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THE PROBLEMS OF STANDARDS IN ILLUMINATIVE RESEARCH.

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The illuminative tradition, sometimes called 'portrayal', sometimes called 'humanistic', sometimes called 'descriptive', now seems to have got off the ground both in research and in evaluation. It no longer needs to fight to establish itself as an alternative to the 'psycho-statistical' paradigm worthy of consideration; and today I want to look from a position of sympathy at the problem of establishing acceptable standards within that research style.

The traditional paradigm, which I have referred to as 'psycho-statistical' was called in Scotland 'experimental education' and it is closely related to a tradition in agricultural research strongly influenced by the work of Ronald Fisher. Crucial to this was the insight that in field experiments in which variables cannot be closely controlled random samples are to be preferred to samples judged to be representative because randomization allows error to be calculated by the use of the statistics of probability. Thus two agricultural treatments applied to random samples may be tested comparatively for effectiveness against the criterion of yield by establishing a level of significance of difference on which the hypothesis of superiority of a treatment is compared with the null hypothesis. Behind this appeal to probability is an appeal to replication.

The attraction of this research paradigm in an applied field is that a criterion of yield allows discrimination between actions or policies without the need to derive prediction from theory. Educational theory is not developed to the point where it is a reliable guide to action in the way that physics guides engineering; and the possibility of finding

one course of action superior to another without understanding exactly why is, of course, a short-cut. In short, the experimental method classically seen as discriminating through a crucial experiment between competing theories was adapted to discriminate between competing policies for action.

There are, however, many problems encountered by this paradigm. For example, as Walker and Schaffarzick (1974) have pointed out, there is a problem of criteria for the measurement of yield in education and different measures may favour different procedures. I do not intend to rehearse all these problems, but it is worth mentioning that most competing educational policies are advantageous in some circumstances and not in others. Experimental approaches to deciding between actions rest upon the assumption that one policy must be applied to all cases (which is not true in education) or that statistical analysis of trait-treatment interactions can guide differentiation of action (a possibility which remains elusive, see Cronbach 1975). We may sum up the situation by saying that work in the psycho-statistical paradigm offers to do better than professional judgement in judging what best to do, and that in overriding professional judgement it fails to strengthen it. It appeals to research judgement: if the design and conduct of my research is correct, then my results must be correct. If you think they are wrong, then fault the design and conduct of the research.

The important point about the illuminative tradition is that it broke away from this and aimed to appeal to, and hence to strengthen, professional judgement. The aim in the end is professional growth in educators: teachers, advisers, administrators. From this desire to appeal to professional judgement comes a range of concerns about presentation and about audiences. In particular, there is a need to capture in the presentation of the research the texture of reality which makes judgement possible for an audience. This cannot be achieved in the reduced, attenuated accounts of events which support quantification. The contrast is between the breakdown of questionnaire responses of 472 married women respondents who have had affairs with men other than their husbands and the novel, Madam Bovary. The novel relies heavily on

that appeal to judgement which is appraisal of credibility in the light of the reader's experience. You cannot base much appeal to judgement on the statistics of survey; the portrayal relies almost entirely upon appeal to judgement.

The commitment of the illuminative tradition to appeal to judgement has meant that it cannot create samples by abstracting features from cases. Such abstraction is fundamental to the psycho-statistical experiment.

Illuminative research has thus been associated with the study of cases, not of samples. Much of this work is communicated in words, but there is a lot of room in case-study for a quantitative ingredient which is at present too much neglected. We need to ask what dossier of statistics one would gather to place a school within the range of schools: site value; library borrowings; number of visitors, and so forth. Also, the potential of such techniques as time-series analysis needs to be explored. The issue is not qualitative versus quantitative, but samples versus cases, and results versus judgements.

Most of those who have worked in the illuminative tradition have been trained as social scientists in the classic mode and thus see themselves as rebels. From the standpoint of their own training, illuminative research appears breakaway, maverick, provocative, a challenge to academia; and some of the people involved in it encourage this by comparing themselves to investigatory journalists or novelists. Indeed, I have recently received an evaluation from America which goes further and has a musical evaluation on tape.

