The Functions of Educational Research

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In 1969, just a little over 20 years ago, Lee J. Cronbach and Patrick Suppes coedited a book entitled Research for Tomorrow's Schools in which they drew a distinction between two kinds of research, each of which they portrayed as having an important bearing on education. The first of these they called "decision-oriented" inquiry, the second, "conclusion-oriented." Here is how they described the difference:

In a decision-oriented study the investigator is asked to provide information wanted by a decision-maker: a school administrator, a governmental policy-maker...or the like. The conclusion-oriented study is a commissioned study ....The conclusion-oriented study, on the other hand, takes its direction from the investigator's commitments and hunches....The latter formulates his own question, usually a general one rather than a question about a particular institution. The aim is to conceptualize and understand the chosen phenomenon; a particular finding is only a means to that end. (pp. 20-21)

This distinction is one I would like to examine and build upon in the remarks to follow. I have three reasons for wanting to do so.

The first is that different ideas about the aims of educational research, about what its chief function or functions should be, give rise to a lot of friction and disagreement within our research community, much of it needless bickering, in my judgment. What better place to talk about such matters in what I hope will be an irenic mode than at a meeting where advocates of radically different research traditions are literally sitting side by side?

A second reason for wanting to talk about the functions of educational research is that the topic bears on the direction the social sciences have been taking of late, which in turn has reflected in the past, present, and future conduct of this organization and its membership. Important changes have been going on within the social sciences over the past two or three decades, as everyone here is doubtless aware. Traditional aspirations have changed; so too has the alignment of the disciplines with each other and with neighboring fields of inquiry. Clifford Geertz has spoken of what has happened as "the blurring of genres," calling it "a sea change in our notion not so much of what knowledge is but of what it is we want to know" (1983, p. 34). These massive changes extend of course to what it is we want to know about education, which pertains directly to the function of educational research, though the way in which it does so may not be readily apparent.

A third and final reason for my choice of topic is that it affects me personally. It links directly to recent experiences I have had in connection with a research project—an observational study of teachers in six schools—in which I have been engaged for the past 2 or 3 years. That work is still very much in progress but its direction is sufficiently clear to allow me to say a word or two about its outcome. This seems to me as good a time as any to do just that. I suspect I shall never have a bigger or a better audience.

So much, then, for why I have decided to address this topic at this time. Now back to Cronbach and Suppes for a closer look at what they were trying to do some 20 years ago.

I have already noted how they distinguished between "decision-oriented" and "conclusion-oriented" research. Actually, the distinction was not theirs to begin with, as they themselves point out. It was first used by the statistician John Tukey in a dinner speech he gave back in 1955. Tukey introduced the categories to highlight the difference between two uses of statistics, one having to do with applied problems, the other with pure science.

To repeat a bit of what has already been said, decision-oriented studies, according to Cronbach and Suppes, are those that someone other than the investigator wants done and sets into motion. They have been commissioned in other words, possibly by practitioners, such as teachers or school administrators, but, given the cost of research these days, more likely by policymakers of one kind or another, which is to say, legislators, investigatory commissions, foundation officials, and the like. There is seldom any question about the rationale of such research. As its name implies, it is designed to help someone (other than the researcher) make a decision about something—about how to teach reading, for example, or how to apportion the costs of schooling among taxpayers, or how to develop a national certification exam for teachers.

Conclusion-oriented studies, on the other hand, originate with the investigator. The researcher is the one who calls the shots, not some outside funding agency. This is not to say that the investigation must lack outside support. On the contrary, if the study costs a lot of money, as most do these days,
it is almost bound to have a sponsor of some kind. The two crucial differences lie in the freedom afforded the investigator and in the fact that the research itself need not be tied to some practical outcome, though the latter possibility is by no means prohibited. To quote Cronbach and Suppes (1969) once again, “The distinction between decision-oriented and conclusion-oriented research lies in the origination of the inquiry and the constraints imposed by its institutional setting, not in topic or technique, nor in the interests held by the investigator” (p. 23).

