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MENC HANDBOOK OF RESEARCH METHODOLOGIES

EDITED BY RICHARD COLWELL

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Richard Colwell

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Edited by
Richard Colwell

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Preface

Important projects require considerable cooperation and this book is no exception. Ms Kim Robinson and Eve Bachrach of Oxford University Press teamed with Mike Blakeslee and John Mahlmann of the National Association for Music Education—MENC to make this project possible. It was their idea and their inspiration that allowed me to be a part of this important undertaking.

The Handbook of Research on Music Teaching and Learning published in 1992 was possible because of the foresight of Maribeth Payne of Schirmer Books and John Mahlmann of the Music Educators National Conference. *The New Handbook of Research on Music Teaching and Learning* published in 2002 required even more cooperation from Payne and Robinson of Oxford and the music education conference. Both handbooks were immediate successes. Ms Robinson contacted Mike Blakeslee to explore ways that the material in the handbooks could be made more accessible to students, faculty, and libraries. Their solution was to identify material that was essential for all scholars in the profession and to make this material available in small, economical, publications.

It has been my pleasure to work with them and not only to have the responsibility of identifying the critical chapters but to work with the authors in updating the material to reflect events affecting the profession since the original publication. It should be of great interest to the profession to see which areas of research in music teaching and learning have changed significantly and which continue to be based upon fundamental philosophies and procedures. In seeking the best minds in the profession, it will come as no surprise that our authors are based in Great Britain and Canada as well as the United States. In two of the nine chapters we found it advisable to have co-authors from outside the profession thus allowing us to avoid the in-profession bias that often accompanies some research procedures.

The chapters are unique and can be read in any order. Bennett Reimer, however, sets the stage by identifying the research issues that require the attention of all scholar/researchers in the profession. Following his intro-

ductory material, our authors portray the essential knowledge one must have to understand historical, philosophical, assessment, qualitative and quantitative research. I am confident that this book will set a standard for publishing in many disciplines and it is noteworthy that Oxford and the National Association for Music Education have taken this leadership step.

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RICHARD COLWELL, professor emeritus of the University of Illinois and the New England Conservatory of Music, has long been involved with assessment and evaluation. In 1970, he published *An Evaluation of Music Teaching and Learning*. Beginning in the late 1960s and continuing into the 1980s he published 22 music achievement tests. He served as a consultant to the states of New York, Illinois, Indiana, and Minnesota in the development of their state music tests and is presently working in the same capacity with the Boston Public Schools. He was the editor of the first *Handbook of Music Teaching and Learning* and

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CAROL P. RICHARDSON is professor of music in music education at the School of Music, University of Michigan. She won a National Academy of Education/Spencer postdoctoral fellowship for her 1995 study of children's music-listening processes. Her most recent book is *Music Every Day: Transforming the Elementary Classroom*, with co-author Betty Atterbury. She is a 2004–2005 Fellow with the American Council on Education (ACE) mentored by Chancellor Nancy Cantor at Syracuse University and President Mary Sue Coleman at the University of Michigan.

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MENC Handbook of Research Methodologies

Toward a Philosophical Foundation for Music Education Research

1

BENNETT REIMER

This chapter explores several important issues that need to be addressed if a philosophical foundation for music education research is to be built. Implicit in this task are three presumptions: (1) that music education research is not at present and has not in the past been guided by foundational philosophical principles, (2) that it would be beneficial for the research enterprise if such principles were articulated and applied, and (3) that careful consideration of several key issues will be necessary if music education research is to be grounded in a coherent philosophical-epistemological perspective.

What is *not* offered here is a philosophy of music education research. Although I will not attempt to disguise whatever preferences and proclivities I hold, I will also not aim toward a particular resolution of the philosophical issues to be raised. It is my hope that sufficient debate about these (and other such) issues will lead interested and capable individuals to formulate philosophical principles that would guide our research efforts.

