

Bitter Pills and Grim Reapers: A Commentary on Barrow and Phillips

Ronald W. Marx
simon fraser university

Undeniably, empirical research in education can be greatly improved. In recent years, many educational scholars and practitioners have commented on the problems of this body of research. Criticisms of the efforts of educational researchers are not new, and they range from attacks on technical grounds to assaults on fundamental questions. Much of the currently vigorous debate about empirical research in education, often written by researchers, is concerned with the distinction between qualitative and quantitative methods of inquiry and the relative advantages of one or the other. Often this debate is misguided, with scholars claiming that their particular research methodology is better and that practitioners of the alternative methodology are committed to a blind orthodoxy. Equally often, writers argue from an apparent "rampant relativism" that all methods of inquiry are equally valuable and potentially compatible. The claim of compatibility can lead to unfortunate and premature closure of an interesting and valuable debate about important issues in educational research, as Smith and Heshusius (1986) have shown.

As one would expect, philosophers of education have participated in this debate. The empirical research community often pays no heed to the philosophers' contributions. It is rare indeed to find reference to philosophy of education work in journal articles devoted to reports of empirical research. I tested this conjecture by examining an issue of the *American Educational Research Journal*. I randomly picked the issue (21[3]) from recent volumes on my bookshelf. There were 16 articles in the issue, all of which reported empirical studies. The reference sections of these articles included a total of 372 citations, not one of which was for a philosophical article or book! However, many citations concerned research methodology and debates on questions of inquiry written by empirical researchers.

It is unfortunate that such a clear separation has grown between philosophers of education and educational scholars pursuing programs of empirical research. Some of the limitations of empirical research in education

no doubt stem from other similar gaps in communication across subdisciplines. For example, curriculum theorists have probably had the same impact on research in education as have philosophers of education. It is likely that some of the serious problems of educational research, particularly problems in conceptualization, would be overcome were these gaps to be broached.

The contributions by Phillips and Barrow are attempts to reduce the separation between researchers and philosophers. Both writers are scholars of education who have spent considerable energy in recent years to help inform the empirical research community of developments in philosophy of education. As well, both at times have taken on the role of critic of educational research.

The papers included here are somewhat different. Barrow pursues a critical appraisal of educational research, concluding with seven prescriptions to which, as he puts it, all educational research *must* measure up. Phillips, on the other hand, tries to unpack some of the claims made by the interpretive or hermeneutic school of research in education, which has gained considerable popularity in recent years. In the process, he takes the stance of a defender of the more traditional, quantitative approach to educational research. I address my comments to the Barrow paper first, followed by remarks on the Phillips contribution.

HAS THE MEDICINE BEEN APPROVED FOR HUMAN CONSUMPTION?

Most governments require new medicines to be tested before they permit distribution. The approval procedure and the testing that is required for approval provide important data. When physicians write prescriptions for patients, they have information available concerning the putative benefits of the therapeutic regime as well as the likelihood of the various side effects that accompany the use of a drug. Unfortunately, Barrow has not provided us with data from his tests, nor has he given us a list of contraindications for his seven prescriptions.

Barrow is a bit unfair at the start where he suggests that his colleagues faulted him for hosting a conference of philosophers who would discuss the future of educational research. Philosophers have every right to talk about research. Everyone in the community of educational scholarship has that right. Moreover, because educational research always has an ultimately applied goal, the whole communities of educational practitioners and policy makers have the right to talk about educational research. Researchers who do not recognize their visibility among these groups are naive indeed. Surely Barrow knows this, as do the colleagues to whom he refers. Of course, he is correct when he writes that it is not so clear who should count as a practitioner of research. (It is unfortunate that the word *research* has come to be

used mostly with reference to programs of empirical studies. If Barrow wants to be labelled a researcher, he is welcome to the title.) Most empirical studies are conducted by teams of researchers. Frequently, the senior member of the team participates only marginally in the data collection phase of the investigation. Smedslund (1986) likens this to doing research in education in the manner of astronomical research, where scientists have to view their phenomena of interest from great distances. As Smedslund puts it, why do educational researchers not try to get closer to their research participants instead of trying to distance themselves?

