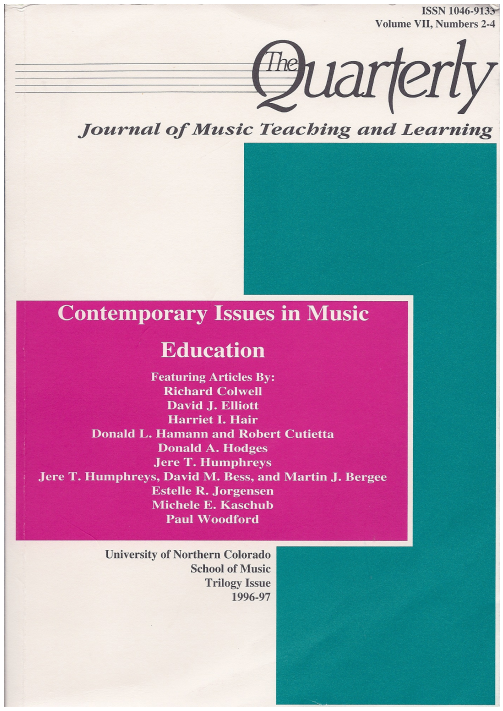

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Expanding The Horizons Of Music Education History And Sociology[1]

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Philosophers and other researchers sometimes "talk past" each other, because philosophers deal with values (that is, what should or could be), whereas other researchers study what was, is, or will be. The distinctions are far from perfect, however. Quantitative, qualitative, historical, and sociological researchers make value judgments when they select topics and research methods. To a degree, they deal with what should or could be, in addition to what was, is, or will be. Similarly, philosophers like David Elliott (1995) discuss not only what should or could be, but also what is, or at least what they believe to be true. Elliott carefully describes what he believes are the outcomes of music practice, as well as the types of music education practices that would best lead to those outcomes in the schools. Other philosophers (e.g., Reimer, 1989) also describe what they believe are, or should be, the outcomes of various musical practices — such as aesthetic experiences — and the practices they believe lead to those outcomes.

One of my premises is that non-philosophical researchers should examine the validity of the assertions made by Elliott (1995) and other philosophers. In other words, philosophical assumptions could be treated as research questions or hypotheses by other researchers. Clearly, philosophy and other types of research share overlapping areas of concern. For example, Elliott asserts that music practice and music education result in self-growth, self-knowledge, and, eventually, improved self-concept. Could and should his assertions be turned into testable hypotheses? Could and should the same be done for the outcomes hypothesized by proponents of music education as aesthetic education (MEAE)? In other words, could and should researchers measure the outcomes of music practice and music education?

As I understand it, Elliott's (1995) philosophy would bring music teaching and learning into closer alignment with actual music practice, which, according to Elliott, consists primarily of composing, arranging, performing, conducting, and listening. Elliott acknowledges that music education is a practice, and he celebrates many of the outcomes of music practice and music education that we all believe exist. He also places considerable emphasis on the personal and cultural aspects of music practice and education (Humphreys, 1996c). To the extent that his philosophy would legitimize more and different outcomes of music practice and education and would place music education into cultural contexts, it carries implications for expanding the horizons of research in music education as well.

Of course, non-philosophical researchers should not be bound by the writings of philosophers, but should attempt to examine everything that was, is, and will be, regardless of the assumptions and value judgments made by philosophers. Nevertheless, I be-

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...philosophical assumptions could be treated as research questions or hypotheses by other researchers...music education philosophy implies a rich research agenda.

lieve that music education philosophy implies a rich research agenda, and that Elliott's (1995) philosophy in particular offers considerable grist for the research mill.

Music education researchers have conducted countless studies of teaching strategies hypothesized to improve sight-reading ability, the ability to sing on pitch, and the like. Those who study such things believe implicitly in the worth of sight-singing and the ability to sing on pitch, but I am defining sight-singing and singing on pitch as enabling strategies, not final outcomes. By defining outcomes, Elliott (1995) and his fellow philosophers help other researchers frame research questions, who could examine the extent to which various enabling strategies contribute to those outcomes and the extent to which the hypothesized outcomes really occur.

