reichardt and Rallis (1994) make this same point, that quantitative researchers who write about such matters tend to have sophisticated understandings of philosophy of science. Of course, sophisticated quantitative understandings might still differ from sophisticated qualitative understandings of the philosophical issues involved. But it is those understandings that we should be debating, not logical positivism or billiard ball causation.

Are these 13 errors important? It depends. When an author makes just one error, when that error is not crucially placed in the logic of the argument, and when the rest of the argument is reasonably accurate, then the error may have minimal logical importance (even though it might have greater rhetorical importance). It may simply reflect on the author's reputation as an amateur philosopher, something the author may not care much about anyway. Undoubtedly many instances of these errors in the quantitative–qualitative debate are of this less important kind. An author may, for example, refer to quantitative evaluators as logical positivists, tossing the term into the argument in passing as a rhetorical device, never really using it in the structure of the argument. In such cases, while we might hope for more informed debate, the main harm here is merely that such errors may distract us from the more important issues. Hence it would save us all time and cognitive effort if we could eliminate these minor errors from the relevant debate.

At the opposite extreme, it is not difficult to find well-known cases in the quantitative–qualitative debate in which many or even all of these errors occur together. In these cases, the worth of the philosophical part of the argument falls apart proportionately. Perhaps the worst of these compound conceptual errors occurs in what one might call the “paradigm” argument. That argument goes something like this: Qualitative methods (or whatever it is the author is arguing for) are based on a “new paradigm” that radically revises our assumptions about the world, about knowledge, and about doing science. This new paradigm is usually contrasted to a “traditional paradigm” that it is intended to replace — lock, stock and barrel. That traditional paradigm is said to be based almost entirely on wrong assumptions, outdated philosophies, flawed concepts, and problematic methods. For example, the traditional paradigm errs in being based on logical positivism, in talking about causation, in believing in realism, in thinking that experiments reveal nature's truth, and in ignoring both Heisenberg's uncertainty principle and Schrodinger's cat. The argument then usually proceeds to

claim that nothing short of a Kuhnian revolution will do to remedy this situation, and so a new paradigm is proposed. That new paradigm is allegedly based on the very best vanguard thinking in philosophy, physics, and every other field where right-thinkers have seen the light; it apparently has no significant problems; and it fortuitously matches the author's preferences very closely.

Even when it is factually or logically wrong, the paradigm argument retains considerable rhetorical power to silence opponents. After all, no one wants to be seen as left behind in an outdated paradigm while the rest of the vanguard moves ahead. Further, the paradigm argument introduces a new jargon that one must know in order to be a part of the debate. For example, if you are unfamiliar with the work referred to by Schrodinger's cat or Heisenberg's uncertainty principle, and or with the definition of logical positivism — and few evaluators really are familiar with these things — it is nearly impossible to argue on level ground with those who do. You must trust that they are presenting the arguments fairly and completely. But as I have tried to show in this article, one cannot assume that is the case. Yet the rhetorical power tends to hide the errors.

Besides the errors, there is another reason why we might hope the paradigm argument would simply go away — today there is no “traditional paradigm” in evaluation, and there probably hasn't been for close to 20 years. Arguably, we may have had a dominant experimental paradigm for a brief period of time during the late 1960s and early 1970s, if you ignore things like most of Michael Scriven's and Lee Cronbach's works of that era, the achievement testing model in education, or econometric modeling in economics. But by the 1970s in mainstream evaluation, diversity of theory and method was the rule of the day as the weaknesses of the experimental model became apparent (Shadish et al., 1991). Glass and Ellett (1980) noted this diversity in their conclusion about the nature of evaluation in the 1970s — “People currently are saying it is many different things” (p. 211). In such a context, to argue today against the “traditional paradigm” is to be woefully out of touch with what has happened in evaluation since its modern inception.

Furthermore, to juxtapose qualitative methods against the “traditional paradigm” is to make one more mistake. The mistake is to think that today qualitative methods are the newcomers, the outsiders, the Young Turks bucking the establishment quantitative methods. Years ago all these points were well taken. Years ago, qualitative researchers had a difficult struggle in getting to the point they are today. Years ago, they had to fight for respectability against a quantitative establishment. We must respect the persistence and courage it took to engage in this struggle. But today, qualitative methods are no longer a new phenomenon. If we are to date the introduction of qualitative and quantitative methods into evaluation by some of the earliest published advocacies of them, qualitative methods

have been around nearly as long (Campbell, 1975; Stake, 1975a, 1975b; Stake & Gjerde, 1974) as quantitative methods (Campbell, 1969; Rossi, 1971). The preparation of a handbook on a topic is usually taken as another marker of the maturing of an accepted methodology, and qualitative methods have that, too (Denzin & Lincoln, 1994). Today, qualitative methods are included in evaluation texts; qualitative researchers participate in grant reviews and funding; they receive awards from evaluation associations; and they are elected to all the major offices of evaluation associations. It isn't clear when qualitative methods should be thought to have passed into the "traditional" repertoire; but if they haven't done so already, they're getting awfully close. Welcome to the establishment!

CONCLUSION

In the face of all these errors, isn't the implied conclusion that there is something wrong with using qualitative methods in evaluation? Definitely not. Long ago in evaluation, both qualitative and quantitative theorists (and those in between) seemed to reach agreement that the methods we choose to use in evaluation do not depend in any direct or deterministic way on the philosophy of science we happen to endorse (Guba & Lincoln, 1982; Reichardt & Cook, 1979; Smith, 1986). In fact, it seems more likely that in many (perhaps most) cases the methods preceeded the philosophy, and we added the philosophy as we tried to understand, explain, and justify why we chose the methods we did. After all, methods such as the experiment and the case study have been used in science for centuries, sometimes changing in the details, but always surviving the passing fits and fads of changing philosophies of science.

Moreover, we have many other good reasons for wanting to use qualitative methods in evaluation. For example, they can provide rich detail about whatever is being evaluated, can help generate new hypotheses to be investigated both quantitatively and qualitatively, can reflect the idiosyncratic constructions and worldviews of each individual in far more complete detail; and they are also enjoyable to read, and easy to relate to current issues in the reader's work and life. Qualitative theorists present such reasons in far greater detail and quantity than I could hope to in this article. But these good reasons do not include the errors outlined in this article. Those errors are at best a distraction, and are at worst downright harmful to progress in the qualitative-quantitative debate.

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