Barrow concludes his introduction by intimating that empiricists are not sufficiently sophisticated to tell the difference between questions of technical matters and questions of philosophy. I rather hate to admit it, but I think he is right. We are quite a barbarian bunch when it comes to matters of philosophy. Perhaps the cause of our deficiency comes from graduate training that only rarely equips us for philosophical dialogue beyond the usually gratuitous foundations course. As well, I suspect that part of this problem is that only in the past couple of decades have educational researchers developed theories that are of much use as guides for their empirical efforts. Crass empiricism without conceptual or theoretical guidance is likely to result in mindless programs of research, which is what happened with much of the research on teaching in the early years (Barrow, 1984; Winne & Marx, 1977).

Context is the first of the three features of educational research that Barrow argues empiricists must clarify in order to improve their efforts. As I understand the argument here, Barrow refers to the conceptual foundation upon which a particular study or program rests. He does not mean the same notion of context that environmental psychologists such as Gump (1980) refer to when they speak of the physical and psychological setting in which people live, work, and play. Barrow's notion, then, concerns a basic shared understanding of the field of education. Take, for example, one of his prototypical questions bearing on "a clear conception of the educational enterprise." The question is "What ought schools to be for?" This is a very good question and is, indeed, fundamental to the understanding of a piece of research. It is also a question that is rarely addressed in the report of a research study. But it is not a question that is readily answered. Indeed, it is likely that many more doctoral theses in the philosophy of education will be written on the topic. (It is also likely that many more professors of educational philosophy will be granted tenure on the basis of their work on the question!)

Barrow, of course, is right. These are important issues for educational scholars to consider and to debate. They are important for empiricists and nonempiricists alike. Nonetheless, Barrow's argument demonstrates a narrow view of the role observation plays in the development of empirically

tested educational theory. At the end of the section on context, Barrow writes: "We want to know not merely what measures were used to establish learning, progress, and achievement (technical matters) but also how those measures are conceptually related to educational success." Barrow divorces technical issues in empirical research from conceptual and theoretical issues in a rather strange way. Operationalization of constructs (a matter to which Barrow addresses himself later in the paper) cannot be separated from a clear understanding of what construct is operationalized. It is unfortunate that research is conducted without a good sense of the hypothetical constructs which are the important objective of study. But educational researchers have been concerned with the logic of operationalism for some time. In one of the classic works in the field of test validation, Cronbach (1971) makes it clear that the very process of validating observational measures lies at the heart of progress in science. Cronbach's position places technical and conceptual issues in the same realm of scholarly activity. It is impossible to make sense of a particular measure of, for example, achievement, aptitude, or interest without conceptualizing the domain that is being measured.

The section on *concepts* is mostly devoted to a discussion of operational definitions in research. Barrow's points are properly made, and the general line of argument is sound. As I have written, there can be no excuse for an absence of good conceptual work when a researcher is operationalizing constructs to be investigated. In this regard, I agree with Barrow. I think, however, that he overstates his case. Behaviourism in psychology and education met a timely death over the last decade or so because of its inability to deal adequately with mental life. After all, most educators—scholars and practitioners included—are more interested in the thoughts of students than in their behaviour. Behaviour is of interest primarily because it represents something more important that cannot be observed itself. A teacher is concerned with the amount of time a child spends on academic pursuits because that time represents motivation and an opportunity to learn, not because time occupied in such a manner is a useful educational goal in its own right. Thus, the objects of most educational research are hypothetical constructs mostly concerned with the mental life of learners. Because they are hypothetical constructs, they can only be studied by means of operational definitions. As Cronbach (1971) has argued, the investigation of the relationships among operationally defined constructs lies at the heart of empirical inquiry in education.