If we stop to think about that last statement, however, the distinction between decision-oriented and conclusion-oriented research begins to fall apart, or at least the labels themselves start to sound inappropriate. For it seems as though conclusion-oriented researchers may be just as decision-oriented as their decision-oriented counterparts, provided of course that they have adopted that orientation on their own and not at the command of someone else, such as an external funding agency. They too, in other words, may be interested in discovering a new way to teach reading or a new formula to apportion school finances or an ingenious new way to evaluate teachers. As Cronbach and Suppes (1969) point out, “this freedom for exploration and self-direction can be present in research on (e.g.) practices in school administration or on the effects of educational television—topics that many people would [consider applied]” (p. 21).

Those of us who are more or less literal-minded, at least when it comes to academic prose, well may wonder what is going on here. Why label as conclusion-oriented something that can also be decision-oriented? Why the apparent mix-up? I am not entirely sure, but I think I know why. I believe Cronbach and Suppes were trying to do two things at once and they settled upon a pair of terms that they hoped would be adequate for both jobs but which turned out not to be, at least not without causing some unnecessary confusion.

On the one hand they were trying to protect the integrity of the individual researcher from the onslaught of external demands. They wanted their intended audience, chiefly policymakers, to understand the importance of allowing at least some researchers, possibly most of them, to follow their own lights (their commitments and hunches) no matter where they might lead. During the mid- to late 1960s, when government agencies and other funding sources were increasingly wanting to play a hands-on role in the formulation and control of research agendas, this was a timely argument to make. On the other hand, Cronbach and Suppes were also trying to speak in defense of a particular kind of research, one that was not tied to practical matters, at least not ostensibly. Here is where the appropriateness of the term conclusion, rather than decision, comes in. This too was an important issue at the time and remains so today. The mix-up in terminology comes from the fact that not all researchers who work on their own choose to ignore practical questions, indeed most may not do so. Hence, some who exercise the freedom of what Cronbach and Suppes call conclusion-oriented research will also turn out to be decision-oriented.

Having identified what seems to me to be the source of the confusion, let me now restrict our attention to the second of Cronbach and Suppes’s categories, setting aside for the remainder of our discussion those cases within that second category that muddy the water. From here on I shall concentrate on educational research that is exclusively conclusion-oriented, which is to say research that is not designed to tell either practitioners or system managers (i.e., policymakers) what to do or how to go about their work more efficiently or effectively. I emphasize the adjective educational because most of the research that Cronbach and Suppes discuss in the section of their book devoted to conclusion-oriented inquiry did not initially have anything to do with education. They chiefly cite the work of intellectual giants, figures like Darwin, Freud, Pierce, and Pavlov, who, as everyone knows, did not set out to contribute to education at all but whose ideas have come to have an important impact on educational thought and practice. The harder cases to understand and to defend, however, are those in which there is a declared focus on educational matters right from the start without a concomitant interest in practical applications. I suspect that that description typifies the work of many of us seated here today, as I know it does mine. The question to which such activity inevitably gives rise is the familiar one of: Why do it? What good will it do?

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Cronbach and Suppes provide two answers to that question. They first point out that we often derive unexpected, practical benefits from research that was not designed to have practical outcomes. This familiar fact needs no elaboration. To sustain it one need only point to the many practical applications that have accompanied advances in pure mathematics or theoretical physics. Presumably the same could happen in education. There too, we might hope, practical outcomes could emerge spontaneously and unexpectedly on the heels of theoretical or conceptual advances. Certainly one would welcome them if they did occur.

Cronbach and Suppes’s second answer to the question of what such research might reasonably accomplish is far more intriguing than their first. It is also of greater consequence for our professional community, or so I believe. That being so, I shall devote the remainder of my remarks to its explication.