Because I will be discussing something that does not yet exist, the consequences of its absence, and the ways our work is likely to improve if we were to have it, I will naturally tend to focus on shortcomings within music education research. After all, if no shortcomings existed, there would be little reason to posit that we are in need of something we do not yet have. It is not particularly pleasant to set out to draw attention to weaknesses as a way of establishing that we have much room for improvement and to indicate some of the ways we need to improve. This is especially the case in a book of this sort, which to a large degree exists, correctly and aptly, to celebrate the achievements in one dimension of music education research. That such achievements have been considerable in music education research in general is admirable, given that this field has a very short research history because it lies outside those disciplines in which research is the central or at

least a major defining activity. That its research endeavor has grown so rapidly, has mastered so many of the complexities of the activity, has developed so many highly competent specialists, has established training programs for preparing new recruits, and has developed a large, wide-ranging literature is ample testimony that high levels of success have been achieved. Yet it can also be argued that the continuing viability of music education research will depend on significant foundational improvements. This chapter suggests what these might be and how they may be achieved.

The Lack of a Philosophical Grounding for Music Education Research

From among the many ways the term *philosophy* can be construed, I focus here on its meaning as "a system of principles for guidance in practical affairs" (*Random House Dictionary*). The term *system* implies that the principles be ordered according to a set of beliefs that achieves a convincing level of consistency and validity.

Philosophical principles, to be valid and useful, cannot simply be a random collection of assumptions. A unifying core of precepts, sufficiently congruent to provide coherence, sufficiently broad to cover the scope of the enterprise, and sufficiently in consonance with what is accepted as well founded according to the criteria established by the community in question, is necessary for a convincing and useful set of philosophical guidelines to exist.

The term *principles* refers to a particular level of mental operation. Principles provide general rules, laws, or guidelines from which specific actions or beliefs might logically spring. As generalities that capture the determining characteristics or essential qualities of a phenomenon or activity, principles provide the nexus for consistent doing and being. Without a set of principles for guidance, practical affairs can be only accidental, lacking the unity of purpose that is required for effectiveness.

Music education research is an enterprise employing disciplined inquiries¹ in an attempt to understand and improve the teaching and learning of music. It has been undertaken, I suggest, without a sufficient level of grounding in a coherent system of guiding principles. With the advent of the initial *Handbook of Research on Music Teaching and Learning* and *The New Handbook of Research on Music Teaching and Learning*, a few substantial, far-reaching explorations of foundational issues relating to music education research have been produced.² Few other discussions exist in the general music education literature about the basic questions that must be grappled with for a set of sound and useful principles to emerge. We find in various articles and in the well-established music education research textbooks a heavy weighting toward a particular (positivist) conception of science as the basis for the endeavor. Issues are seldom raised as to what is valid music education research;

how music education research should be organized and conducted; who should do music education research; what science means; how science has radically questioned its own nature during the twentieth century; the uncertain relationship of the physical and biological sciences with the so-called social sciences; the uncertain relationship of the physical, biological, and social sciences with the domain of art; the vexing dilemmas of the relation of basic research to applied research; and a host of questions about the compatibility of education as a social-political endeavor to the particular model of scientific research that music education has tended to adopt uncritically as its *modus operandi*.³ This is not to say that the quantitative, positivist definition of science and research that has dominated the history of music education research until only recently is, *ipso facto*, mistaken or misguided. It is to say that we have been mistaken and misguided not to have examined, carefully, critically, and continually since its inception, how and why and when such a definition might be or might not be appropriate for our research purposes. I am not questioning here the substantive issue of the adequacy of positivistic science as a basis for music education research. (Under the section "Several Key Issues . . .," I later return to this issue in some detail.) I am raising the question of our historical need, and our failure, to think about music education research at a metacognitive level. That is the level from which principles could emerge that might have helped our research become more efficacious. It is important to think at that level. We have not yet, I suggest, sufficiently engaged in professional discussions about the basic issue of what scientific truth might mean and not mean. We have not yet adapted our research practices to be in accordance with a more thoughtful grounding for them. Therefore, we remain uncomfortably mired in the traditions established in the earlier years of our research endeavor.

