CASE RECORDS AS AN ARCHIVAL RESOURCE FOR EDUCATIONAL RESEARCH:  
AN INTER-INSTITUTIONAL PROGRAMME.

1. The Context and Presentation of this Proposal.

This proposal for a programme of research follows from a proposal for a project which provides a foundation for the programme, but which is not essential to the programme (nor unjustifiable without the programme). That project is concerned to produce four exemplary case records for discussion at a conference to be held at Easter 1979 (and the project proposal includes financial provision for that conference). The present programme proposal involves 10 institutions of higher education in England, and the assumption is that the participants in the programme would be the participants at the 1979 conference which marks the conclusion of the project providing exemplary case studies. Soundings have been taken and there is no doubt of the willingness of institutions to take part.

The present proposal is for an English programme. There is a background assumption that parallel developments would be desirable in Scotland, Wales and Northern Ireland, but that these should be conducted within the different educational traditions of those countries. An international proposal is also being drafted by the principal investigator. This invites each nation involved to contribute four case studies of comprehensive schools. It is hoped that this will both make possible an international survey of problems and possibilities in comprehensive education and provide each participant nation with an opportunity to evaluate the utility of the case record methods to be tested domestically in the present proposal. A supplementary proposal will be submitted to an American source to support the participation of four American scholars in the English programme (if this proposal attracts funds). This will not only make a contribution to the English programme, but will also provide an opportunity for American participants to evaluate the relevance of the approach to the development of educational research in North America.
The present proposal is set out as follows. Section 2, immediately following, provides a summary introduction to the aims and procedures of the programme. Section 3 makes the case for case study, while Section 4 makes the case against case study, diagnosing weaknesses to which this programme is an attempted response. The major problems which emerge are treated in the following sections:

5. The problem of verifiability; 6. the problem of cumulation; and 7. the problem of application. Section 8 describes the structure and organization of the programme, and outlines the responsibilities of staff and institutions within it. Section 9 outlines the promise and the problems of establishing a contemporary educational records archive, noting that the problems need to be tackled within the programme. Section 10 gives an outline plan of the programme. References are provided at the end of this section. The accompanying paper was sent to contacts in institutions who were being approached to establish the feasibility of inter-institutional collaboration.

2. A Summary Introduction to the Aims and Procedures of the Programme

This proposal is for an inter-institutional programme of research and of conferences allowing an opportunity for reflection on that research, the general purpose of which is to lay a foundation of disciplined conventions for the conduct and utilization of case studies in education. It is an attempt to create a tradition for the descriptive study of cases as supportive of research as is the existing tradition for the dimensional and inferential study of samples.

Among the criteria such a tradition should satisfy are these:

a. It should show promise of yielding 'intersubjectively verifiable cognitions' and of supporting the cumulation of the findings of a multiplicity of researchers; that is, it should be public and communal and accessible to critical appraisal.

b. It should be useful to those involved in the conduct of education; that is, it should be practical and it should appeal to experienced judgement rather than override it.

c. It should offer to those who work within it, either as
researchers or as educational practitioners, the possibility of personal and professional development, adequate safeguards from exposure to invasion of privacy and access to the conventional rewards of the profession: that is, it should be motivationally realistic.

Case study resting upon the observation of educational phenomena implies a respect for the particularity of those phenomena. The crucial characteristic of the approach is that a case is taken to be merely exemplary of its class, but not, as is a sample, representative of it for the purpose of inferring generalisations holding in a target population. The utility of cumulative case study to action is in the application to a particular case of intelligence gleaned from experience across cases and not in the application to an additional case of a generalization true of all known cases. The generalization involved is a retrospective generalization of experience so far. The detailed inference is drawn by the actor and is case-made by the conditions of his own case.

The present proposal would involve 10 institutions of higher education in the case study of 10 primary, 10 middle and 10 secondary schools. (See Section 8. The Structure and Organization of the Programme). The immediate product would be this number of case records, lightly edited versions of the field data derived from interview or participant observation. Such records would probably be 392 to 980 pages in length (equivalent to 4-10 microfiches). Potentially, they would form the basis of a contemporary educational records archive (CERA) and it would be one task of this programme to explore the feasibility of setting up such an archive to which educational researchers would have access. It would be necessary that the archive be indexed, and retrieval could be computerized. Schools would clear the lodgment of anonymised case records in this archive.