I grew up in Scotland and took my degree in education here, and that stamps me immediately as a brake on that bandwagon. I want to see that academic standards are maintained in this new paradigm through the development of adequate academic conventions.

I see academia as a social system for the collaborative production of

knowledge through research. Research is systematic inquiry made public. It is made public for criticism and utilization within a particular research tradition, the problem of criticism being that of verification and the utilization problem being that of cumulation. The function of such a research discipline is to enable the modestly intelligent to make a contribution. A physicist addressing the question, "Is sociology a science?" declared: "If the methods of sociology are sufficiently well-developed for a second-class honours graduate to make contributions to knowledge, sociology is a science: if no contribution can be made without outstanding ability, then it is not." This does not mean that there is no room for genius: just that there is also room for honest hard work because it has the support of a discipline. I think it is a tremendous boon to all of us that one doesn't need to be a genius to do something worthwhile in our field.

My concern is that we should be sure that the illuminative tradition builds towards that desideratum. And this means clarity about verification and cumulation.

I reject the analogy with fiction. Fiction aims at a fuller truth, about human life than research does, but gets airborne by shedding the burdens of authentication. It appeals only to what J.H. Hexter writing of the historian's craft has called "the second record": the accumulated experience of the reader. Authentication requires a first record: the documents of the case.

Nor do I think the analogy with investigatory journalism a fruitful one except in very rare cases. The warrant for truth in investigatory journalism depends upon the appeal to hidden evidence which is being used by the researcher and a powerful adversary. We trust the investigation of Nixon because we know that he had the resources to mount a well-ordered, research-based counter attack. Investigatory journalism rests on a debate in which both sides have access to research skills just as the judicial process depends on both sides having access to legal skills.

As an examiner of doctoral dissertations I find no substitute for the building of a research tradition as a basis for verification and cumulation in illuminative research. I think some doctoral students - and some post-doctoral researchers - are declaring themselves 'illuminative' to escape the pressure of standards and that cannot be allowed to continue. At the same time most doctoral supervisors are unsure what standards to advocate in illuminative research.

My front runner as discipline from which we must learn if we are to found illuminative work properly is history.

History finds verification on the appeal to the publicly accessible source. While science tends - logically at least - to rest on the possibility of replicating experiments, history rests on the replication of judgement of the same data, the possibility of persuading the reader who knows the sources of the cogency of an interpretation. To achieve this historians invented the footnote, a running record of appeals to documentary sources, accessible alike to the reader and the writer. That does not mean that every reader pursues these leads, but every reader can, and scholarly critics do.

Now when I examine a doctoral student's thesis I want to act as a scholarly critic. I find myself confronted by an illuminative thesis and saying to myself: "I'm worried about this point: what does the data look like? Is it in books or on cards? Is it indexed? How much of it is there? Are there tapes? How many? How many hours did it take to listen to them? Do you have notes on them or transcriptions?" Now, at the moment I'm quite tender on this problem, because it is not publicly clear within the academic community what the standards for this kind of work should be, and this leaves the student in a very exposed position. And too the supervisor. If you are in the experimental paradigm, you know how to guide the student about research methods and about presentation of his work (though I must say that the case of Cyril Burt shows how careful we have to be even in an apparently secure convention). If you are working in the illuminative paradigm, methods are much more problematic. We must give more guidance.

Two research traditions seem to be directly relevant to illuminative work: the ethnographic and the historical. And in fact perhaps we need to draw on both.

I know less about ethnography, though it does seem to me that within it some of the problems of illuminative research are still unsolved. Ethnography started in the study of situations unfamiliar to the student, mainly in preliterate societies. Its prime method is participant observation in which the researcher becomes absorbed into the meanings of the social setting in which he is placed. I am worried about the ethnographic tradition on two counts: first, that few participant observers make their field notes available to others, and second, that ethnographers go into different communities and impose upon their accounts of them concepts - such as 'ritual' or 'rites de passage', for example - which are the vocabulary of ethnography rather than of the society which is the subject of study.