By contrast, we have traditionally thought a great deal about the various modes or methodologies by which music education research might be carried on. Few articles or textbooks on research neglected to discuss the differences among types of research, such as philosophical, historical, descriptive, experimental, and variations thereof. Perhaps the most inclusive treatment was provided by Robert Sidnell, who, after reviewing several classifications, proposed a three-dimensional matrix including methods of inquiry (historical, descriptive, experimental, philosophic), central variables (the teacher, the learner, the interaction of teacher and learner, content, and environment), and disciplines (education, musicology, psychology, sociology, anthropology, history).⁴ Our substantial interest in types of research (there is far less discussion of central variables or disciplines) reflects the characteristic focus by music educators on issues of methodology. In every aspect of music education, from the most practical to the most theoretical, we have historically been fascinated by (if not fixated on) methodological concerns. This may stem, in part, from our need to demonstrate our capacity to be scholarly, but it is also likely to be a result of our concentration, from the early colonies to the present, on the teaching of performance, with all the attendant needs for regularity, careful sequencing, technical finesse, and constant monitoring

and assessing. Such requirements and the remarkable success the profession has achieved in meeting them raise methodological issues to high levels of consciousness and inevitably transfer to endeavors not directly related to performance, such as research.

Therefore, discussions of the various research types or modes have focused largely on the methods and techniques by which they should properly be carried out. Given the long-standing dominance of quantitative research, the great number and intricacy of technical details related to it, whether descriptive or experimental or correlational, and the special languages, computations, and symbolic representations they require, major attention has been and is now given in the music education research literature and in research courses to their methodological particulars. Such (necessary) attention to detail fits well not only with the positivist and quantitative bent mentioned previously but also with the seemingly natural predilections of many music educators.

Little similar attention has been paid to philosophical issues related to the various research methodologies. It is generally agreed that all of them are necessary, but questions of why, and in what ways, have seldom received more than cursory treatment. A step toward principles was taken by Charles Leonhard and Richard J. Colwell in their 1976 review of research and projections for the future, by their suggestion that in order to achieve better clarity about significant research topics, philosophers and scientists will have to collaborate.⁵ But we have not built on this suggestion by trying to define what the characteristics of significant research topics might be, whether the research types we have traditionally identified are relevant to or sufficient for dealing with such topics, how each type of research might be expected to contribute toward useful knowledge, how and for what purposes each type (including those more recently identified) might collaborate or interact with the others, whether particular types may be incompatible with one or more of the others in the context of some topics, and whether combining two or more types might yield insights larger than the sum of the parts included. Lacking examination of these issues, we cannot simply assume that so long as we have various types of research being undertaken, we are doing our work responsibly. We need to attend to the principles lurking beneath the surface of our previous, largely technological discussions of the ways research can be conducted, by focusing on issues such as (1) what each type allows us to know, (2) what good such knowings are, and (3) how our knowings might be enhanced by combinations and juxtapositions currently not attempted because of our limited understanding of which dimensions and dynamics of music education each type can be expected to clarify.

Few generalizations would seem more self-evident than that different types or modes of research yield different pictures of reality. In addition to being clearer about how that occurs so we can exercise more intelligent control over it, we also need to be clearer about what realities we are interested in exploring through research. Little sustained discussion exists in the music education research literature of the issue of what it is we need to

know in order to improve music education. The Leonhard and Colwell article mentioned previously attempts to suggest a set of "major research questions," and other attempts have been made over the years to delineate topics that might drive the research enterprise.⁶ The most frequent way such topics are suggested, however, is through the "Recommendations for Further Research" sections of doctoral dissertations and other studies, but these are generally limited to extensions of the particular topic of the dissertation or study. No mechanism exists to gather, coordinate, and prioritize the many recommendations made. Further, such recommendations are *ex post facto*—they suggest follow-ups to topics that were chosen without the guidance of an overarching plan leading to that specific research effort. No such plan exists because no widely adopted philosophical principles for music education research exist to provide a foundation for such planning.