My work on two projects in recent years — the chapter on music ensembles for the *Handbook of Research on Music Teaching and Learning* (Humphreys, May, & Nelson, 1992) and the arts education research compendium commissioned by the National Endowment for the Arts (National Endowment for the Arts, 1995) — convinced me of the lack of research on the outcomes of music education. We have little solid data on how music participation and instruction change people's lives: on how individuals and societies profit from music education, and on which types of people and groups profit and in what ways. Is it possible to determine such things? I believe it is, at least in part, and I believe it is incumbent upon researchers to try. In other words, researchers should make a concerted effort to identify and, yes, measure more and different types of dependent variables and the related independent variables. Even if political realities did not force us into outcomes research, we should do it anyway because we need to assess the results of music education, both in relation to the values set forth by philosophers and any other outcomes that may result from music education.

Within-Group Expansion

Music education historiography remains alive and well after several decades of success. The largest single body of research consists of more than 600 dissertations on the history of music education produced at American universities since 1923 (Heller, 1995; Humphreys, Bess, & Bergee, 1996). Unfortunately, dissertation writers have given little attention to the sociology of music education. Master's theses constitute the second largest body of historical research, but inadequate bibliographic tools limit access to this material (Heller & Wilson, 1992). The same is true for the relatively small body of sociologically-oriented master's theses.

A few scattered journal articles and book chapters on the history of music education appeared before the founding of the *Journal of Research in Music Education* (JRME) in 1953 (Heller, 1995). Since then, historical articles have appeared regularly in that and other journals. However, according to Yarbrough (1984), historical articles constituted only 17 percent of articles in the JRME through 1983, and Yarbrough's descriptive category includes only a few sociological articles. Sample's (1992) research suggests that no historical or sociological articles were cited 10 or more times in selected music education journals over a period of several decades. On the positive side, *The Bulletin of Historical Research in Music Education*, founded and edited by George Heller, serves as an important specialized outlet for music education history, and *The New Grove Dictionary of American Music* (Hitchcock & Sadie, 1986) represents a significant improvement in the coverage of music education history over earlier music dictionaries and encyclopedias

(Heller & Wilson, 1992). To date, there is no journal devoted to the sociology of music education comparable to *The Bulletin of Historical Research in Music Education,* or to the sociology of music comparable to the journal entitled *The Sociology of Education.*

Historians have contributed five book-length historical surveys on American music education. After Edward Birge's (1966) classic first appeared in 1928, two books came out in the 1970s (Sunderman, 1971; Tellstrom, 1971) and one each in the 1980s (Keene, 1982) and 1990s (Mark & Gary, 1992). There are several other books on music education history as well (see Heller, 1992). The major book on the sociology of music, Farnsworth's (1969) *The Social Psychology of Music,* is more than 25 years old.

Two excellent conferences on the history of music education in the United States held during the last few years include A Sesquicentennial Celebration Symposium, which marked the 150th anniversary of American public school music education, and The Ithaca Conference on American Music Education, which commemorated a century of music at Ithaca College (Fonder, 1992). One important conference devoted to the sociology of music education, called Symposium '95: The Sociology of Music Education, took place in 1995 at the University of Oklahoma.

Finally, the History and Social Sciences Special Research Interest Groups (SRIGs), under the auspices of the Music Educators National Conference (MENC), now make important contributions. Both SRIGs are large, active, and well organized.

Beginning with Birge and continuing through Allen Britton and their followers, researchers have contributed enormously to the knowledge base on the history of music education. Most of the sociological research related to music education has been completed by non-musician researchers in other fields. Regardless, the field would benefit from more studies that utilize traditional techniques. For example, most historical dissertations deal with the professional lives of individuals and with the histories of specific organizations and music programs. According to a recent study (Humphreys, Bess, & Bergee, 1996), historical dissertation topics have remained consistent over time except for biography, which has received a statistically significantly larger share of dissertation writers' attention since the 1950s. Nevertheless, the lives of many important individuals remain unchronicled. Similarly, researchers have produced more than two dozen theses, dissertations, and books on the histories of various state-level music educator associations (Biffle, 1991), which leaves approximately half of these organizations with no written histories. I agree with Heller and Wilson (1992), who wrote that "Gaps remain in the present story of people, places, and ideas associated with music teaching and learning" (p. 102).

Music education historians have produced few macro-level works that synthesize the results of existing smaller-scale studies. We have no meta-analyses of the biographies of leading music educators, and we know little about the commonalities and differences among state-level organizations because no one has attempted to synthesize the results of the existing studies. Similarly, there are no synthesis studies of the extant histories of local choral societies, histories of college music departments, and the like.