Each of the case records produced within the programme would be compiled by a different researcher, the common ground being that each would agree to work to the criterion that a case record is to be judged by its usefulness as a data source for others. Given
the acceptance of this criterion, each worker would have freedom to use whatever methods and approaches seemed to him to be best. However, exemplary case studies are likely (this is contingent on the funding of the project mentioned above) to be available for discussion at a conference which will be held at the beginning of the programme, and participant workers will be asked to make their positions clear when they present their studies. The criterion which case records should attempt to meet can be grasped more fully by reference to the canons of criticism of evidence in history. The problem for the field worker/record compiler is the self-conscious production of a good "edited primary source".

The case records would be used to satisfy three publics: those working in the schools concerned, educational researchers and those responsible for or interested in the conduct of education.

When the contract for the study was being negotiated, each school would be asked to undertake to attempt to use the fruits of the study domestically. The pattern of utilization would be negotiated individually with each school and there would be no pressure provided towards uniformity. For example, a school might use the information provided by a case record or case study or consultant's report as a basis on which to build an evaluative report on itself to its governors, or as a document from which to work out a development plan, or as a starting point for one or more staff working parties charged with studying and making recommendations for dealing with specific problems or lines of development. The case worker might service such responses by presenting a copy of the case record to the school, subject to clearance by those involved. Alternatively or additionally, he might provide an interpretive case study using, and citing, the case record as a source. Such a case study would probably become the possession of the school in the sense that it could not be published without the school's consent, though possession of copyright would be negotiable by the researcher. A third possibility is that he might offer consultancy either verbally or by means of a written report, possibly responding to a brief or series of questions drawn up by the school.

The initial research use of the archive would be as a basis for
monographic studies of particular themes or problems written by participant case workers. Each of these would have the right to nominate a topic in which he is interested: deputy headship, conduct of staff meetings, pastoral care, school and work, mixed ability teaching, learning to read, discipline, the teaching of a subject and so forth. For, say, a year after the opening of access to the archive each person who had contributed a case record would have a sole right to use the archive for the production of a monographic study of his nominated theme.

This is the minimum utilization within the programme of the foundation archive. It is hoped that the archive will grow both by the inclusion of cases of the same class (primary middle and secondary schools) and of other classes such as classroom, local authorities, teachers' centres, curriculum projects. It is also hoped that the initial set of monographs will stimulate interest in the use of the archive by researchers working on topics or problems which could be set in context by archival reference, and also persuade them to lodge case records where this is appropriate.

It is worth emphasizing the principles of procedure which are at stake in the strategy proposed here. Case-study work could not be formally replicable unless the case were judged representative of, not merely exemplary of, a class; and this has been rejected. In experimental science replicability is the basis of a test for truth or (to adopt an alternative formulation) of the establishment of intersubjectively verifiable cognitions. The loss of replicability in case study suggests a need for an alternative procedure. In this programme the intention is to establish a convention for grounding intersubjective verifiability on an appeal to the scholarly community based upon the critical judgement of the handling of sources of evidence in archives accessible to all students on the same terms. This is the basis of intersubjective verifiability in history: the critical discussion of accessible data by numerous observers. The archive would be a source, cited in footnotes giving precise locations, a procedure invented by historians in the nineteenth century.
The logic of this proposal may be set out summarily as follows:

a. Institutional action in education - at classroom, school and system levels - involves complex situations in which the interaction of variables within the situations and the ecological interaction of situation with context are sufficiently problematic that no viable sample can be deemed representative of its class if generalizations about the effects of educational action are at issue.

b. Because sampling breaks down, the classic approach to cumulation through educing predictive generalizations accessible to replicable testing breaks down; and this is the basis of field experiment in the so-called agricultural paradigm, deriving from Fisher and others, which has been the staple of field experiments in education in which parametric statistics are used to discriminate between alternative generalizations concerning a population sampled by a small group of cases. (Cronbach 1975: Walker and Schaffarzik 1974)

c. An alternative approach involves the study of cases as exemplary, not as representative, of their classes. In this paradigm the relationship of case to class remains problematic and hence predictive generalizations about classes of cases cannot be the basis for verification. But although the study of individual cases must accept and represent their particularity, there must always be attempts to work towards generalization (theory) by comparisons across cases, since there is no possibility of dialogue about action among those located in different cases without generalization.

d. Any generalization from case studies must appeal to the same criterion for its validity, and since this cannot be replicability it would be well to adopt the historian's appeal to the concurrence of trained judgement of publicly accessible sources. This means that the data gathered by each field worker must become accessible to all.
Microfilm techniques make it realistic to aspire to a Contemporary Educational Records Archive which would fulfil this condition.

e. Given such an archive, its critical use would progressively refine the criteria of acceptability for case records: that is, edited primary sources based on field-work data. It is an important feature of the research programme that it should be inter-institutional, thus bringing a variety of traditions to bear on the problems of criteria and standards.