So we are left with a conundrum. The nature of mental life has been the topic of enormous debate throughout the history of Western thought. That debate is just as vigorous today. Recent developments in philosophy, education, psychology, cognitive science, and artificial intelligence have raised

many old questions along with new ones about the nature of thought and methods for the study of mental phenomena (see, for example, Haugeland, 1981). Thus, Barrow is demanding perhaps too much of empiricists in educational research when he enjoins us to articulate “a full, clear and coherent account of what constitutes an instance of something” and “to do this philosophical groundwork in the proper manner.” Barrow is calling for individuals to achieve a goal that entire traditions of scholarship have yet to accomplish, particularly since it is unclear what “a full...account” and “the proper manner” really mean.

The third feature of educational research that Barrow discusses is *content*. I agree entirely with the main point here—i.e., the educational enterprise involves teaching content. To act as if the content that is taught is of little consequence to a program of research is foolish. It is also the case that many researchers (myself included) have acted foolishly in the past! Barrow’s digression into what is and what is not a skill is perhaps a red herring. Barrow is not the first critic of a skills-based approach to the study of teaching and of teacher education based on this narrow approach. The competency-based teacher education movement was one of the first casualties of the death of behaviourism.

Before he reaches his conclusion, Barrow makes an unnecessary side step into questions of technical issues in education. The step is unfortunate, because it shows some of Barrow’s misunderstanding of empirical approaches to the study of education. As I have tried to make clear, validity is an issue central to educational research, not merely a minor topic concerning measurements. Barrow seems to think that validity is something other than an attempt to “get at the truth.” Indeed, getting at the truth is exactly what validity is all about. To use Barrow’s example, if a creativity test is not assessing “our complex notion of a truly creative individual,” then it is not a creativity test. Scores generated by the test will not have validity for claims about creativity. The problem is that it is not only the educational psychologists who have done a bad job of conceptualizing creativity but also everyone else. It is simple to say that a measure of creativity should identify the “truly creative individual.” It is yet another matter to gain consensus about the attributes of creativity and creative people.

Two other matters in the section of the paper on technical matters are disquieting: the discussion of sampling and the list of all the variables ever studied in psychology and sociology. Truly random samples and larger samples are not panaceas for empirical research. The need for random samples depends on the question that the researcher is asking, along with the experimental and statistical methodology used in the study. Conducting educational research is not like conducting agricultural research (the discipline that contributed the basic factorial research design to education). It is

not a simple matter to select a random sample of teachers or students from a population, include them in a study, and then act as if each responded equivalently to the treatment. Populations of people are not the same as populations of corn seed, regardless of the statistical model one is using. In contemporary research that conforms to proper guidelines for the ethical treatment of research participants and is conducted in real school settings, truly random samples are virtually impossible to obtain.

The issue of large samples is similar to the random sample issue. What does it mean to have “larger samples”? If a researcher is investigating the development of classroom task structures among expert mathematics teachers over the period of a year (Barrow’s third panacea was longitudinal studies), how many teachers would constitute a large enough sample? First, you have to find out who is considered to be an expert teacher. It is unlikely that large numbers would be identified in any locale. Second, you have to gain agreement to participate in the research study from the group of identified teachers. (The reader will have noticed that the idea of “a truly random sample” will have had to be given up for this study.) Third, the principal investigator should spend enough time with the teachers to understand the phenomena that are the topics of investigation (remember Smedslund’s advice). It is likely that four or five teachers would constitute a large sample in this instance. Is that large enough? If not, then the research staff would have to be expanded, and possibly the study would have to be conducted at more than one location. The budget for such a study would be huge.

The point here is that conducting empirical studies in education requires dozens, perhaps hundreds, of compromises between various conceptual and technical options. A dispassionate critique of the researcher’s decision may show that the study is flawed. Without an understanding of such decisions, the empirical enterprise could seem arbitrary.