So far, I have described traditional historiography as it is practiced in music education and noted a need for more of the same. However, the revolution sweeping the wider field of historiography, the so-called "new history" that is turning modern historiography on its head (e.g., Burke, 1991; Graff & Monaco, 1980), has scarcely touched music education. New research strands, including Black and Gender Studies, and new political and economic realities, such as the demise of world colonialism and the advent of a global economy, are resulting in widespread dissatisfaction with traditional historical research. The roots of this dissatisfaction can be traced far back, but it coalesced in the 1970s and 1980s and shows no signs of abating in the 1990s. Some dissidents now even reject the findings of traditional historians, but most scholars continue to value the contributions of traditional historiography and would welcome more of the same. I agree with those who value traditional historiography, but I also believe that its findings remain incomplete — seriously incomplete.

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More diverse thinking among historians would help music education historiography expand beyond the traditional research paradigms. The "new history" utilizes a wider range of source materials and employs expanded research perspectives. So far, music education historians have not done this. For example, the authors of both the earliest and most recent books on the history of American music education, Edward Birge (1966) and Michael Mark and Charles Gary (1992), respectively, gave significantly less coverage to female than to male music educators (Humphreys, in press). Similarly, individuals from the MENC Eastern Division are significantly over represented relative to the division's population in one of those books, and the Northcentral Division is over represented in the other. Music educators from the Southern Division are significantly under- represented in both books (Humphreys, in press).

There were significantly more female than male MENC members during much of that organization's history (Humphreys & Schmidt, 1996), and it is probably safe to conclude that there were more female than male music teachers in the United States, at least during most of the twentieth century. Furthermore, the Southern Division had less than its share of MENC members at least during certain decades (Humphreys & Schmidt, 1996), but the region has a rich history of music education activities nevertheless. If all this is so, why did these representations occur? After all, these are capable authors who produced good books. Were the authors biased against women and Southerners? I think not.

One reason for the traditional representations relates to source material. Southern music education tended toward local, less organized activities, the practitioners of which left few written records. In addition, a significant majority of historical dissertation writers in music education are men (Humphreys, Bess, & Bergee,1996), who tend to write about what most interests them, which is frequently other men and male-dominated activities. Moreover, universities in the Northcentral and Eastern divisions have produced more than their share of historical dissertations (Humphreys, Bess, & Bergee, 1996). These dissertations and their authors play a large role in the historical research network and contribute substantially, directly and indirectly, to the knowledge base upon which book authors draw. Finally, at the time their books were published, the book authors themselves lived in the two regions that are over represented in their respective books. In other words, the authors relied heavily upon available traditional sources, which tend to deal with certain types of activities and people.

In addition to being males from the Midwest or East, virtually all music education historians are classically-trained musicians. This leads historians to study what fits into their conceptions of what should have been, rather than what was, and to underrepresent or ignore the rest. For example, music ensembles first appeared in most American colleges and universities during the late nineteenth and early twentieth centuries. Music education historians have written a great deal about the early orchestras, mixed choirs, and bands, but have virtually ignored the huge numbers of glee clubs, harmonica bands, accordion and mandolin orchestras, guitar ensembles, and athletic bands. These latter groups were extremely popular in American colleges during that era (Humphreys, 1992) and undoubtably helped pave the way for the more traditional ensembles.

Why did Birge (1966), for one, ignore these popular groups? He was professionally active during much of that period and must have had first-hand knowledge of them. Was it because they did not fit into his conception of what should have been, in other words, of what was really important? Did he fail to dis-

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cuss these groups because they were not associated with art music? These numerous, popular groups outshone the more traditional art music ensembles of the period on many college campuses. Historical and sociological insights into the whats, hows, whys, whos, and whens of this form of music education would enrich our understanding of the past. Ignoring it leads to incomplete conceptions.

In addition to being classical musicians, music education historians are trained, experienced educators. This expertise facilitates certain kinds of insights, but it blinds researchers to other things. For example, Eaklor (1994) examined nineteenth-century musicians in some new ways in her recent article. She touched briefly on the strong service mentality that permeated education in the nineteenth century. This service orientation was fostered by Horace Mann and other educational leaders, who attempted to indoctrinate teachers and normal school students toward service because the field of education offered so few tangible rewards to teachers. Because a strong residue of the service mentality remains among educators today, including college music educators, music education historians tend not to see it. Eaklor, a trained music educator but now a professional historian, enriched our view of the past because she was able to overcome this particular myopia. The elitism and idealism that permeate the world of classical art music, and the duty-bound culture of service that characterizes public school teaching, tend to constrain our research perspectives.