3. The case for Case Study.

A study of a course of events, such as the Crimean War, or of an institution such as the Greater London Council, is not necessarily to be regarded as a case study. A bounded study of this kind becomes a case study when it is perceived in terms of its class; that is, a case is exemplary of a class. There is an immediate implication that there are other courses of events or institutions with which this one bears comparison, thus the Crimean War is seen as exemplary of wars, the Greater London Council as exemplary of elected local government councils. However, unlike a sample, a case is not regarded as representative of its class. What is true of the case is not true of the class. In so far as we can make generalizations from cases they can be based only upon cumulative comparisons.

I am going to argue the case for case study as a response to the breakdown of sampling as an approach to the study of educational actions, events and institutions. Case study is advocated or defended on other grounds, but without prejudice to these, I propose to concentrate on the sampling problem as crucial.

Most educational research and most research whose findings can be applied to educational practice has been in the tradition of experimental psychology and this tradition has dealt in samples,
i.e. instances or numbers of instances taken for the purposes relevant to the experiment to be representative of the class from which they are drawn. Often the sample has been a single instance or a very limited number of instances, the appeal being to replicability rather than to sampling theory. This is true of many classic laboratory experiments in learning and in motivation, for example. The principle is that, if we are dealing with a general law, its effects, or the observations from which those effects are inferred, will be present in every instance. The only risk is that given instance impurities or masking factors will obscure from observation aspects of the situation which are nevertheless present.

Small samples or single instances are effective units for experiment where it is the case that relevant variables can be effectively controlled. However, many educational experiments need to be conducted in field settings because the complex of variables involved in educational realities cannot be reproduced and monitored or controlled in a laboratory. In order to conduct experiments of this sort educational researchers adopted procedures derived from Fisher's pioneer work in agricultural research (Fisher 1925, 1935) and combined random sampling and inferential statistics to create a research paradigm. In this paradigm, a random sample or a sample deemed to be a random sample is drawn and it is randomly divided into control and experimental samples, statistical techniques being used to calculate the probability of chance errors accounting for any observed differences between experimental and control and the probability of the effects observed in the sample generalizing to a target population from which the sample is drawn. It is important to recognize that this paradigm depends upon an assumption of the systematic distribution of an attribute (e.g. height) in a distinguishable category of objects (e.g. adult males). Classically, it has been an assumption of normal distribution.
Some of the compromises which are associated with the attempt to apply this paradigm in field research in education are discussed by Campbell and Stanley (1963) and by Snow (1974). Taking these into account, the paradigm still seems to me a useful, indeed a powerful, tool where what is at issue can be reduced to the distribution of attributes in populations. Ideally, of course, what should be at issue is a matter of theory for which the result is crucial, but much research in this mould is fact-finding rather than theory-testing.

Increasingly, educational research has become interested in experiment which seeks to relate educational policy or educational action to its effects. This is particularly true in the area of research in curriculum and teaching; and here the shortcomings of the psycho-statistical paradigm have been widely noted, though not always carefully analysed.

A paper of Lee J. Cronbach (1975) offers a classic diagnosis. He looked back on almost twenty years of effort to disentangle the problem of unsatisfactory generalization from samples to target populations by the analysis of aptitude-treatment interactions (sometimes and better called trait-treatment interactions) and drew pessimistic conclusions.

Important as ATIs are proving to be, the type of investigation I advocated in 1957 no longer seems sufficient. Interactions are not confined to the first order; the dimensions of the situation and of the person enter into complex interactions. This complexity forces us to ask once again: Should social science aspire to reduce behaviour to laws? (116)

Some social scientists nowadays are eager to establish rigorous generalizations about social policy by conducting experiments in the field. We have already seen mammoth federal experiments on performance contracting in education; on alternative rules for making "negative income tax" payments, and on alternative practices in compensatory education. As these experiments have moved toward completion, their advocates have become increasingly pensive. Alice Rivlin, a leader among those advocates, has just reiterated her belief that social experiments are worth their cost.
But she also (Rivlin, 1973) entertains the thought that their proper use is to compare alternative rules, rules so formal that the winning competitor can be embodied in an act of Congress and enforced uniformly over the nation. The welfare alternatives are of this sort. Rivlin doubts that gross experimental comparison can produce useful rules for schooling, where a treatment is multifaceted, cannot be standardized, and interacts with pupil background. Under these circumstances, the between-school variation in practices stamps out any generalized effect of the specified treatment variable. (122)

Generalizations decay. At one time a conclusion describes the existing situation well, at a later time it accounts for rather little variance, and ultimately it is valid only as history. The half life of an empirical proposition may be great or small. The more open a system, the shorter the half-life of the relations within it is likely to be.