In essence, Cronbach and Suppes argue that conclusion-oriented research is chiefly of value because of what it does to something that they call “the prevailing view.” This key term, they explain, refers to “a widely held belief system [that] underlies the normal practice of every institution” (p. 125). As applied to education, it “embraces the ends to be sought, the procedures that may be used, the costs it is...
reasonable to incur, and the degree of success that may be expected” (p. 125). It “is not monolithic,” they go on, “and it does not change all at once,” though it does indeed change over time. At any given moment, it is characterized by a “strong central trend” that pervades the outlook of a sub-community of people who think more or less alike on a variety of topics, such as the teachers at a certain level of schooling or those who teach a particular school subject. This prevailing view, Cronbach and Suppes conclude, “is a synthesis or compromise shaped by human nature, political institutions, and the writings of learned men” (p. 126).

One of the things that makes this notion of the prevailing view interesting is its obvious resemblance to Thomas Kuhn’s well-known idea of “shared paradigm,” which dominates within what he calls “normal science” (1962). As Kuhn explains,

“...the enlightenment of the investigator and the small community of specialists thinking about the same problem.”

Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal sciences, i.e., for the genesis and continuation of a particular research tradition. (p. 11)

Cronbach and Suppes’s description of how the prevailing view undergoes change further resembles Kuhn’s account of the process by which paradigms are changed or abandoned. Here is how they speak of it.

They first observe that “[a] conclusion-oriented study is not performed for the mass of educators; it is performed for the enlightenment of the investigator and the small community of specialists thinking about the same problem” (p. 127). They then add these details about how the process works:

As investigators discuss their results with colleagues, a restlessness emerges. Findings from different studies seem to conflict. Phenomena are noticed that cannot well be summarized in the available language. Similarities are observed among findings on what were previously considered to be distinct topics. As investigators puzzle over these irregularities, they find themselves assembling what they know in a new way. Some of the concepts so created will influence thought throughout the educational world. Some especially stimulating or clarifying concepts will influence even the views of the public, especially when the researchers come forth to argue their positions in public media. (p. 127–128)

Two features of that description are especially noteworthy. One is that Cronbach and Suppes depict the change as occurring first within a small circle of specialists and then fanning outward, creating a kind of ripple effect. The ripple metaphor suggests the possibility of there being some kind of continuity or commonality between the prevailing view of the scholarly community on the one hand and the general public on the other. That possibility in turn suggests a linkage between the idea of the prevailing view as Cronbach and Suppes use the term and a range of other ideas, like common sense, public opinion, Zeitgeist, and so forth, that have been employed to refer to the shared outlook of large groups of people. I shall return to that connection in just a bit.

The second noteworthy feature of the description Cronbach and Suppes offer is its ambiguity or perhaps one should say its complexity, particularly as it pertains to the nature of the change being accommodated. We commonly speak of the fruits of research and scholarship as though they constituted mere additions to knowledge or perhaps substitutions for what is no longer true. The process, in other words, is usually described as being straightforwardly additive or subtractive and as taking place more or less mechanically. But that’s not the way Cronbach and Suppes speak of it at all. Their words encourage us to envision something far more complicated and dynamic. They speak of a restlessness emerging out of discussion. They talk about findings that seem to conflict, which implies the possibility of being mistaken. They depict investigators as being puzzled, as noticing things that as yet cannot be described in words, phenomena for which language is lacking. They speak of boundaries getting blurry, of topics merging into one another. Investigators, we are told, “find themselves assembling what they know in a new way,” almost as though they were doing so unconsciously or perhaps even against their wills. When it comes to how educators in general and the public at large might be influenced by these intellectual advances, Cronbach and Suppes’s choice of language is especially revealing. Nonspecialists, we are told, will find the new concepts stimulating and clarifying, not simply informative or corrective, as might be the case if such alterations of view were looked upon as mere additions to or subtractions from what is already known.

All of this calls attention to the multiplicity of functions that conclusion-oriented research might play within the psychological economy of investigators, practitioners, and the public at large. I am reminded in this connection of some of the benefits William James (1901) once claimed for the study of psychology when he was addressing an audience of classroom teachers. “[I]t saves us from mistakes, he said,

It makes us...more clear as to what we are about. We gain [in] confidence...as soon as we believe that [our way of working] has theory as well as practice at its back. Most of all, it fructifies our independence, and it reanimates our interest.... (p. 11)

I am not sure that the study of psychology, or of any subject for that matter, can do all that for everyone but it certainly is true that such things do happen from time to time and when they do the effects are memorable. The question is: What can we as researchers do to increase the likelihood of that happening?