When most scholars in a given field share the same demographic characteristics, unintentional homogeneity can be reflected in the research results. The variables of gender, race, geographical region, nationality, and training have led historians to virtually ignore non-school music education, both formal and informal, and to under-emphasize most types of music and music education throughout the world. Just as Grout and Palisca's (1960; 5th Ed. 1996) *History of Western Music* is not really a history of western music, but a history of western art music (and only a relatively small portion of that), music education historiography deals with only a small subset of what was really there.

Finally, traditional historical research in music education (and other fields) is restricted by the top-down approach to historiography. Top-down historiography emphasizes the contributions of, say, MENC presidents at the expense of rank-and-file music teachers and students, and the MENC and leading music programs at the expense of more ordinary organizations and programs (Humphreys, in press), not to mention informal music activities. Considerable attention is being devoted to the concept of "history from below" in other fields (e.g., Sharpe, 1991), which essentially is the study of ordinary people and events. The top-down approach contributes to under- and over-reporting on certain types of people, places, and events, especially when coupled with the other perspectives of traditional historiography noted earlier. For example, top-down historiography contributes to the underrepresentation of women and other groups in historical writing, and to national and regional research biases, not to mention the lack of attention given by general historians to arts and education history in comparison to political, economic, religious, and military history.

So far, I have described a highly traditional yet reasonably healthy historical research community in music education, but an almost nonexistent sociological research community. Sociological research in music education suffers from under-subscription: there simply are not enough researchers doing this type of work. McCarthy (1994, 1995), Mark

...researchers with different skill sets could expand the parameters of our research and thereby develop important new insights.

(1995), and a few others have begun to examine relationships between music education and society. For example, the profound influence of the progressive education movement on music education has been investigated increasingly in recent years (Humphreys, 1992; McCarthy, 1994, 1995). Nevertheless, the sociology of music education needs more of an identity, and the field needs people with training in sociology and sociological research techniques.

Between-Group Expansion

Now for a discussion of between-group expansion, the combining of two or more methodologies, which historians Graff and Monaco (1980) call "methodological pluralism" (p. 23) and education researcher Jon Wagner (1993) calls "disciplinary annexation" (p. 18). Most individual researchers utilize a single methodology, generally throughout their entire careers. However, quantitative researchers continue to fret about "the gap" between research and practice, and historians, philosophers, and qualitative researchers complain about the lack of recognition for their work within the research community. In fact, proponents of each methodology believe, with some justification, that their research does not receive the attention it deserves from other researchers and music teachers.

Reimer (1992) likened the research community's focus on research methodology to practicing music teachers' focus on teaching methodology. Reimer's (1992) solution to the overfocus on research methodology is to develop philosophical foundations and policies for music education research. Mine is for researchers to expand their horizons and to work together in small groups, combining their talents and insights in hopes of producing new and different results. I believe, like the qualitative researcher Laurel Richardson (1991), that no research method or theory "has a privileged place" or "a universal and general claim to authoritative knowledge" (p. 173). My main concern, however, is not whether knowledge is authoritative, but that we seek different kinds knowledge, primarily about the outcomes of music practice and education.

Music education is so complex and so intertwined with the world that fragmented research efforts, no matter how well designed and carried out, fail to consider enough variables to make the research appear relevant, especially to non-researchers. Research methodologies are the tools of our trade, but our preoccupation with methodology has resulted in methodological parochialism. This, in turn, has caused us to overlook some important research arenas. By working together, researchers with different skill sets could expand the parameters of our research and thereby develop important new insights.

Like Elliott (1995), most music education researchers tend to focus on school music education. Perhaps it is appropriate for a guiding philosophy for professional music educators to focus exclusively on intentional music education in schools. However, if we accept Elliott's notion of the cultural bases of music, music making, and music education, and if we are to examine his assertions about outcomes, historians, sociologists, and other researchers must consider the cultural roots and contexts of what he calls "musicing." For me, that includes learning more about informal, unintentional modes of music education as well. The examination of cultural issues and informal music education will require the use of multi-modal research approaches.

Farnsworth (1969) utilized quantification in his pioneering studies, as did Price (1990) in his study of professional orchestra programming practices, Hargreaves, Comber, and Colley (1995) in their study of musical preferences, and Bowles (1988, 1991) in her study

of concert attendance. These and other studies (see Hoffer, 1992) shed light on musical preference and activity outside the school setting. The scholarly study of a culture or cultures implies more than the study of leading individuals, institutions, and programs. To complete broad-based studies, demographic, political, and economic variables must be considered, which in many instances requires sampling and statistical analysis.

