

Forum

Harry E. Price, Editor
Journal of Research in Music Education

This being my first "Forum," I would like to say that I was fortunate to serve on the Editorial Committee under two exemplary models I hope to emulate, Jack Taylor and Rudolf Radocy. They provided excellent direction and helped enhance the *Journal of Research in Music Education*. Dr. Radocy and I spent several informative and productive days going over journal procedures in an effort to make the transition of Editorial Committee Chairperson as seamless as possible, both for committee members and for individuals who have submitted manuscripts for review.

It is out of respect for our community and the ideas for which this flagship journal of our discipline stands that I promise complete neutrality and discretion regarding all submissions. All manuscripts will continue to be treated impartially, regardless of subject matter and mode of inquiry. In Dr. Radocy's final "Forum" (Summer 1994), he stated that the journal has established that it is not "biased for or against any particular approach to research," and this will be maintained. The issues under consideration should, and will, deal only with the quality of the research and not the type; good research is good research. Also, be assured that no confidences, to which only the Chair is privy as a result of the normal functions of the position, will ever be divulged. To my knowledge, never has information such as the names of reviewers or authors(s) of a manuscript been divulged, nor have results of reviews ever been shared with anyone except the author(s).

The strength of the *JRME* rests not only on the many fine manuscripts submitted, but on an exceptionally qualified and committed editorial committee. These people give unselfishly of their time, with little extrinsic reward. The new members are Martin Bergee, Ruth Brittin, Don Coffman, Julia Koza, and Jan McCrary.

Following tradition, this issue presents research of a high standard. It is also special because it contains the 1994 Senior Researcher Award acceptance address. The recipient, Dr. James C. Carlsen, is Professor Emeritus of Music at the University of Washington and a past Chairman of the *JRME* Editorial Committee. He has served on and chaired the Executive Committee of the Music Education Research Council, and helped to found the Research Commission of the International Society for Music Education. Professor Carlsen is a true gentleman who has served the research community with distinction, and his words speak for themselves.

James C. Carlsen is the recipient of the MENC 1994 Senior Researcher Award. The following speech was presented on April 8, 1994, at a special session of the Society for Research in Music Education at MENC's National Biennial In-Service Conference held in Cincinnati, Ohio.

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"The Need to Know": 1994 Senior Researcher Award Acceptance Address

Let me begin by thanking those who placed my name in nomination for this award, as well as the Music Education Research Council and the National Executive Board of the Music Educators National Conference for the honor they have bestowed upon me. One does not develop in any arena of activity without the inspiration of others. For that, I am grateful to my colleagues and students over the years, who have challenged my ideas and, as a result, have caused me to think in ever-new ways.

Certainly, some people stand out as major players in my development. One in particular is the late William Bergsma, who, as director of the School of Music at the University of Washington in the 1960s and early 1970s, understood that the musician's *need to know* could not be satisfied unless appropriate means for acquiring knowledge were facilitated in the academic community. He had the vision to inaugurate a program of systematic research intended to cut across all divisions of music and to serve as a complement to the more traditional historical and theoretic/analytic research programs already in place. I dedicate my remarks today to his memory and to his vision.

Over the years that I have been involved in music research, like you, I have observed a variety of motivations that have driven individuals' research. One such motivation has been an RFP (Request for Proposals) from some funding agency. Another is a graduate requirement to complete a thesis or dissertation. Another is the

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requirement that people seeking tenure or promotion either "publish or perish." Still another I call the hammer syndrome. It has been said that if the only tool you have is a hammer, then everything becomes a nail. In that sense, if a researcher has only one methodology, all problems are submitted to it. It is this latter motivation of pursuing a methodology that concerns me and serves as a theme in my comments today.

Forty-two years ago, the first issue of the *Journal of Research in Music Education (JRME)* was printed, and from its very beginning, that publication has revealed the breadth of research interests of music educators and the disparate methodologies those researchers have used. That first issue included a quantitative analysis of band programs, a historical piece on university music study in the Middle Ages, a historical study on the first shape-note tune book (*The Easy Instructor*), and yet a third historical study on the flute and its music. It also contained a bibliographic/theoretic treatise on musical ability, a survey on administrative policies of the university band, and a survey on musical experiences necessary in the training of elementary school teachers.

That distribution pattern of content in the *JRME* has changed over the years, probably reflecting the developing interests of the researchers involved. With it, the editorial policy and the publication style of the *JRME* also changed over time in an attempt to resonate with those interests.¹ In spite of these shifting patterns, the diversity of topics and methodologies found in the first volume is also evident in the most recent volume. I underscore this diversity in my opening remarks because I believe that those who are engaged in music research and those who are to assume responsibility for the preparation of future researchers must understand both the nature of that diversity and its importance for our profession.