This puts construct validation (Cronbach, 1971; Cronbach & Meehl, 1955) in a new light. Because Meehl and I were importing into psychology a rationale developed out of physical science, we spoke as if a fixed reality is to be accounted for. Events are accounted for — and predicted — by a network of propositions connecting abstract constructs. The network is patiently revised until it gives a good account of the original data, and of new data as they come in. Propositions describing atoms and electrons have a long half-life, and the physical theorist can regard the processes in his world as steady. Rarely is a social or behavioural phenomenon isolated enough to have this steady-process property. Hence the explanations we live by will perhaps always remain partial, and distant from real events (Scriven 1956, 1959b), and rather short lived. (122-123)

The time has come to exorcise the null hypothesis. We cannot afford to pour costly data down the drain whenever effects present in the sample "fail to reach significance." Originally, the psychologist saw his role as the scientific observation of human behaviour. When hypothesis testing became paramount, observation was neglected, and even actively discouraged by editorial policies of journals. (124)

When we give proper weight to local conditions, any generalization is a working hypothesis, not a conclusion. (125)

And Cronbach closes his paper with this peroration:

Social scientists are rightly proud of the discipline we draw from the natural-science side of our ancestry.
Scientific discipline is what we uniquely add to the
time-honoured ways of studying man. Too narrow an
identification with science, however, has fixed our
eyes upon an inappropriate goal. The goal of our
work, I have argued here, is not to amass generalizations
atop which a theoretical tower can someday be erected
(Scriven 1959b p. 471). The special task of the social
scientist in each generation is to pin down the
contemporary facts. Beyond that, he shares with the
humanistic scholar and the artist in the effort to gain
insight into contemporary relationships, and to realign
the culture's view of man with present realities. To
know man as he is is no mean aspiration. (126)

I have quoted Cronbach at some length because I think his
address the most coherent expression of a mood which is
becoming more widespread among American social scientists:
a doubt of their quest. And because I believe that doubt is
up to a point well-founded.

In his paper he argues severe limitations to predictive
generalization, clearly implying a need to fill these out with
data less "distant from real events"; and such data would
clearly be based upon observation. Indirectly, he argues a
case for case study, and he ends up with a yearning towards the
humanistic, which is not fully thought through, but which might
lead him to welcome the case-study movement.

I think, however, that the case can be made more strongly than
he makes it, and less destructively towards generalization.

Although Cronbach at the beginning of his paper (first quotation
above) casts doubt upon the reduction of human behaviour to laws,
later he declares:
Our troubles do not arise because human events are in principle unlawful; man and his creations are part of the natural world. The trouble, as I see it, is that we cannot store up generalizations and constructs for ultimate assembly into a network. It is as if we needed a gross of dry cells to power an engine and could only make one a month. The energy would leak out of the first cells before we had half the battery completed. (123)

I reject this view of the situation.

The notion that the natural world is lawful is disputable even in the physical sciences, though I shall not enter into that dispute here. Let us merely say that there can be no doubt that it is profitable to understanding to treat the physical world as lawful, whether this be a mere strategy or an epistemological stance. But human affairs are not in Cronbach's sense part of the "natural" world. This is because the creation of languages - syntactical, algebraic and artistic - produces a social and a personal world which is not natural but cultural, that is, man-made. And this man-made world is not in principle predictable because meanings enter into the complex of factors involved in action and meanings are not predictable. In short, experience cannot be irreducibly translated into behaviour, nor can action be reduced to happenings.

This is too complex an issue to argue here, but it will be helpful to set out the implications of such a view as I need them for my argument:

1. Experience and action cannot be rejected from the study of human affairs as irrelevant;
2. Experience and action cannot be regarded as attributes systematically distributed in populations;
3. Hence, experience and action are not accessible to representative sampling;
4. Hence, experimental approaches to experience and action cannot meet the criteria of internal and external validity.

A crucial barrier to the study of experience and action is the failure of sampling, and it is this failure of sampling - which
is intrinsic and not a mere shortcoming in technique — that justifies the proposition that a mature human science must face the problem of the study of cases.

At the same time, there is a place for the study of characteristics, behaviour and happenings (events which are uneventful), that is, the aspects of human life and living which prove to be susceptible to sampling.

There is a complementary relationship between law-seeking sample study and descriptive case study and that relationship is founded in reality. Neither study feeds merely on the inadequacy of the other. Human life is partly lawful, partly unlawful. In so far as it is unlawful it can be studied only through cases exemplifying classes which are defined in terms of linguistic meanings rather than in terms of operational specifications.

The case for case study in educational research can be made on the grounds of the nature of what is to be studied and is not contingent upon temporary setbacks in the employment of other research styles.