These researchers all utilized multi-modal methodologies. Price (1990) studied orchestra programming changes over time, examined sociological phenomena, and employed quantification. Is his research historical, sociological, or quantitative? I believe it to be all of the above. Price went beyond school music to examine musical taste and a few related business aspects of music, and he in effect extended his study backward in time by comparing his results to those of similar studies from earlier periods. Price's line of inquiry could be expanded to include other countries and different regions within this country, and different social and age strata within cultures. Then we could begin to make cross-cultural comparisons, over time, for different subgroups.

A study-in-progress by Humphreys and Schmidt (1996) on MENC demographics is another example of a multi-modal approach. We are correlating MENC membership during twenty-seven of the organization's early years with several variables, including state population, male-to-female teacher ratios, ratio of MENC members to number of teachers, educational expenditure per capita, mean teacher salaries, distances to convention cities, and more. That study should be extended to the present. We should determine the characteristics of MENC members and non-members, MENC "dropouts," those who attend national-, divisional-, and state-level conferences, and other variables. We should investigate age, sex, subject matter taught, grades taught, family income, and many, many other variables. Just as McDonald's Corporation undoubtably uses multiple regression models to predict the number of hamburgers a given store will sell from housing and traffic patterns, family income, proximity to schools, and many other variables, we should attempt to identify variables that predict MENC membership and participation. This type of research could be applied to other professional organizations, arts patron groups, listening audiences, Elderhostel programs, and on and on. We know very little about the people we strive to serve, or about ourselves.

A study-in-progress (Humphreys, 1996b) of outcomes involves an examination of selected longitudinal data compiled by Louis Terman and his followers (Terman, Cox, & Oden, 1925-1959). Terman's data, which were collected at intervals from 1921-1959, include a few items of interest to music educators related to gifted students' enjoyment of music and other arts classes and their later success as adults at various stages in their lives. The examination of these data requires historical, sociological, and quantitative techniques, as well as sociological perspectives.

Still another study-in-progress (Humphreys, 1996a) examines the musical aspects of a longitudinal data set representing 24,000 eighth-graders compiled by the National Center for Education Statistics (e.g., National Center for Education Statistics, 1990). A national sample of students was questioned about school music ensemble participation. Bergonzi and Smith (1994) examined these data in relation to arts participation. I am taking a different tack and treating academic grades and several social behaviors as dependent variables in relation to school ensemble participation.

Another potentially fruitful area for multimodal research is popular, commercial music. Music education researchers and musicologists have tended to ignore popular music, one of the most important cultural phenomena of the twentieth century. But let's take a closer look. Where did this music come from?

I would argue, first, that twentieth-century popular commercial music came into being in part because of the concept of adolescence, which has been around for roughly one hundred years. In older hunting and agrarian societies, children passed directly into adulthood, perhaps after one or more ritual rites of passing. As society became more complex, especially after the onset of the Industrial Revolution, people required

more formal education, and they needed more maturity to deal with an increasingly sophisticated world (Coleman, 1961); hence, the concept (and reality) of adolescence was born. Commercial popular music caters to adolescents (Coleman, 1961), with its focus on heterosexual love, a major concern of adolescents. The second enabler of commercial music was technological: first the player piano, then the phonograph, then the radio, and so on. The confluence of a new social construction, adolescence, and developments in technology led to an entirely new phenomenon in the history of music.

Popular commercial music is different from art music, and it must be studied differently. Traditional harmonic and formal analysis fails to reveal its essence. We should study the lyrics, characteristics of the performers, and historical and sociological factors surrounding the music's composition and performance in addition to the music itself. Some research of this type has been done and reported in journals such as *Popular Music & Society*, and in books, e.g., *Country Music U.S.A.* (Malone, 1985) and *American Popular Music and its Business* (Sanjek, 1988). However, except for a few studies similar to those by Pembrook (1987) and Thompson (1994) on rock music, music educators are not doing the work. Additional meaningful research on popular music would require collaboration among historians, sociologists, theorists, quantifiers, and perhaps qualitative and philosophical researchers.

We could also learn from multi-modal research being completed in other fields. Anthropology, for one, provides some tantalizing models. Anthropologist Donald Brown (1988) used a complex mix of historical, sociological, and anthropological techniques in his attempt to predict the quality, quantity, and nature of historiography across cultures (Humphreys, 1990). Brown gave a full account of his research methodology, including how he developed and ultimately rejected several hypotheses that had been proposed as explanations for varying types and quantities of historiography across cultures.

