

CAN RESEARCH IMPROVE TEACHING?

Summary of a Lecture given by Lawrence Stehhouse at the
Scottish National Conference on Curriculum and Evaluation
for P.E. Teachers. January, 1979

Research in such disciplines as philosophy, psychology and sociology might contribute to the improvement of teaching: by clarifying - or possibly settling - issues about the appropriate aims of education, by providing insights into the process of learning and the development of the learners, and by defining the social and institutional contexts which the teacher must take into account. This is to say that the results of research in these disciplines might have implications for educational practice. A good case could be made to support the assertion that practice has been affected in this way, but the process seems to be a rather indirect one and perhaps unselfconscious: a matter of the gradual changing of attitudes rather than of deliberative process.

Educational research promises a more direct contribution, and its growth dates from the turn of the nineteenth and twentieth centuries when it seemed possible to apply not merely the results but rather the methods of social science - and particularly of psychology - to the problems of teaching and learning in schools. A major breakthrough can be associated with the work of R.A.Fisher who developed a paradigm of field research in agriculture which has been widely adopted in education.

Central to Fisher's work is the idea of random sampling. In order to conduct experiments which have general application we must study samples - of oxygen or iron filings or hydrochloric acid - which are comparable with each other and which can represent a more general population - all oxygen, all iron filings, all hydrochloric acids. This condition, which is met in chemistry by control of purity based upon analytic techniques, presents

difficulties in the biological, and even more in the social sciences, both because of variability in the units of study - plants, animals and people - and because of the variable contextual conditions which constitute a complex ecological factor.

Fisher saw that in agriculture uncontrollable variations of this sort could best be handled by random sampling, that is, by choosing a sample from a population it was intended to represent on such a basis that each unit has an equal and independent chance of being selected. Since randomness instates the rule of chance the mathematics of probability can then be used to discriminate whether the differences observed in populations are 'significant', that is, highly unlikely to be due to chance. Subsequently, Fisher's pattern of experiment has been adapted for situations where true randomization has not been achieved.

In its agricultural application this experimental paradigm has been used to test seed strains and fertilizers and feeding stuffs and levels of feeding one against the other in order to discover the procedure which maximizes yield against investment. It underlies the systematic treatment of crops and the factory farming of livestock and poultry.

The attraction of this experimental design for the educational researcher schooled in a social science tradition is obvious. It apparently allows one to follow experimental or survey procedures without full control of all the variables - some of which are sampled rather than controlled - and without the guidance of fully developed theory. Since the kind of theory from which hypotheses can be deduced and tested as a means of testing the theory is hard to come by, there are obvious attractions in testing educational actions against one another in the same way that alternative agricultural procedures may be tested. We might be able to decide on the best course of action without understanding at a theoretical level why it is best.

But, of course, we must have a criterion by which to judge what is best. In agriculture the criterion is gross yield, the weight of the crop per acre or square yard. In education it is not so easy to see how to measure the gross yield. The researcher has tended to pass this problem on to the teacher, though with the insistence - researchers often seem to have more power than teachers - that the problem be seen and solved in terms of the researcher's devising. Arguing that education is an intentional activity, the researcher has suggested that intentions imply goals. Arguing that learning is change in student behaviour, he has gone on to insist that the teacher should be able to state rather precisely the behaviours the student will be capable of after he has been taught. Specifications of such 'intended learning outcomes' are most commonly known as 'behavioural objectives', and it is intended that they should be precise enough statements to provide constructors of objective tests with a criterion to which to work.

My own view - a controversial, though a widespread one - is that the simplistic logic of this procedure falsifies the nature of education except at very low levels of aspiration. But that is not the issue I wish to pursue here.

Were we to assume it to be a reasonable procedure to prespecify the effects of education for all students uniformly, this approach to educational research would still fall far short of what we need to improve teaching, because it rests upon the assumption that we shall better teaching by adopting a teaching procedure uniformly across all children in our class or more frequently across all classes. But the results obtained in experiments conducted on this model suggest that we shall get the best results by using informed professional judgement and diagnostic skills to differentiate our procedures from class-group to class-group and among the individuals in a class group.