If one added to this stew the requirement that “all relevant variables [should be] taken account of,” then it would literally be impossible to conduct any empirical studies in education. It is hard to find the basis for Barrow’s argument here. The design and execution of an empirical study requires a balance between decisions concerning internal and external validity. In particular, if the researcher has had the luxury of random selection and random assignment of participants to treatment groups and if the sample size is relatively large (remember Barrow’s panaceas), then it is highly likely that individual differences among the participants will be equally distributed among the groups in the study. The design of the study is then used to control the major variables of interest. To take into account all the relevant variables would make the research design gargantuan, beyond the scope of even the most elegant fractional factorial design.

On balance, it is hard to argue with the general tone of Barrow’s comments. His advice to empiricists echoes certain popular recommendations.

Indeed, my first reaction to the paper was that, in it, Barrow suggests that educational research ought to be done well, not poorly. In particular, he wants empiricists to be more conceptual and theoretical. It is hard to argue against such counsel. But what his advice lacks is a sense of the nuance and craft of empirical research in education. As for whether educational research is to be taken seriously, I am certain that it already has been. For good or for ill, many teacher educators, practitioners, and policy makers have taken and continue to take educational research seriously. From Barrow's perspective, I would guess that this state of affairs is unfortunate, but it is the case nevertheless.

HAVE EDUCATIONAL RESEARCHERS HEEDED THE CALL OF THE GRIM REAPER?

In the last two decades or so, educational research has enjoyed fairly vigorous growth. The major international association representing educational researchers, the American Educational Research Association, has had a dramatic increase in membership. In Canada, the Canadian Society for the Study of Education and its research affiliate, the Canadian Educational Researchers Association, have experienced the same kind of growth over the last decade. With respect to research funding, the Social Sciences and Humanities Research Council of Canada has provided increased funds for educational research and has recently named education as an area for strategic research funding. There has been, of course, a refreshing and invigorating growth of pluralism in modes of inquiry. Correspondingly, there has been a reduction in the hegemony that psychology—particularly the more quantitative forms of psychological research—has had over methodology in educational research. But it is wrong to conclude from these developments that empirical studies in education are dying. Based on these observations, I would concur with Phillips' assessment of the power of the grim reaper over the educational research community.

In his paper, Phillips provides a useful service to those educational researchers who are unfamiliar with the philosophical roots of many of the current developments in qualitative methodology. The brief overview that constitutes the first half of the paper will no doubt introduce some fundamental concepts that provide the cornerstones for interpretive approaches to educational scholarship. The second half of the paper constitutes Phillips' critique of, as he puts it, the *core hermeneutical case*. From the point of view of an empirical researcher, I have little criticism of the paper. (I also know less about hermeneutics than perhaps I ought to. But then again, I may be one of the barbarians!) I have, however, some observations that were provoked by reading the paper.

I believe that Phillips is correct when he writes that "it ...seems incontrovertible that much action ...requires explicit interpretation." A large

body of educational research in the last couple of decades constitutes an attempt to do just this. Phillips provides examples of students' activities in classrooms that require interpretation in order to be understood. Examples could also be offered of teachers' actions. Teachers, in some sense, exert their influence upon students by making known their intentions. As Harré (1984) has shown, intentions are a realization of collective acts with social definitions. For someone to understand a teacher's intentions, that person would have to have been socialized in the community to which the teacher belongs or at least have extensive knowledge of the language, norms, and practices of the culture. Because virtually every educated person (including scholars of education) in Western culture has attended school, the meanings of teachers' actions seem clear. But this clarity is not so obvious to some young children who may be students in classrooms, as Winne and I (1982) have shown. An important goal of educational research, then, is to provide interpretations of these activities from the points of view of teachers and students.