Among the many cultures he investigated, past and present, Brown found a positive correlation between the quality of historical writing and vertical social class mobility — mobility that is attained more by achievement than by birthright. At the opposite end of the continuum, weaker historiographical traditions evolved in cultures with strong, relatively static hereditarily determined systems of social stratification. Brown then searched for and reported on causes for these correlations, because "without intelligible linkages . . ., any correlations would be mere curiosities" (p. 6). He also reported all important conflicting and missing evidence (Humphreys, 1990). Brown did not utilize quantitative techniques overtly, but he amassed an impressive array of evidence to support his conclusions. Music education researchers could use similar approaches to compare music education in different cultures.

The results of a single-culture study that employed several research modes are reported in a fascinating book entitled *What Makes Democracy Work: Civic Traditions in Modern Italy* (Putnam, 1993). Harvard University government professor Robert Putnam first rated the effectiveness of Italian regional governments, most of which were formed in 1970. Using quantitative and qualitative methods, he developed historical predictive models for why some of these governments now function more effectively than others. He concluded that the most powerful predictor of successful democratic government is the number of citizens involved in organizations whose members cross class and other social dividing lines. These groups include soccer clubs, bird-watching groups, and choral societies — in other words, social organizations that help people feel more connected to each other and thus more aware of their communities. Putnam believes that social constructions change extremely slowly, and that even as early as the middle ages the now-successful regions demonstrated more "civic-mindedness" than the less successful regions. His quantitative analysis suggests that past social cohesiveness, including people's willingness to work together and to trust each other and their government, predicts current governmental effectiveness better than any other variable.

Incredibly, Putnam (1993) can predict current regional economic conditions with his historical "civicness" scale better than with

measures of past economic conditions. Music education researchers could utilize similar multi-modal techniques to examine how, why, and in what ways different types of people, institutions (e.g., schools), and communities support formal and informal music education, in addition to the results of such support or lack of support. To date, music education historians have focused on the contributions of outstanding individuals and programs, but sociological research on group behavior of the type completed by Putnam is virtually non-existent.

It is interesting to speculate about the possible results of such research in music education. What if researchers were to find that music education really does improve mathematics performance, or that people who participate in music activities have lower divorce rates, attain more formal education, or possess higher self-esteem? What if we were to find that marching bands really do help produce better citizens? What if we were to find, as Elliott (1995) speculates, that many of the positive outcomes of music participation and education, such as self-concept, could result from other activities as well? What if we were to find, like Putnam's (1993) research suggests, that these outcomes may result from multiple activities, music education among them? Putnam did not specifically identify choral society participation as a correlate of more effective government in Italy, but he did find choral participation related to other types of civic behavior that predict effective government. Our notion that the outcomes of music education must be unique to music education has contributed to our overlooking the possibility that, as Elliott sets forth, music practice and education may produce some of the same outcomes as certain other activities, like sports. But what's wrong with that? The MEAE philosophy and our long-standing professional paranoia about the status of music education (Humphreys, 1988) constrain our perspectives and prevent us from examining what are probably extremely valuable, but perhaps not unique, outcomes of music education.

It is also interesting to speculate about the reactions of music educators should researchers identify such outcomes. Would music educators deem the findings irrelevant? I hope not. Music education research exists not to promulgate particular research methodologies or philosophies, but to improve music education practice. To accomplish this, researchers need to determine, among other things, what music education does and does not do.

Some Strategies and Conclusions

Between-group expansion would require changes in our entire system of research: attitudinally and behaviorally on the individual level and structurally on the system level. Past strategies proposed to improve research in music education include more collaborative projects between researchers and practicing music teachers and more user-friendly formats for the presentation of research results. Certainly, practitioners' perspectives add new dimensions to research, and improved communication of research results helps also. However, due to inherent differences in the nature of research and practice, practitioners' perspectives tend to be limited also. More collaboration and communication with practitioners would expand our research horizons, but mainly in the direction of teaching methodologies. Researchers need to collaborate with each other in order to expand in other directions.

So, what should we do? For starters, the MENC should provide more support for research. Molnar (1948) observed that the MENC provided less support for research than that provided by other similar organizations. Clearly, the MENC remains less than fully supportive of research. In addition, it uses unpublished, non-juried research results as supporting planks in its advocacy campaigns. This ends-justify-the-means mentality cheapens the research process and the findings. The MENC might be more willing to support research, however, if the research community would engage in the types of research that I am advocating.