The problems of ethnography are compounded by the fact that most societies have been studied only once or twice. Levi-Strauss (1963) looks forward to the time when each community will have been studied by many different ethnographers so that there will be a process of verification of the style operating in history where many historians study the same sources. But this is not easy to achieve. Over time communities change or are even swept away. The past cannot be revisited and the preliterate past has left no indigenous records. Educational transactions occurring day-to-day also yield sparse records. The only way I can see to tighten up verification in ethnography is to preserve the field-workers' records. At doctoral level I think that we need the ethnographer's notebooks to verify his thesis. And if at doctoral level, presumably at more advanced levels.

Some would say that there is an alternative approach: we should build theory and see individual studies as testing theory by replicating applications. This certainly will not pass muster at doctoral level for it involves only a gradual discarding of individual studies which do

not prove theoretically significant; and the doctoral thesis needs to be judged formally and on a day. In educational work I have a further reservation. Ethnographic theory tends to be esoteric and inaccessible to the actors in the situation studied. There is already criticism of social anthropology as tending to increase the power of the community to which the ethnographer belongs without making a similar contribution to those who are studied. In a professional school research should feed practice and hence should be accessible to practitioners. This calls for parsimony of theory, and theory which is within the literacy of the actor.

A demand of this sort leads us naturally towards history, which is the most accessible of studies. We turn to histories of art or football or country life because they further our understanding by retrospective generalizations and summaries of experience which ask for little technical language other than that of the subject - art, football or country life - with which the interested reader may be presumed to be familiar.

Case-study in a historical tradition would attempt to treat of education in a language comprehensible to the educator (though it might aspire to build out that language). Its verification would depend upon making its sources accessible and on footnoting them so that readers could find their way around them.

Now, an ethnographer could work in a quasi-historical way by preserving his sources and citing them in his case-study. In my terminology there would be reference in a case-study to a case-record which lay behind it and was accessible at least in microform.

However, from a historical point of view the preference would be for broader documentary studies. The distinction is this. The ethnographer in fieldwork acts as a participant observer and uses his specific experience of participation and observation in that community to interpret it while he is part of it. He then writes up his account

from his notes. The historian on fieldwork gathers documents or creates them by interview exercising a parsimony of interpretation in the field and then sets about reflective interpretation of the study in the light of a second record he believes he shares with his chosen audience; in the present case, educators rather than historians.

This means that the historian will prefer other people's observations to his own, that he will work through interview supported by documents produced by the school or classroom for its own purposes and that he may use photography (as indeed may the ethnographer). He is essentially attempting to gather the perceptions and understandings of the participants in the situation he is studying and to soft-pedal his own while in the field. Some people would call this approach phenomenological, but the term phenomenology merely signals a particular awareness of a long-established approach to understanding.

The yield of fieldwork is a case-record which is then used as an historian would use his sources. Such a record can readily be made available to others through photocopying and microform and it seems to me to provide an acceptable, though by no means a perfect basis for verification. I believe that all doctoral students engaged in case-studies should be asked to make such a case-record available, whether they are working in an ethnographic or a historical tradition. I also believe that the historical approach is more accessible at doctoral level and may be a stage of training for an ethnographer working in his own society.

Case-records of the kind I have in mind would provide a foundation for cumulation. For example, if I had a doctoral student undertaking a case-study of a comprehensive school, he could use case-records of other comprehensive schools just as, if I had a doctoral student working on the history of an abbey, he could use work on other abbeys. And behind this lies the possibility of general surveys working across case-records, perhaps of such themes as mixed ability teaching or headship or school assemblies.

This prospect points to the desirability of lodging such records in a national educational records archive where researchers can consult them and from which they can obtain microform copies for use in their own libraries. To be useable in this way records would need to be indexed, but this is probably true even at the level of doctoral verification.

I am trying to see my way forward to establishing such an archive in England, but I believe our tradition in Scotland may make it an easier task here. For this reason I am coming to you with this suggestion: if there are people working in Scotland on descriptive case-studies based on contemporary sources, what is going to happen to their data? Could it be used to found a Scottish Contemporary Educational Records collection? What would such an archive look like? What is the potential of microfiche for such an archive?

Could this Association do anything to further such a development? If so, you would certainly strengthen my hand in attempting to found a parallel archive in England. It would not be the first time that Scotland has led the way.

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