4. The Case Against Case Study.

The case against case study which I wish to argue is based upon a view of social science — or scholarship — and I must set this down first.

I take it that social science or social scholarship is a form of institutionalized cooperative endeavour to extend and deepen understanding. It deals in public knowledge, public critique, public techniques, and through them creates a tradition, accessible to people of reasonable ability and industry, within which contributions to knowledge and understanding can be made. By means of a collaborative public structure it enables fairly humdrum people to make contributions to human understanding of a kind which pre-scholarly societies received only from the
outstanding visionary or sage. Social science is democratic in the sense that the community of social scientists shares knowledge in such a way that the work of less able is strengthened by the work of the more able, and that critical principles are accessible in the light of which the most authoritative scholars can be called to account.

It will be apparent that this is a normative and ideal, rather than an empirical, account of the community of social science: but that does not invalidate it as a basis for a critique of case study.

In contrast to the social sciences I believe that the arts, although they produce communities, are essentially individualistic and are also open only to exceptional talent and industry. They commonly address not a community, but an audience. In one sense, they are more accessible: social science tends to be written for social scientists, novels are written for people. In another sense, they are less accessible: reading social science and acting on it will make you a social scientist; but reading novels and acting on them will not make you a novelist.

I do not think art is less true than social science: on the contrary, because it does not require to be authenticated—that is, because it is fictional—it can pursue truth at a greater depth than the demand for authentication allows. But its basis in community is not like that of social science. Social science is and ought to be a community based upon a tradition. That is what makes it accessible.

In research in education which involves the dimensional study of samples there is an abundant literature on design and research methods which make explicit the conventions of the tradition. Although there are, of course, disputes within the tradition, it is possible to advise a student what he should read to acquaint himself with the conventions expected to guide his research work and the presentation of his findings. These conventions are not binding, but departure
from them needs to be justified. It is clear from discussion with doctoral examiners and from exchanges between research workers at conferences there is a lack of conventions supportive of the student who enters the field of descriptive study. The tradition is not fully public, and it is important that scholarly research should be openly and publicly accessible within the company of scholars.

This seems to me the main case against case study. The openness of scholarly research rests upon the establishment in any field of inquiry of such a communal tradition as will provide principles of verification, conventions of communication and an organization of data capable of supporting cumulation.

From a social science perspective the case against case study is that it is liable to escape into art because (1) it readily becomes unverifiable; (2) it is relatively resistant to cumulation; and (3) its practical application appears to be problematic.

There will always be a type of case study which is art: Boswell's *Life of Johnson*, Flaubert's *Madame Bovary*. But this proposal is concerned to meet the case against case study as an approach within social science.

5. The Problem of Verifiability.

The principle of verifiability demands the possibility of criticizing the results or the conclusions of any researcher by undertaking independent work which tests that which is in question. It is this principle which supports the democracy of the scholarly community against the authority of individuals.
In the experimental sciences verifiability rests upon the principle of the replicability of experiments. However, this principle depends upon the possibility of ignoring the location of experimental events in time. Cronbach (1975) argues that even the experiments of physics cannot ignore time because they occur within the history of physics. However, physical theory has a long half-life.

In social science, experiments involving samples of populations in interaction in the presence of action cannot in practice or principle be replicated, I have argued. This breakdown of verification is signalled when the data are criticized only in the light of the process by which they are gathered and the results are criticized only in terms of an evaluation of the operations performed on the data. An experimental process designed with replicability as an underlying assumption is subjected to a critique inappropriate to it. Data processing is not subject to historical critique, because the data involved are not sufficiently open to judgemental scrutiny.

In descriptive case-study work, as it is generally practised in education at present, neither replicability nor critique of data and its processing appear to be possible. Replicability is ruled out by the location of the case as action in historical (non-replicable) time. Critique is eluded because the case study is based upon fieldwork, but the data derived from such work, which provide the only support for the account of the case, remains inaccessible to other scholars.

This proposal attempts to make possible a critique based upon an analogy with history as a basis for "intersubjective verifiability". In history what is "replicated" is not a repeatable experiment but scrutiny of the same data. History "rests on the critical study of documents by numerous observers, which can therefore be compared and cross-checked" (Levi-Strauss 1963,17)

Levi-Strauss, viewing the problem as a social anthropologist
dealing with the cultures of societies, pursues an analogy with history by hoping for an increase in the number of ethnographers, so that each culture will have several students. This would lead to multiple independent case studies of a single school if followed into school case studies. To this there are objections too obvious to rehearse.

Instead, this proposal aims to tackle the problem of producing, and making available through microfiche reproduction, a case record representing the field-work data in a form accessible to critical appraisal by the methods of the historian and such extension of these methods as shall prove to be necessary in the differing context.