One way that our profession has acknowledged this diversity is through the conferences and workshops that have been mounted by different groups to train researchers in one methodology or another. If I am not mistaken, the first was the training session on experimental design in 1968. Since then, research methodologies have been the focal point of several panels at Conference meetings. Methods of philosophical research in music education have been addressed at two recent conferences, and in May 1994 a conference on qualitative methodologies for music education researchers will take place. If participants gain increased understanding of the way that a particular methodology complements other research methods, as well as obtain increased skill in the appropriate application of that method, our profession will have been well served.

Regrettably, the complementarity of the different research methods has not had the emphasis that it deserves. If our emphases on methodologies send the signal that there is some natural hierarchy of methodologies, in and of themselves, we send the wrong signal; it just isn't true. Furthermore, if we allow ourselves to fall into such a trap, we exacerbate our tenuous relationships within the research

community and inhibit the opportunity to move cooperatively in our quest for knowledge. Such signals are transmitted to our students, who then categorize themselves early on as experimentalists, or naturalistic researchers, or ethnographers, or historical researchers, or what have you.

As researchers, we do need to know the different ways of knowing, and we do need to know when each is appropriate. Nonetheless, I am convinced that an introduction to research through research methodology is not the appropriate foundation for the training and nurturing of future researchers. Instead, I contend that, in the initial stages of development, a budding researcher must examine four philosophical questions: What is research? Why do research? What is it that we need to know? and What does it mean?

What Is Research?

One industry's motto has been: "Research is our most important product." We ourselves have been known to suggest that "research is the answer." In fact, research is neither a product nor an answer; it is a process, the outcome of which should be new knowledge.

The Random House Dictionary of the English Language (Stein, 1973) includes as one of its definitions of research the "diligent and systematic inquiry or investigation into a subject in order to discover or revise facts, theories, applications, etc." Roger Phelps (1980), in his second edition of *A Guide to Research in Music Education*, echoed this definition when he said that it was his "contention that research is a carefully organized procedure that can result in the discovery of new knowledge, the substantiation of previously held concepts, or the rejection of tenets that have been widely acclaimed" (p. 4). The emphasis on the word "discover" or "discovery" indicates *new knowledge*, the revealing of something that has not been seen or known before. If we want to learn what someone else already knows, the efficient way is to go to school, or to examine the available record of knowledge. The critical term here is *efficient*. This does not preclude the use of the discovery method to enable a person to learn what is already known. Although such a procedure may be effective and instructional in itself, and may even contribute to understandings not possible by other means, more direct approaches to authority would seem to be more efficient. But, if we want to learn what no one else knows, our *only* recourse is research. It is in this sense that I reserve my use of the term "research" for that process in which we learn what no one else knows.

Why Do Research?

Bennett Reimer reminded us 10 years ago in his provocative address to the Conference that the role of a researcher "is to crave knowledge" (Reimer, 1985, p. 21). Allen Britton, in his acceptance address to this body four years ago, said, "... research, whether sci-

entific or scholarly, should be pursued not in the hope of practical gain but for the joy of it, for the satisfaction of ascertaining and telling the truth about something" (Britton, 1991, p. 178). I like both of those statements, but I don't think that either precludes the possibility of identifiable and substantial motives for satisfying our need to know. I get joy out of doing research, but I doubt that there are many who can afford the luxury of "joy-seeking" as a motivation to undertake research. What seems to be more likely is an awareness of the many compelling problems for which no known answer is forthcoming.

One example: there are fewer orchestras per capita in our high schools today than there were 50 years ago. That is a fact, and many musicians consider that a problem. As validation of that statement, during the early years of the Second World War, my high school of 350 students in a small farming community had a full orchestra. Even during those war years, when many junior and senior boys were gone to the armed forces, we still had 30 or more students in the orchestra. Today, that high school, with an enrollment five times larger, has only a 25-piece orchestra, and that includes students from the middle schools as well as from the high school. This was true not only of small schools: During that same period in Seattle, every high school had a full orchestra; today, only two of the 10 high schools do.

Does anyone *know* why that decline has occurred? Do we truly have the knowledge that will produce a solution to that problem? Apart from intuition, hunches, and possibly a few good theories, the real question is "Do we *know*?" If not, and if we agree that there is a need to know, then that directly observed problem should serve as strong motivation to do some research.

In addition to such *directly observed* problems, there are two other types of problems that serve as motivators to undertake research. One is the *contradiction of facts and their contradictory conclusions* that occasionally appear in the literature. In the early 1970s, when the controversy of hemispheric specialization for music was becoming a cause célèbre, the research literature being cited was often contradictory. Toni Reineke (1981) focused attention on those contradictions, identifying them as the problem motivating her research on a partial processing theory of perception. Her findings provided a partial explanation for those contradictions. Until contradictions such as those and others in the research literature are resolved, our knowledge base is open to question.