Cronbach and Suppes offer two sets of criteria that might be used to judge whether a particular study or set of studies stands a chance of affecting the prevailing view. One takes an external perspective, asking how such studies might be judged from afar, so to speak; the other takes an internal one, asking how they might be judged close up, almost from the inside. The externally oriented recommendations, directed chiefly to funding agencies, call for diversifying the range of studies to be supported (thus hedging the agency’s bets) and for choosing “broadly educated reviewers” to help make the judgment. The internal recommendations call for well-crafted studies, ones that are rigorous in method, thorough in outlook, and cumulative in their programmatic linkages, which means giving preference to researchers who have demonstrated an enduring commitment to a problem or set of problems.

What I find interesting about these internal criteria is how much they resemble the standards we might apply to the judgment of any work of quality or any proposed undertaking, whether it be research, a work of art, a surgical opera-
tution, a form of human service, such as teaching, or the repair of a broken pipe under our kitchen sink. We want all such work to be done thoroughly and rigorously and, other things equal, we would usually like it done by someone with a reasonable amount of experience who was committed to what he or she was doing. The detailed standards under each of those headings will vary from one context to another, true enough. Thus, what makes for a rigorous and well-crafted poem, as an instance, is surely not the same as the criteria we use to judge the rigor of a piece of survey research, which in turn have little to do with the way we determine how well the plumber has done his or her job under the kitchen sink. Nonetheless, the overall worries about attention to detail, sensitivity to all that is going on within a particular context, making sure that nothing essential has been overlooked or forgotten, tidying up loose ends, and so forth, seem to be pretty much the same in all of those situations.

Now the question is whether that is all that can be said. Are the standards of good work in general, which is what Cronbach and Suppes seem to be talking about, sufficient to identify which conclusion-oriented studies have the greatest chance of affecting the prevailing view? That such standards are terribly important cannot be denied. Yet we all know of studies that have had a tremendous influence within the educational community and even within the public at large and yet have been badly flawed and poorly executed from a technical point of view.

I recall in this connection an experiment undertaken some years back by a sociologist whose name I have since forgotten. In preparation for a speech he was to make before a professional society he chose several of the classic texts in sociology, studies whose findings and whose theoretical frameworks had shaped the field, and he sent them out to be reviewed by a bunch of young but well-trained newcomers to the profession. The results were predictable, which is doubtless why he tried such a thing in the first place. The reviewers tore the classics to shreds. They exposed all of their books that shaped the field of sociology, the ones that set the terms, the studies chosen for review certainly had altered the prevailing view.

Were we to try a similar experiment within the field of education today, I suspect the results would be pretty much the same. Think, for example, of what someone with a knowledge of modern statistics would do to the work of E. L. Thorndike or, coming even closer to the present, imagine what a well-trained and hard-nosed ethnographer would make of Piaget’s casual observations of his own children. In a word, they would become mincemeat. Assuming that to be so, what are we to make of it?

One possibility is that our standards of judgment have risen dramatically in recent years, which would mean that studies of a quality similar to those that once had a tremendous impact would not create much of a stir today. That interpretation may be comforting to those who put a lot of faith in the power of technique and research design. Moreover, I suspect it may even be partially true. But I find it unconvincing all the same. For I am inclined to believe that the long-term influence of those classic texts depends on something more than technical competence, as important as the latter may be. So too the potential impact of the research and scholarship we are engaged in today.