Our research system could use an overhaul. We have long complained about the fact that most music education research is conducted by doctoral students, who lack the time, funding, and sophistication to conduct large-scale studies. However, we now have a

cadre of career researchers who could undertake larger, more collaborative studies. In addition, if our research were less fragmented, funding agencies might be more willing to support it. We should tackle several large data sets that have been compiled by government agencies and various social researchers (L'Hommedieu, 1992). A current limitation of such data sets is that the compilers did not ask enough of the right types of questions. For example, the longitudinal study of eighth-graders mentioned earlier (National Center for Education Statistics, 1990) combines band and orchestra data, and it includes little about general music. The music education profession, led by MENC and its research community, should attempt to influence the design and content of future large-scale studies conducted by the government and other agencies.

In the long run, more collaborative work among researchers would be facilitated if we researchers would broaden our horizons and fill university positions with researchers of different types. We should move out of our comfort zones, take some bold steps, and avoid hiring methodological clones. In the meantime, universities should be encouraged to support large-scale collaborative work between researchers at different institutions, especially now that we have the data-sharing capabilities of the Internet. Doctoral research could be enhanced by better access to faculty members at other institutions, gained through more creative policies governing faculty load credit and the teleconferencing of dissertation defenses. All this would facilitate the completion of large-scale, collaborative, even longitudinal studies by colleagues at different institutions, supported by graduate students. Finally, the current reward system for researchers has been fostered by quantitative researchers, the dominant methodological group in music education and in universities today. The quantifiers' proclivity toward counting things has resulted in a system that rewards small-scale studies. A more flexible reward system would foster the production of more large-scale studies.

Music educators began to conduct research approximately 75 years ago. Early researchers borrowed their methods from experimental psychology, although early on there was a sprinkling of historical studies and many, many nondescript descriptive and curriculum studies. We are now in the fourth or fifth generation of music education research. The enmity between researchers of even one generation ago has abated considerably. Researchers of different persuasions tolerate each other and each other's work better now.

Each of our research modes has strengths and each has limitations. All research that is carefully constructed and carried out has worth, but let's look for ways to cooperate and collaborate and thereby make our collective efforts more meaningful. One way to begin is to look for the strengths in our fellow researchers and their methods instead of their weaknesses. Think of the research community as an orchestra. Each member of an orchestra plays only one or two instruments, but without each other there would be no orchestra, only soloists. Clearly, most music education researchers are soloists.

Superficially, historical research appears qualitative in nature. Historians certainly utilize qualitative techniques, and their presentation mode, the narrative account, resembles the preferred presentation mode of qualitative researchers. However, traditional historians' overriding concern for objectivity, operationalized via external and internal criticism, together with the exalted status accorded written documentation in the hierarchy of source material, place traditional historiography squarely in the positivist camp (Graff & Monaco, 1980). Until recently, nearly all historians sought to determine what was by following these dictates, which were promulgated by the nineteenth-century German historian Leopold von Ranke. Clearly, only a positivist would call for a definitive history of anything. Modern sociologi-

cal research also follows the positivist tradition, with its predilection toward quantification and theory-building. Of the research methods currently in use in music education, only qualitative seeks to expand beyond positivism by declaring that no research or researcher can (or perhaps even should) be completely objective. This is a much-needed perspective in music education research, but positivism is not the evil empire. Positivist research, historical and quantitative, has proven its worth. Nevertheless, qualitative research may bring much-needed new perspectives.

Some researchers view qualitative research as simply an alternative methodology (or set of methodologies) to traditional research modes. Others seem to suggest that qualitative research is not a method at all. For example, Denzin (1995) states that qualitative research should embrace all types of research methodologies, but in the same article he states that the essence of qualitative research centers on naturalistic techniques and serving as a watchdog over positivist researchers. I, for one, am confused. Is qualitative research to be merely a new, alternative methodology, or is it to serve as an umbrella under which all methodologies can contribute? At present, the term qualitative is in danger of becoming too vague to be useful. Arguing with someone about qualitative research methodology is like arguing about the *Bible:* one can find support in the literature for, and ammunition against, almost any position.

Nevertheless, historians, sociologists, and other positivists could learn a great deal from qualitative research premises. I believe that all types of researchers could benefit from an understanding of the concepts of inherent researcher bias and the importance of context, as well as some other qualitative perspectives.