A third type of problem is the *gap in knowledge*. The discovery of the planet Neptune is a classic example. When the planet Uranus proved not to be located exactly where Newtonian theory had predicted, it was hypothesized that an unknown and heretofore unseen planet existed. With appropriate calculations, the size and likely location of that planet were determined, and the planet was, in fact, located (and named Neptune). The gap in knowledge had been filled.

Why do research? Because we encounter problems that reveal our ignorance, and we can find no one with the knowledge we seek. We do research because we need to know.

What Do We Need to Know?

Journalists think that they have the answer to this question. It is a maxim of journalism that a newspaper article answer *who*, *what*, *where*, *when*, *why*, and *how*. (Researchers frequently use *how* interchangeably with *why* when the intent is to explore cause. More accurately, *how* is a question of procedure, a question usually answerable by someone who already knows. For this reason, *how* will defer to *why* in this discussion.) Which of these questions the researcher asks depends upon the nature of the problem identified and the character of the need to know. A brief examination of each of the five "w" questions will show how this is so.

First, the *who* question. Music history is rife with misattributions. There was reason to doubt that Purcell wrote some of the trumpet works attributed to him. If not Purcell, *who*? How would we learn *who* if no one else knows? We would certainly not distribute a survey instrument or conduct an experiment. A theoretic/analytic approach comparing stylistic features would be a more suitable method. (Such research is no longer necessary; we now know that it was Jeremiah Clarke.)

Next, the *what* question. In his book *How Musical is Man?*, John Blacking refers to the "biological and social origins of music" (1973, p. xi). Such an idea could provoke the question: "What is the role of music in relationship to the various aspects of a culture?" The theoretic/analytic approach suggested for the *who* question has little potency for answering this kind of *what* question; the most appropriate would be an anthropologic/ethnographic methodology.

A third kind of need to know is *when*. One of the values to be gained from an understanding of *when* an event took place is the greater perspective that it provides for other events happening near or at the same time. *When* questions may also be used to assist in answering *who* questions. For example, determining when a music manuscript has been produced can rule out potential candidates for who wrote it, especially those candidates whose known death occurred prior to the manuscript's production. To establish the point of an occurrence or a sequence of events, we depend on the record, whether written, oral, or in other exposition. It may be necessary to use means other than, or in addition to, bibliographic ones to establish the validity of time estimates: for example, an examination of watermarks, or chemical analysis of the ink used on the manuscript under consideration.

Occasionally, it is important to learn *where* something occurs. The claim was made earlier that there has been a decline in high school orchestras. Although the general evidence indicates this to be true, we do hear of places where orchestra programs are flourishing. A

study of the circumstances surrounding such programs could be quite fruitful, but before we could make such an analysis, we would need to learn *where* such programs exist. We would not expect bibliographic methods to be as helpful as a survey of some sort to answer that *where* question.

And the *why* question? The question *why* raises the issue of cause. Several years ago, we wondered *why* some of our undergraduate music majors made certain errors in melodic dictation. We conducted an experiment to test the hypothesis that violated expectancies would produce higher error rates than fulfilled ones (Unyk & Carlsen, 1987) and found the prediction to be correct. As a result, we gave credence to a theory of cognitive filtering of perception. A survey could not have established such inference, but the experiment could.

Each of these subquestions—*who*, *what*, *when*, *where*, and *why*—are indicative of the kinds of real musical concerns that grow out of identifiable problems. When the answers to such questions are not readily known, research is required.

What I have tried to illustrate with this brief exposition is that each kind of question demands a methodology different from the others. No one methodology is superior to others, but, depending upon the question before us, one method will prove to be more appropriate than any other.

What Will It Mean?

It is a grievous mistake to think that facts alone truly satisfy the need to know. In and of themselves, few facts are profound. At best, they serve as sparks to ignite the tinder of our minds—minds capable of meaning-making. It is the meaning of these facts that satisfies our need to know.

There are three types of meaning that can help us determine whether we are on the right track with the research we intend to pursue: (1) what the facts will mean for our theory, (2) what the revised or new theory will mean for future research, and (3) what that theory will mean in terms of practical applications.

First, what will the facts we unearth mean *for theory*? By theory, I refer to those explanations of the facts we observe. No research is undertaken theory-free. Every researcher has certain a priori notions of the reality in question. It may be a notion that a composer leaves a signature or that a composer will be stylistically consistent. It may be a notion that music has biological and social origins. It may be a notion that the music curriculum mirrors changing social attitudes. It may be a notion that advanced musical development depends on an urban setting. It may be a notion that our music perceptions are a function of familiarity with the style.