What might that “something more” include? Obviously no one knows for sure, but it seems to me there are one or two fairly obvious things we might do if we took seriously the goal of having our work reach a wider audience of both specialists and nonspecialists. The most obvious of all is that we could try to improve our writing. We could try to write clearly and well and by so doing make our work more accessible and perhaps more interesting to others. This need not entail watering down what we have to say nor need we wind up writing for the masses, as Cronbach and Suppes might put it. No matter how hard we try, most of our articles and books will likely appeal to limited audiences, and that is probably as it should be, for we certainly don’t want to simplify and popularize our notions to the point of having them lose whatever richness and force they might otherwise have. So let our writing be as difficult as it has to be to deliver its message in full. But at the same time let it be as user-friendly as possible, if I may borrow a phrase from a language whose native speakers are not very friendly when it comes to their own linguistic practices.

Let our writing be as difficult as it has to be to deliver its message in full.

This may sound like the kind of advice you might hear from a high school teacher of English, which I hasten to say I have never been, but I make no apology for it sounding that way. As an editor of an education journal and of a forthcoming research handbook, I regret to report that far too many within our research community do not write nearly as well as they are capable of doing when called upon to tighten up their prose. We could all stand an occasional prod in that direction, I am sure.

“Fair enough,” one might reply, “but is that all we can do to increase the likelihood of our research making a difference in the world: improve our writing? Even if we all deserve a rap on the knuckles now and again for our tangled sentences and our excessive use of the passive voice, surely there must be other avenues of improvement we might explore as well.”

And so there are. Yet I confess that all the ones I can think of are related to writing in one way or another, which should come as no surprise. For if we are to change the way people think about educational matters, which presumably is what we hope our conclusion-oriented research will do, we have no choice, it seems, but to rely rather heavily upon the efficacy of the written word, which not only entails trying to write as well as we can but also means paying close atten-
tion to what we write about. And *that*, in turn, may mean choosing to write about matters that heretofore have gone relatively unexamined. It may also mean changing the angle of vision from which we look at our own work. The latter two possibilities are worth exploring briefly.

To speak of how we might improve our writing either by choosing different subjects or by looking at our own work differently takes us far beyond the province of both the grammarian and the editor, that infamous pair of linguistic watchdogs who seem to take a perverse delight in nipping at our heels whenever we stray from the confines of accepted practice. When, for example, we begin to consider what the various disciplines within the social sciences have to contribute to educational understanding, we enter a realm of discourse in which the techniques of writing and even the topic of writing *per se* takes a back seat to more substantive considerations. Yet even from the back seat our thoughts can be driven toward what are essentially literary matters, as will be seen.

I noted at the start that a lot of changes were taking place within the social sciences of late, the main features of which are so familiar as to require only a passing reference. There has been a new infusion of thought from abroad, particularly from France and Germany. Terms like *poststructuralism*, *deconstruction*, *interpretivists*, *hermeneutics*, *critical theory*, and more have entered our vocabulary. New allegiances and alliances have emerged. Some of the gaps between the humanities and the social sciences are being leap across, if not permanently bridged. Ethnographic studies have risen to a new position of prominence. What is sometimes referred to as *mainstream social science* (usually with pejorative intent) has come under attack by critics of various kinds. Conversation within certain quarters of the social science community has become newly politicized. Near-forgotten scholars have been rediscovered—Giambattista Vico, Wilhelm Dilthey, and Antonio Gramsci—and new ones lionized—Hans-Georg Gadamer, Jurgen Habermas, Michel Foucault, Jacques Derrida, Julia Kristeva, Stanley Cavell, Richard Rorty, and Charles Taylor, to name nearly a baker’s dozen.

Within our own community of educational research and scholarship we witness a widespread reaching out and borrowing from these new or newly discovered scholarly sources. There are related developments as well. The preeminence of psychology as the social science from which educators have most to learn has been sharply challenged. The study of educational history has undergone a renaissance and is now one of the more vigorous subdisciplines within education. Educational philosophy, having passed through the nit-picking phase of linguistic analysis, is once more tackling issues of broad interest. Anthropological perspectives and ethnographic investigations of all kinds are flourishing.

