The Significance of Significance

The rapid tempo of change in our culture, particularly in recent years, has brought about increased questioning of the content, processes, and goals of education. While this wave of questioning has led us to considerably more knowledge about education, what we also seem to be realizing is the tremendous disparity between that understanding and what we need to know.

Who can say, for example, with any certainty, what today's sixth grader ought to be doing, who should teach him, what his classroom should be like, or even whether he should be in a classroom? A few enlightening opinions are sometimes offered; however, most schools continue to operate according to what has been traditionally done, lacking a historical assessment of where we have been and unmindful of where we should be going.

Of course there is a great deal of uncertainty about the future. Yet the major task of the decision makers in the schools and universities will be to develop new, more effective approaches to schooling in the face of this uncertainty. Likewise, a major partner in this enterprise will be the research specialists whose charge will be to share in the responsibility of charting directions and assessing results.

At present, the prospects for such coordination of efforts are threatened by a division of opinion about what constitutes useful research, a dichotomy implicit in the title of this article, "The Significance of Significance." I shall not argue for the abolishment of statistical significance as a criterion of research, but rather seek to redress what I see as an imbalance of priorities in educational research today.

For the research specialist, "significance" is statistical measure of the extent to which his findings may be relied upon, clearly implying that when there is a high level of significance an appropriate goal has been attained. However, when significance becomes the paramount goal in the research effort, the quest for significance begins to produce some unhealthy consequences. The pursuit of significance tends to have the effect of forcing the researcher to so limit, simplify, and refine the data with which he deals that his findings, although statistically significant, offer little application to practical educational situations.

Furthermore, the press for statistical significance often determines the research-
er's choice of study, leaving him to choose those issues that can be "well researched" rather than the more complex, more murky areas which may present research difficulties but which carry greater promise of contributing to decisions on pressing educational issues. My fear is, then, that researchers have been so concerned with doing "significant" research that they have lost track of the need to research the right problems.

Challenge to Researchers

What are the right problems? While they have been expressed differently by many people in varying situations, they all seem related to four basic questions:

1. How do people learn?
2. What should people learn?
3. What should people learn in school?
4. What do people learn in school?

An almost endless list of more specific questions can be formulated from these basic questions. For instance:

1. How differently do children learn? How can we assess how a given child learns? How can we assess teacher style and match it with individual learning style?
2. How long should the school year be? How long should the school day be? How many days a week should English be? How do we know when they are through?
3. What values and attitudes are being communicated in the schools? How might we go about changing the values communicated if we so desire? How is racism being transmitted through the schools? How do schools become nonracist?
4. What should a $10,000 a year teacher do? What should a $5,000 a year teacher do? How do children learn how to learn? Can we use students as teachers?

Clearly, research into these intricate and complex areas will not yield the clean definitive and conclusive kinds of studies which normally characterize "good" research. However, such research can reach tentative hypotheses which point the way to fruitful areas of investigation, and can provide data—statistical, documentary, and design—that will form the basis of subsequent projects.

Some may object that research which does not yield a "significant" result may be inaccurate and misleading. The danger certainly exists, but is not insurmountable. First, in view of the ambiguity and uncertainty surrounding these large educational issues, it is the responsibility of the researcher and decision maker to pool their collective knowledge and experience to determine if a tentative hypothesis points in a promising direction for future action.

Second, if schools follow the action research model described in this article rather than a prepackaged approach to innovation, decision makers will have the opportunity to change or abolish unproductive programs based on data generated by those programs, thus minimizing the dangers of inaccurate research findings. The most important consideration, however, is that we must not hold back from researching the right areas simply because we cannot guarantee conclusive results.

In addition to dealing with important issues, research must also influence practice if it is to be considered significant research. The results of research, often very good research, do not find their way into practice. Decision makers do not find research very useful. One clear indicator of this can be found in the fact that very few superintendents’ or deans’ offices include a research component. My contention is simple—both decision makers and researchers must change their ways so as to make it inconceivable for an educational leader to be without a full-time research effort to assist him.

Dual Responsibility

The responsibility for this lack of cooperation lies with both the decision makers and the researchers. Decision makers often have not made the effort to understand research studies and thus are leery of using them. Administrators often have biases about educational policies which blind them to research that would challenge their opinions...
and actions. Others have grown weary of change and the pressure for change.

Research specialists, on the other hand, are often not interested in the practical aspects of involvement and continuing change in school systems and universities. Furthermore, the failure of “significance” leads them away from the kind of ongoing evaluation which administrators would find useful in designing and developing new programs. The potential benefit to education that closer cooperation between decision makers and researchers can offer is enormous and worthy of any efforts to reconcile the breach.

I envision a major failure of innovation in the schools being resolved through a teamed effort of researcher and decision maker. This effort should take the form of what I choose to call an “action” research design—in which the decision maker receives feedback on the progress of a new program during the course of the program. With such data, appropriate changes can be made in the program while it is in progress, creating a growing and learning situation rather than a win-lose, all or nothing environment for innovation.

An action research design will help decision makers face the turmoil and uncertainty which accompany most change attempts with some idea of whether the pains involved are symptomatic of growth or disintegration. Without such data there is a tendency to fall into the all-too-familiar pattern of following advances into uncertainty with retreats to the security of “what we have always done,” because there are no indications available that might show that the new is any better than the old. Thus, far more ongoing action research is needed to give decision makers the confidence to follow a promising path in the face of turmoil and uncertainty, to make improvements in innovative programs in progress, and to cut off unpromising alternatives which otherwise would sap the energies of faculties, students, and administrators.

I would also like to see researchers cooperate more closely with decision makers as a force to quicken the pace of innovation in the schools. Since the most convenient and available data sources are existing programs, research focused on them tends to increase the sense of investment in the existing themes and leads to the development of variations on old themes rather than the creation of new approaches based on new assumptions potentially more appropriate to the demands of the present and future. It would, therefore, be most appropriate for researchers to press for more diverse, more innovative programs in the schools as a real data source for charting the future of educational processes.

In summary, I am implying a broader test of significance in research by suggesting that educational research is only significant if:

1. It provides new insights into areas of pressing educational concern;
2. Decision makers find it useful and make decisions based upon it; and
3. It helps decision makers to maintain the process of growth and renewal in their organization.

Existing statistical definitions that have proved their value should certainly be maintained. I offer the above tests of significance as complementary additions to assist in reducing the knowledge gap now confronting education.

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