Inevitably, the case record will involve some reduction of data if it is to be manageable at all. The problem is how to produce self-consciously a record usable as a primary source. One approach to this problem is to produce rules for editing, but this will only take us a certain way. Much more important is the criticism of records by those who use them as sources. Discussion of the quality of a growing corpus of case records is the essential element in improving approaches to the problems of record compilation.

There are analogies between the problem faced here and the problem of the reporting of archaeological investigations. Often the site cannot be restored or left exposed. The only source is the record of the excavation.

It is, of course, of great importance that an attempt to produce case records that meet these demanding criteria should be inter-institutional since this insures against idiosyncratic developments and creates a community with a progressively sharpening grasp of the standards involved.

I am claiming that on the basis of a case record procedure advances can be made towards verifiability in descriptive case study. Presentation of descriptive data, as case studies or in other forms, should cite in footnotes case records as sources in support of statements and generalizations. Only by this means can
judgements be scrutinized in the light of the data on which they are based.

6. The Problem of Cumulation.

There are two levels of cumulation at stake: cumulation of accessible and usable data, and cumulation of "theory", interpreted here broadly as ideas and meanings ascribed to data.

a). Cumulation of data

Social scientists are used to thinking about the problem of cumulation of data in terms of comparability. This leads to an aspiration to reduce (that is, to abstract descriptions from) data as it is gathered in a manner sufficiently consistent to enable each datum to be accepted as comparable within its set. This is, of course, a necessary underpinning for the quantification of data.

I believe that some case-study data should be quantitative - for example, attendance rates, library holdings and borrowings, examination results and the like-in case studies of schools and that it is desirable that these data should be comparable in this sense. However, most data in case-study work will not be comparable in this way and it is this problem I want to consider here.

I turn once again to guidance from history: the central problem of history is the cumulation of data which lack comparability of the type provided by virtue of consistencies built in at source when the data were first gathered and recorded.

The historian uses governmental archives, reports of debates, memoirs and diaries, eyewitness accounts, newspapers and journals, legal documents, pictures, films, music, material artefacts and more as data on which to build a basis for his account. These materials are evidence. Evidence must be subjected to critical examination to estimate its genuineness and its significance, and to consider the basis of its comparability.
Historical method is in part a response to the recognition that the comparability of the data to be handled is inescapably problematic since every observation reflects in a complex and variable way (not capturable by a personal equation) the point of view of the observer. The bias, tone and complexion of personal records must be considered critically in the light of this recognition and even apparently "public" records (including statistics) must be examined in the context of their assumptions.

If we take the position (as I do) that no approach to case-study can aim at standardization for comparability at source in the classic style of data gathering, then an archive of case records is needed to support critical comparative appraisal of evidence about cases. In this respect social science founded upon case study may be regarded as contemporary history and the problem of cumulation of data is the problem of establishing well-ordered and indexed sources.

b) Cumulation of Theory.

History is sometimes considered a non-theoretical social science, but this is a superficial view based upon its narrative origins. Narrative appeals to implicit theory assumed to be shared by its audience. Another type of history, the analytic survey, makes theory much more explicit. Such history compares and analyses, for example, Iron Age burials, or joint-stock companies, or life in the monasteries in the fifteenth century. (The paper attached to this proposal explores this ground more thoroughly).

The theory involved in such surveys rests upon retrospective generalisation. Retrospective generalization orders and ascribes meaning to experience but is predictive only in the sense that it provides norms for the range within a class. Thus, there is an implied range of variation within Iron Age burials and the discovery of a site lying outside this range evokes surprise and is reported carefully and publicly. Of course, it makes nonsense to ask what future Iron Age burials will be like though it does not make nonsense to predict what newly discovered Iron Age burials will be like.
In education, a theme like "Elementary schools between the two World Wars", seems to have the classic characteristics of such an historical survey.

Now, all historical periods are to an extent arbitrary - some more than others - and they contain change within them. There are early, middle and late examples of the category surveyed within the period. Contemporary case study should in my view proceed by defining a category - in this programme, three categories: primary schools, middle schools, and secondary schools - and intersecting it with a definition of an historical period which includes a future.

By taking as cases primary, middle and secondary schools the programme begins to lay the foundations of an archival source for a history of each of these "in the last quarter of the twentieth century." In describing case-study as the basis of contemporary history, I mean a period of history in which the present moment is a point which is not its conclusion. There is a future within the period though it will be captured for study only when it becomes a past.

This view of the situation does not imply that we cannot write history until the period ends. It indicates only that new work will be regarded as falling within the study of the period. New studies are undertaken as contributions to the cumulating archive and regarded as comparable with those studies already lodged.