These developments have occasioned discord in some quarters, the way rapid and significant change is almost bound to do. A vigorous argument has broken out over the relative merits of qualitative and quantitative approaches to research. Indeed, the perpetuation of that argument seems to have become almost a cottage industry. Feminists continue to chip away at the crumbling bastions of the male-dominated research establishment, offering new perspectives on old problems. Topics relating to race, gender, and ethnicity have almost literally elbowed their way to the top of today’s educational research agenda. Other struggles have been more localized and perhaps more ephemeral as well. There was a flurry of concern a few years back, for example, over whether there is such a thing as a field of curriculum or, less metaphorically, whether curricular studies constitute a legitimate domain of scholarly endeavor. And so it goes. Challenges and counter-challenges enliven the discourse of a research community that once devoted itself almost exclusively to issues of tests and measurement, a community that at least to some outsiders looked as impenetrable and as stodgy as the interior of a private club.

Beyond welcoming most of these changes, as I believe proper, what shall we make of them? How do they bear upon the distinctions introduced in these remarks? One connection I have pondered, though have not yet formulated as well as I would like, links the contextual or situational emphasis of much of today’s research with what Cronbach and Suppes might call its conclusion-orientedness. Time, plus my own uncertainty about what to say, allows for only a fleeting and provisional statement about that connection.

In recent years we have witnessed a growing interest within our research community in the use of techniques and scholarly traditions that provide a close look at the everyday affairs of educational practitioners and those they serve. At the same time we have also witnessed a renewed interest in our educational past, in how things came to be the way they are. These two trends, if they may be called that, are paralleled by a corresponding decline of interest in the part of many of us in what used to be looked upon as our main business, which was the discovery of rules and principles of teaching and of running schools that would prove to be universal or nearly so in application and invariant or nearly so over time. That dream of finding out once and for all how teaching works or how schools ought to be administered no longer animates nearly as many of us as it once did. In its place we have substituted the much more modest goal of trying to figure out what’s happening *here and now* or what went on *there and then*. This does not mean that we have given up trying to say things that are true from situation to situation or that we are no longer interested in making generalizations. But the kind of truth in which more and more of us seem interested these days takes a very different form than it once did. As Geertz has pointed out, the change is not so much in our notion of what knowledge as it is in what we want to know.

Does this shift translate into a movement from decision-oriented to conclusion-oriented research? Not quite, for a focus on local knowledge (to borrow yet another term from Geertz) can be just as oriented toward solving the day-to-day problems of practitioners as can the search for invariant principles of practice. Nonetheless, the move toward more descriptive strategies, with their heavy dependence upon narration, naturalistic observation, and all of that, does signal a more relaxed attitude toward the relationship between the research community on the one hand and the world of practice on the other than has been true in the past. That change of attitude has as one of its components an increased willingness to look and listen appreciatively to what goes on in schools, taking into account not only what is “out there” in some objective sense but also what the researcher himself contributes to what is seen, all of which brings me finally, to the question of how my own research bears upon the distinction I have been discussing.

My two research assistants and I have been working with 18 teachers (half in elementary schools, the other half in high
Each of those tropes captures an aspect of what it is like to view the ordinary in a fresh light, to see it from a different angle, to pry beneath its surface, to catch it by surprise. For the moral messages that teachers and schools might be giving to students, whether intentionally or not, has led me to a subsidiary conviction, which is that in at least one very important respect we researchers are no different than the teachers and the institutions we are studying. We too are constantly sending out messages of all kinds (many of them moral) to those who read and interpret our work. Also like teachers, we are probably only partially aware of the messages we send. But to talk of messages sent and received, as our moral life project has also taught us, is to indulge in yet another metaphor, one whose effect is liberating and constraining at one and the same time. It is true that more meaning is often read into our research efforts than we have put there. This being so, it is important that we seek to understand what meanings may be extracted from what we have done, what interpretations placed upon it, whether we intended them or not. Hence the value of thinking about unintended messages. At the same time, the meaning and significance of our work as gleaned from our writings is not totally reducible to messages units of one kind or another, whether intended or not. In other words, the metaphor of message units fails to capture some of what we should be talking about as we seek to understand the impact our research might have on others.