One more issue should be mentioned regarding the lack of philosophical guidelines for music education research. To what degree do we expect music education research to relate to, influence, or in any way be connected with practices of teaching and learning music? We often give strong indications that we expect research to have practical consequences, as in our attempts to translate research results into language nonresearchers can understand and to make these user-friendly reports available in a variety of ways. This is under the assumption that research frequently is or should be applicable to practice. That assumption has often been questioned. The general literature on educational research reflects an intense examination of whether and how research relates to schooling and why it often does not, an examination carried on with particular energy in our sister field of art education. We have not paid similar attention to the theoretical issues of why research in music education seems to have such little relevance for the great majority of music teachers. This has been noted outside our own field, as in the comment by Beverly Jones and June McFee in the *Handbook of Research on Teaching* (3rd edition): "The controversy regarding separation of research from practice which is pervasive in art education is conspicuously absent in the literature of music education."⁷

I return to this issue in my discussion of the question of who should do research. The point here is that a carefully devised set of principles for music education research would offer guidance as to whether and when we should expect practical payoffs from research and how such payoffs might be achieved. We do not at present have such guidance available to us, accounting in large part for our disorganization as to how we approach the conduct and application of research. Such disorganization, ironically, is quite atypical of music education as a whole. Why, then, can it be argued, as I believe it validly can be, that music education research, which should be characterized by thoughtful, effective structures within which its diverse activities can be generated and carried on coherently, is largely devoid of such structures, all existing structures being *ex post facto*? The answer lies, to a large degree, in the lack of a solid foundation on which a research structure can be built.

The Need for a Philosophical Foundation for Music Education Research

The discussion in the preceding section focused on several important factors demonstrating that we have carried out our research endeavors in the absence of guiding principles. We have not attempted to define sufficiently what we mean by science, what we can and cannot expect from science, and how we can utilize science to help us with the problems we think are important. Therefore, we cannot exercise optimum control over how we engage in science in our research endeavors. Instead, we tend to "do science" in ways only vaguely related to a definition of science that is itself quite vague.

There is a historical basis for this situation. A good deal of music education research in the past and continuing to the present has been influenced by the assumptions of behavioristic psychology, which is the paradigm case in the human sciences of positivism as it has existed in the physical and biological sciences. There is a tendency to regard such research as being the very model of science, and those who have done it most and best as being our most "scientific" researchers. We have not discussed whether this particular model is (1) viable within the larger fields of philosophy of science, psychology, and educational research; (2) pertinent for the needs of music education; and (3) supportive of values we hold for both music and education. If we had discussed the issue with some thoroughness and rigor, we would have discovered that (1) behavioristic assumptions were being severely questioned in both philosophy and psychology at the very time we began adopting them as the basis for much of our own research, (2) they do offer important insights and guidelines for certain aspects of music education, and (3) they do support certain values we tend to hold but are inimical to others.

What difference would it have made if we had achieved a reasonable level of clarity about such matters through our ongoing discussions of them? Perhaps we would have been able to use behaviorism more insightfully and powerfully, taking advantage of what it can do very well from the perspective of what it cannot do very well. Perhaps we would have been better aware that other models from psychology were and are viable for our research and could have pursued them with the energy they deserved, achieving a balance in psychological orientations more relevant to the diverse nature of music education than we otherwise were able to achieve. We would have been able, perhaps, to recognize the importance of behavioristic research in light of its particular strengths while also being cognizant of its inherent weaknesses. In short, our philosophical-theoretical groundings could have made our research endeavors more sensible.

We are now in a new era in psychology with almost wholesale abandonment of the interest in and the credibility of behaviorism and the rise of cognitive psychology along with the broader domain of cognitive science, and we are beginning to see this change reflected to some degree in music