Such a contemporary history shares with recent history and with the social sciences an embarrassingly large store of potential data. Sampling is necessary. But it is not sampling of the kind we are familiar with in the psycho-statistical tradition in the form of representative sampling. In representative sampling we draw a sample to represent the target population in order to establish that our generalizations - which may be about uniform effects or distributions of effects - will hold across the target population. In case-study we have renounced that aspiration and regarded cases as exemplars not
Accordingly, when we begin to gather an archive in the face of an opportunity to add cases to it, our sampling procedure will be directed to the exploration of the ranges of interconnections of variables in the class. This calls for a sampling procedure akin to that advocated by Glaser and Strauss in their Discovery of Grounded Theory. We select apparently like cases to expose the degree of variation (and by reciprocal implication, similarity) contained within our definition of likeness and we select apparently unlike cases to expose the degree of similarity (and by reciprocal implication, variation) holding across our distinctions. This is good practice in the historical survey or in selecting which archaeological sites to dig.

If we are to embark upon a programme of this sort, we have got to accumulate some records resulting from case study and look at the problems of cumulation, which are, of course, related to critical verification of each case record. To take a simple example we have got to consider the problem of the critical comparison of interview data from two interviewers, one of whom appears to tend to evoke self-justification, the other to evoke self-criticism. It is, of course, this kind of problem, among others which the programme addresses through conference among the participants.

In all these circumstances it seems best (and motivationally most realistic, to begin the attempt to cumulate ideas and meanings towards retrospective theory by giving each participant case-study worker rights across the archive in a particular theme such as, for example, mixed-ability teaching or headship or the relation of school to work or the teaching of reading.

These topics and their allocation to individuals would be known to all participant workers. The hope is that the range of interests will provide a reasonable tentative framework for the studies and that the cases will begin to make possible the
comparisons which might be yielded by sampling on the principles set out above. A beginning cannot be too sophisticated.

Since the procedure has been described at rather an abstract level, let me give a concrete example. I have myself undertaken only one field case-study of a comprehensive school. The head was quite explicit that he had derived his leadership and management style from an admired commander under whom he had served during the war. What range of models or theories, I wonder, do heads draw on for their management styles? I want sources which will allow me to begin to answer that question. It is this rather simple level of cumulation which seems the wisest aspiration for a programme at this stage.

7. The Problem of Application.

Case-study work of the kind envisaged in this proposal may, of course, contribute to theoretical sociology, psychology or anthropology. However, I believe that educational research has an overriding responsibility to inform the practice of education.

The application of research to practice has commonly been seen as a problem of generalizing from a known experimental sample to a target population, not known directly as the sample is. This is usually called the problem of "external validity." In principle, this procedure diminishes the need for those involved in practice to exercise judgement by establishing laws which can be taken for granted. The findings of research are taken to override personal judgement in the way that the laws of physics confound our common experience of observation. And the need to judge the case is narrowed by the possession of generalizations true across all cases "other things being equal".

Case-study cannot adopt this procedure in its normal form. The problems of the application of case-study to educational practice require, therefore, some examination at three different levels: (a) in relation to educational policy; (b) in relation to action in the system at large; and (c) in relation to those schools which are the subjects of case-study.
(a) Application to educational policy.

I take educational policy to be concerned with legislation or official advice intended to shape either educational action or the conditions of educational action. The introduction of comprehensive schooling might exemptify legislation concerning the conditions of educational action. The "Handbook of Suggestions for Teachers" was an advice intended to shape educational action. I am working with rough and ready categories, but this is not the place to refine them.

In relation to both types of policy the survey of studied cases is relevant, and indeed this is the classic basis for Her Majesty's Inspectorate's briefing of ministers. The case-study proposed here has two main differences from that of the inspectorate: it seeks to make possible a public, rather than a confidential, tradition, and it is likely to gather material different from that accessible to the inspectorate by virtue of their authority situation (though it will, by the same token, lack the authority position to gather some data accessible to inspectors).

Case-study can throw light on the range of practice and problems, but not (without a conventionally sampled survey) on their frequency of incidence. It is diagnostic of the pattern of educational conditions and thus throws light on both the need for and the results of policy. In short, it constitutes a taking stock of experience.

It would be useful both at central and local policy levels, I am arguing, to have available case-based studies of headship or mixed ability teaching or disciplinary practices. Such studies clarify the range of problems confronting practitioners and the range of professional responses to them and hence reflect upon, and potentially guide, legislation, advice and resource allocation.