What do we send beside messages? The power of the metaphor makes it hard to abandon, thus I am tempted to extend it by saying that along with messages we also transmit "static" or "background noise" or something like that. But that's no good because "static" and "noise" are mere interferences, they are conditions to be avoided or eliminated, whereas what I want to call our attention to here does not always have a negative connotation. Better to abandon the association to telegraphy and electronics entirely and move to a much more open-ended conception of how meaning emerges from our encounters with complex symbol systems. Doing so, I would say that what we communicate to our audiences besides "messages" is something like "impressions," quickly adding the reminder that an impression need not be fleeting and ephemeral as connoted by the familiar phrases "quick impression" and "mere impression." On the contrary, some impressions, even "first" ones, wind up lasting a lifetime.

The work of our research community impresses its varied audience in countless ways. They see it as important, frivolous, eye-opening, mind-boggling, serious, dull, elegant, sloppy, refreshing, interesting, boring, scientific, pseudo-scientific, pretentious, modest, ambitious, exciting, frustrating, puzzling, and more. Moreover, the point is not that different people may see the same work in different ways, although that certainly can happen. Rather it is that some impressions are justified and deserved, whereas others are not. These impressions, which may also be thought of as judgments, have other properties worth mentioning. It is true that more meaning is often read into our research than we have put there. This being so, it is important that we seek to understand what meanings may be extracted from what we have done, what interpretations placed upon it, whether we intended them or not. Hence the value of thinking about unintended messages. At the same time, the meaning and significance of our work as gleaned from our writings is not totally reducible to messages units of one kind or another, whether intended or not. In other words, the metaphor of message units fails to capture some of what we should be talking about as we seek to understand the impact our research might have on others.

The impressions our research creates relate in a variety of ways to phenomena that philosophers and psychologists, among others, have spoken of for generations. They are close to what John Dewey (1934/1958) called "the immediate quality of experience" (p. 119). This was the expression Dewey used to refer to the very first phase of every encounter with...
the world, the portion that occurs before we have had time to reflect upon the experience itself and separate out its elements and parts. A similar notion figures prominently in the writings of those who discuss the functions of art, as Dewey himself did of course. Nelson Goodman, for example, would have us understand first of all that every work of art symbolizes, and secondly that much of it does so by expressing qualities that are not merely represented by the work but that “show forth” from within. “What some symbols symbolize,” Goodman insists, “does not lie entirely outside the symbols” (p. 60). Rather, it is embedded within the work itself. The work expresses or exemplifies (two terms that have a special meaning within Goodman’s frame of reference) that portion of its significance. The same is true, mutatis mutandis, or so I would argue, for those works we researchers produce. They too can be read as expressing qualities that may or may not have been placed there consciously by we who create them.

And what if that is so? How does it apply to the topic at hand? It means, I believe, that our success in altering the prevailing views of non-specialists, and by “our” I refer now to all within earshot, will depend at least in part on our willingness to turn our vision inward so to speak, even as we maintain our focus on educational phenomena whose reality is broadly shared. This change in our customary way of looking at things calls upon us to become self-reflective about our research goals and methods and what they mutely and perhaps inadvertently communicate about our enterprise to the world at large. It requires, among other things, that we conscientiously explore our most deeply held assumptions and presuppositions and that we do so with a directness, a candor, and a tenacity that has seldom typified our work in the past.

It was Yeats (1958) who said “God guard me from those thoughts men think/In the mind alone; He that sings a lasting song/Thinks in a marrow-bone.” (p. 326). Yeats’ prayful entreaty issued from the soul of a poet, yet its truth, it seems to me, applies to us all. “He that sings a lasting song thinks in a marrow-bone.” Those are words we all might take to heart. But how? What does it mean to do so, especially if one is not a poet? To put a point on it, how does a conclusion-oriented educational researcher, of all people, think in a marrow-bone, or dare one even try? That question undergirds the distinction bequeathed us by Cronbach and Suppes some 20 years ago. I call upon us all to answer it today as best we can.

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