Where do such notions come from? They derive from our existing knowledge base, however limited. When the findings of research increase our knowledge, our notions undergo revision and, perhaps,

become theories. In some instances, a previous theory must be discarded to make place for a better theory. That is a *vital* meaning that we can derive from new facts.

Next, what might our new facts or revised theory mean *for future research*? As facts accumulate and theories become more precise in their explanation of those facts, we are tempted to elevate our theories to the level of "truth." But theories are not true, only plausible. For this reason, the existence of a good theory, in itself, constitutes a suitable gap-in-knowledge problem. As such it would deserve to be tested. The research process, in this sense, can be depicted as a helix, ongoing and leading to the next level required to satisfy our need to know. An understanding of this concept should help the budding researcher realize that there can be a research life after the doctoral dissertation.

Finally, what might the *practical applications* of our increased knowledge be? If the best we have are good theories, and if theories are not true, what are we supposed to do in the meantime? First, we can assume that our *facts* are true, and we can build activities consistent with those facts. We can assume that, lacking a better theory, we can operate *as if* the theory were true. When new facts emerge and the theories change, we can make the appropriate adjustments in our activities to be consistent with the new knowledge.

To illustrate, if our original attribution of composer were inaccurate, what would that mean, practically? For the publisher, the need to change the presses; for the music history professor, the need to modify the syllabus; for the music scholar, a rethinking of the composer's output and of the genre; for the methodists (with all due apology to John Wesley, I refer here to those who consider methodology, especially their particular methodology, to be the foundation for research), an increased appreciation of the theoretic/analytic approach, even if it were not their "preferred" method. For different people, there would be different practical meanings.

Closing Comments

As musicians, we have learned that effective performance depends upon a rather rigid sequence of events: the program is determined, the performers to assist are selected and engaged, the parts are learned, the works are rehearsed, a sound check is made, and the performance takes place. Alterations in that sequence are either impossible or take place at the expense of the effectiveness of the performance. Preparation to do research, too, has its sequence of events, and in that sequence, the selection of methodology occurs sometime *after* our consideration of what we need to know and what the facts we gather will mean.

I am not in disagreement with Madsen (1988), who has suggested that the "selection of a major professor for university music students be made along methodological and special interest lines rather than traditional subject-matter classifications" (p. 136). The implication

of my remarks, however, suggests that the methodology selection process occur after the student has determined what it is that is needed to be known—that which no one else yet knows. It is the nature of that question (*who, what, where, when, or why*) that will identify the methodology, and, presumably, the professor with the suitable expertise. To be sure, it is in the interaction with professors that a student's interests materialize and mature, and a knowledge base is developed to the point that decisions about what is known and what is needed to be known can be made. The sequence I have outlined should not be construed to mean that we ignore this important student-teacher interaction involved in arriving at the need to know. Beyond this, by considering ahead of time the meaning of all the possible outcomes of the research, the researcher is in a better position to evaluate whether a planned course of research activity will truly satisfy the need to know.

What I am in disagreement with are those statements by researchers that imply a hierarchy of research methods. One such example that may have contributed to "methodist-divisiveness" is the comment by Campbell and Stanley (1963) when they indicate that their book "is committed to the experiment: as the *only means for settling disputes regarding educational practice*, as the only way of verifying educational improvements, and as the only way of establishing a cumulative tradition in which improvements can be introduced without the danger of a faddish discard of old wisdom in favor of inferior novelties" (p. 2) [italics mine]. Had Campbell and Stanley qualified their statement by limiting it to those aspects of educational practice that pertain to cause/effect issues, then a claim to the appropriateness of the experiment would have been in order. If, however, the educational system is truly a *system*, then we must acknowledge that input components of that system, at least, may be better assessed by means other than the experiment. It would not be difficult to find similar exaggerated claims for the other methodologies we use; this one is sufficiently illustrative.

The challenge for us today is not to undertake research only because it will allow us to use our "favored" methodology, but rather to consider carefully the problem we have identified, along with the compelling question that problem raises, and to allow that question to dictate the method of inquiry. In so doing, our research will have the more substantial motivation: *the need to know*.

Endnote

1. Concerning publication style, the first 26 volumes of *Journal of Research in Music Education* adhered to the *MLA Modern Language Association Style Sheet*. For quantitative studies, the style was changed to that of the *Publication Manual of the American Psychological Association*. The style option was relaxed with volume 33 for nonquantitative studies, permitting authors to use the APA manual, the *MLA Handbook for Writers of Research Papers, Theses, and Dissertations*, or K. Turabian's *A Manual for*

Writers of Term Papers, Theses, and Dissertations. In the current policy, the *MLA* handbook has been supplanted by *The Chicago Manual of Style*. Because *The Chicago Manual* now includes separate styles for quantitative and nonquantitative studies, it could be selected to supplant both the APA and the Turabian manuals if the editorial committee were so inclined.

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