Phillips goes on to examine the problem of the meaning and the significance of social practices. One must realize, of course, that the activities that constitute schooling are social practices which can be analyzed from the perspective of *authorship* as Phillips analyzes other social practices. The classroom recitation, for example, is a social practice that has been widely diffused and for which *an* author clearly cannot be identified. Hermeneutical questions—i.e., questions of the meaning of the activity—can nonetheless be asked of participants in classroom recitation. But Phillips is right when he claims that the question of the meaning of the activity is separable from the question of the significance of the activity, in so far as significance is taken to mean effect or consequence. What complicates matters is that the meaning of a social practice usually influences the effects of that practice on people. Cognitive theory and research in psychology and education are predicated on the role of meaning and interpretation as mediators of social events.

Phillips concludes his paper by contrasting hermeneutics with new approaches to social science, showing that the two are not as incompatible as one might expect. In particular, he discusses the role of the hypothetico-deductive method in hermeneutics. One of the more important issues involved in this contrast is the nature of evidence and procedures used to support claims of interpretation. How is it, for example, that one or another interpretation proposed by hermeneuticists can be judged to be better or worse? What evidence or rules of procedure can be used to solve interpretive debates? In considering these questions, Phillips comes close to arguing that hermeneutics is merely another branch of scientific inquiry.

This argument suggests an exaggerated and even dangerous compatibility (Smith and Heshusius, 1986). Hermeneuticists must chart a course between the hegemony of the quantitative school and the anarchy of complete relativism. The appeal to facts as states of nature, which derives from quantitative approaches to educational inquiry, can provide no safe haven for interpretive research. On the other hand, hermeneutics has to provide grounds for judging the worth of conflicting interpretations. It is simply not sufficient for hermeneuticists to argue that uninterpreted facts do not exist and that the value of a particular interpretation is based on the extent to which it is believable to the reader. If there are no foundations for knowledge, then we are "adrift in a world where nothing binds us, where there are no criteria of right or wrong, correct and incorrect, and where all is reduced to a clash of taste and opinion" (Smith & Heshusius, 1986, p. 10).

CONCLUSION

The future of educational research is bright. While there are undoubtedly problems, both technical and conceptual, empirical inquiry in education is robust and will continue to be so. The contributions by Barrow and Phillips add to the vigorous debate about research that has been gaining momentum in recent years. The immediate result of this debate is not an early death of the empirical approach to educational research. Rather, I think that empiricists, having been forced to defend their territory, are developing a new sophistication about their research methods and the philosophical foundations upon which these methods stand. The long-term benefits of this debate will most certainly accrue to the broader educational community, as educational scholarship becomes more robust and develops a more rigorous intellectual tradition.

REFERENCES

- Barrow, R. (1984). *Giving teaching back to teachers*. Brighton, Sussex: Wheatsheaf.
- Cronbach, L. J. (1971). *Test validation*. In R. L. Thorndike (Ed.), *Educational measurement* (2nd ed.) (pp. 443–507). Washington, DC: American Council on Education.
- Gump, P. V. (1980). The school as social situation. In M. R. Rozenzweig & L. W. Porter (Eds.), *Annual Review of Psychology*, 31 (pp. 553–582). Palo Alto, CA: Annual Reviews.
- Harré, R. (1984). *Personal being: A theory for individual psychology*. Cambridge: Harvard University Press.
- Haugeland, J. (Ed). (1981). *Mind design*. Cambridge: MIT Press.

- Smedslund, J. (1986, April). *Common sense psychology and educational research*. Paper presented at the meeting of the American Educational Research Association, San Francisco, California.
- Smith, J. K., & Heshusius, L. (1986). Closing down the conversation: The end of the quantitative-qualitative debate among educational inquirers. *Educational Researcher*, 15(1), 4–12.
- Winne, P. H., & Marx, R. W. (1977). Reconceptualizing research on teaching. *Journal of Educational Psychology*, 69, 668–678.
- Winne, P. H., & Marx, R. W. (1982). Students' and teachers' views of thinking processes for classroom learning. *Elementary School Journal*, 82, 493–518.
-

Ronald W. Marx is a professor in the Instructional Psychology Research Group, Faculty of Education, Simon Fraser University, Burnaby, British Columbia V5A 1S6.