The findings obtained so laboriously and with sincerity via the discrete research modes remain valuable, and my remarks should in no way be construed as negative toward past, current, or future efforts of the same type. The expanded research paradigms that I have discussed will depend on sound discrete methodologies and findings. If we are to build collaborative research models, traditional research results from the past, present, and future will become more important, not less. Studies that utilize a single methodology will not become obsolete; rather, they will remain valuable in and of themselves, and they will become essential building blocks for multi-modal studies.

Much of the small body of sociological research in music education to date has been completed by music education historians. Not surprisingly, with a few exceptions, this group has ignored current sociological issues. Also, historians, unlike professional sociologists, tend not to utilize quantitative techniques. In fact, most sociological work in music education is really social history. Much of the remaining sociological research has been completed by empiricists, who tend to fail to consider historical factors adequately. Obviously, both approaches to music education sociology — historical and quantitative — are worthwhile, but the results could be enhanced if both approaches were combined. Interestingly, sociologists in some fields are increasingly incorporating the findings of historians, and modern historians are using sociological techniques. Our field remains impoverished by the lack of sociological research and researchers, regardless of type.

I have given a few examples of research of the types I am advocating, both within and outside the field of music education. I realize that most of what I am recommending remains "pie in the sky." Nevertheless, much work is being done in other fields. What is being called the "new history" is now reported in a wide range of new journals, including the *Journal of Interdisciplinary History, Journal of Social History, Journal of Psychohistory, Social History, Journal of Historical Geography, Journal of Urban History, Comparative Studies in Society and History, Journal of Family History,* and *Societas* (Graff & Monaco, 1980), in addition to older outlets like the *Journal of the History of Ideas* and others. I agree with Heller (1992), who pointed out that:

> Historians in related fields are debating the issues revisionism, relativism, and political correctness with considerable passion...and it seems unlikely that music education... historians can continue to ignore these concerns." (p. 61)

Generally, the current status of research in our field is reflected in the *Handbook of Re-*

search on Music Teaching and Learning (Colwell, 1992). Only two chapters of 55 are devoted to history. Of those, one chapter deals with methodology (Heller & Wilson, 1992) and the other largely with the history of empirical research (Mark, 1992). Philosophy received three chapters (Jorgensen, 1992; Reimer, 1992; Stubley, 1992), and there are single chapters on qualitative research (Bresler, 1992) and sociology (Hoffer, 1992). The *Handbook* represents music education research very well, with the notable exception that, despite their substantial and significant findings, there are no chapters devoted to the findings of historians and philosophers of music education. Only one chapter is devoted to research on international and comparative music education (Kemp & Lepherd, 1992), and there is one chapter on model building (Edwards, 1992). The remaining 46 chapters deal largely with quantitative empirical research methodology and findings. Will we see chapters on interdisciplinary research methods and results in the next music education research handbook? Signs are positive. For example, Indiana University hosted an international symposium on interdisciplinary studies in music in 1995.

David Elliott's (1995) philosophy is a practical one, and it implies that our research efforts could become more practical also. Many aspects of the music education practices he advocates are researchable. He sees music and music education not as esoteric, nearly unapproachable phenomena, but as practices inseparable from the cultures of which they are part, practices that affect participants in tangible ways. Elliott's music and music education practices are multidimensional phenomena; therefore, research on the practice of music education must be multidimensional also.

To summarize and reiterate, there is much right with our current modes of inquiry, but each mode by itself remains incomplete. Each methodological group could broaden its horizons by developing new visions and new research questions. Such efforts will require collaboration among music education philosophers, historians, sociologists, quantifiers, and qualifiers, as well as anthropologists, general historians, historians of education, economists, demographers, and even psychoanalysts. But more than specific strategies, expanding our research horizons will require attitudinal shifts about what constitutes worthwhile research.

Elliott's (1995) philosophy represents a very different philosophical paradigm from that set forth by previous philosophers. The primary task of philosophers is not to provide an agenda for other researchers, but Elliott has done just that. We may find that music education improves mathematics performance, an outcome that Elliott denies but that I believe may well be true, if for no other reason than music education probably really does improve self-esteem — an outcome that he does believe in. Regardless of what new research efforts may reveal, I hope that you will consider the foregoing discussion not as an alternative to traditional research in music education, but as ideas about how our horizons might be expanded.

Notes

1. A slightly longer version of this article was presented at the Society for Research in Music Education's Key Focus session at the biennial meeting of the Music Educators National Conference, Kansas City, MO, April 17, 1996. The session, entitled "Philosophical, Behavioral and Historical Perspectives for Research in Music Education," was organized and chaired by Harriet I. Hair.

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