Education is less like agriculture than like gardening. In

agriculture the same treatment is given to whole fields, and so the field contains one single crop. Gross yield is all: the fate of individual seeds or plants does not matter. Not so in gardening. Different plants are cultivated in a garden and treated differently by the gardener. The individual plant is a matter of consideration. So in education to teach excellently we need to teach differently in Wolverhampton and Bourton-on-the-Water and Newcastle-upon-Tyne. We need to teach IIIc this year differently from IIIc last year. And within the class we need to teach individuals differently. It is this differentiation which is the most important challenge to teachers today. We fall far short of what we might hope for: the aspiration to differentiate is not fulfilled. The capacity to differentiate practice in the light of professional judgement is central to improvement in the art of teaching: not least in physical education.

The problem with the classical paradigm of educational research which I have discussed above is that it seeks to present results or conclusions as products of a technical research process and thus to override teacher judgement, appealing instead to research judgement. It might tell us what to do if we were not to place reliance on teacher judgement, nor adopt the aim of improving it. This would be to waste the most expensive element in education: the teacher.

Research cannot improve teaching without helping the teacher to develop skills in a context of judgement. How can research strengthen the judgement of teachers? Certainly, only by appealing to teacher judgement.

Such a view of the relationship of research to the improvement of teaching has implications for the style of research I have discussed critically and for the development of new styles of research.

Cronbach (1975), schooled in the classic research paradigm,

observes that though the researcher 'may reach an actuarial generalization of some power, this will rarely be a basis for direct control of any single operation'. (p.126) In short, generalizations couched in terms of probability do not apply to situations. "When we give proper weight to local conditions", says Cronbach, "any generalization is a working hypothesis, not a conclusion". Thus, what the classic research procedure offers to teachers is not conclusions to accept but hypotheses that need testing.

This implies that the hypotheses which are the products of research need, if they are to improve the practice of teaching, to be couched in such terms as make them amenable to testing in the classroom. If we ask the teacher to pose the question: "Does this more or less widespread observation hold for my particular situation?" we must in our presentation of the observation give him the means to answer the question we have recommended to him. It is one of the great - but often unexploited - strengths of Piaget's work that, since most of his experiments are cast in the form of teaching or assessment, his work is readily verifiable in the classroom.

There is a sense in which hypotheses emerging from experiments in the psycho-statistical mode must be testable by curricular action if they are to be applied by the teacher. A curricular action is one justifiable not only in experimental or research terms but also in educational terms.

But there is a need to supplement experimental styles of research which are sensitive to the needs of application to practice by observational and descriptive styles of research which tutor judgement by extending experience. Here history is the model. It reviews and orders past experience - that of yesterday as well as that of former centuries - and attempts retrospective generalization.

This summary ordering of experience depends upon judgemental rather than mathematical calculation and consequently it demands data accessible to thoughtful consideration. This calls for the realistic portrayal of cases rather than the abstract representation of samples. Cases cumulate, as for example in history we cumulate cases of long-barrow burials or cathedrals or joint-stock companies, but the retrospective generalizations derived from surveying the cumulated cases do not provide predictive generalizations in the classic form: they define rather a gradient of expectation and explore the logic of rarities seeking to explain what gives rise to the unusual. Retrospective generalization, like the study of individual cases, can improve practice only through an application of judgement to the case in question. In short, the capacity of research to improve teaching depends upon - and in turn feeds and strengthens - the teachers professional judgement.

A direct way of stating this would be to say that the application of case-study research requires a comparative study of your own case. You must weigh up your own situation against the accounts of other situations.

I conclude that research can only markedly improve the art of teaching if it:

- i) Offers hypotheses (i.e. tentative conclusions) whose application can be verified because they can be tested in the classroom by the teacher.

OR

- ii) Offers descriptions of cases or retrospective generalizations about cases sufficiently rich in detail to provide a comparative context in which to judge better one's own case.

I also believe that at the moment the improvement of schooling depends more than anything else on the development of the art of teaching.

It is clear that if the teacher is to experiment in the laboratory of his own classroom and study carefully his own case he will have to be given more time for planning and reflection. The great barrier to the improvement of teaching is the inexorable load on the teacher's attention of the burden of present contact hours.