Studies of this kind will emerge from the present proposal, but it would also be interesting to test the potential of the archive to deal with topics and issues not anticipated by the field workers. This could be achieved by inviting the DES and local authorities to
pose questions they see as relevant to their policies in order to discover what light survey of the archive could throw on them.

(b) Application to action in the system at large.

I intend to raise here the relevance of case-based study of, for example, heads and teachers and advisers and schools to heads, teachers, advisers, schools, which are not themselves the subject of study. It is this level of application which is most problematic. True, practitioners can apply case-based work of the kind addressed to policy-makers, but the case-study approach appears to offer prospects of a much richer application.

The logic is one of application of intelligence gleaned from a variety of cases to one's own case. Such an application differs from generalization in that it depends upon the practitioner's knowledge of his own case. Starting from a diagnosis of his own situation, he seeks relevant insights from other cases.

At a formal level the process is not unlike that of learning to improve one's chess by studying other people's games, but the rules of the game tend to make cases more similar than cases are in life. Nevertheless, the player must know his own temperament, strategies and weaknesses.

The chess analogy is helpful because it throws into relief the fact that the communication of games of chess is achieved through a formal notation, not an extended verbal description. Because of the standardization of rules, board and pieces, it is possible for someone to recreate a game which has been reduced in this way (though the psychological elements in the play have to be inferred from the reconstruction of the moves).

This symbolic reduction is not possible in the real-life world of educational practice, and there is a problem in reproducing for practitioners at large full case records or large numbers of extended accounts of schools.

In the proposed programme thematic treatment across cases would
be used as the basis for reporting the work for the use of practitioners at large. Through this strategy it is hoped to preserve much of the texture of descriptive material but to reduce the bulk by comment and summary. It seems that, if the problem of reduced presentation cannot be solved, then case-study approaches will be seen to be applicable only at policy level by those who are actually studied.

Case-study work will be utilizable by practitioners at large to the extent that it exposes its content to judgement since the application depends upon the practitioner's judgement of the relationship of other cases to his own.

This formulation is too abstract to catch the essence of the problem, and I propose, therefore, to provide an example.

The following is a verbatim transcript of an interview in a case-study. (My words in Roman; the head's words in Italic).

'I wonder if you would be prepared to talk to me about your room?'

'This room - yes. In the first place the doors must never be shut: nobody must feel that this is not their room. It is an essential point in the school to which people should gravitate and in our organization I take on the personnel relationships and I do know schools within this city where they have to make appointments to see the head. I think this is a whole lot of nonsense and I think that this school, this room is an integral part of the school, it's not my room, I didn't buy it, it is a room to be used for a specific purpose, and those purposes are, one, for private talks to the head, for it is the head's office as such, and it is a room which is available to any of the groups, any group in the staff at any suitable time, suitable to them, suitable to me, which they can use. I see it as a room which should be comfortably furnished which is not the headmaster's room, and this is where, at that end I have got it as a sitting area, it just so happens that the desk is here, and more often than not I talk to people sitting in that part of the room. It's a room where parents and visitors can come and they don't talk as we are
talking at the moment across a desk. This I think is wrong because this was borne in upon me by the number of parents who say "yes doctor" to you, they equate talking across a desk with the only experience they've had, that is a doctor, and innumerable times it has happened that they say 'yes doctor' and this is wrong. It's a wrong relationship and so really in my previous school the desk was cut in two and pushed in a corner, but that was nice because I never sat behind a desk, I had to turn round. This isn't the kind of desk that would do it anyway. But I think it's essential that this room should be available to the kids as well and this is a matter of personal relationships.'

'When you say available to the kids?'

'They can come and talk to me here.'

'Yes. And they should feel comfortable too.'

'They should feel comfortable in coming in and if kids come in here I always ask them to sit down, I never have them standing in front of the desk and usually, more often than not I pop out and sit from behind the desk and then we sit round in an informal way and this is the reason we have got these easy chairs in here to create that atmosphere.'

'So I would be right in thinking you have given a lot of thought to the nature of your room, haven't you?'

'Yes, yes, I have.'

'And that you have consciously not stamped your personality on it in a way that would be assertive.'

'No.'

'Is that right?'

'It just happens to be the place from which I work.'

'Yes, I understand that.'

'And I feel very strongly that it must be of that nature, and I took my lead, during the war I was in ack-ack command and General Tim Pyle had a room with a little glass spy-hole in the door and you looked through the spy-hole and if there was nobody there, you went in, and I learnt it from that.'
I believe that material of this kind can appropriately be extracted from the case record of a school (i.e. that the school need not be treated holistically as a case) and that protocol data of practitioners' views of their problems can be assembled into manageable presentations. It also seems reasonable to point up issues in the data, such as the relationship of the lay-out of furniture to authority, and to relate the data to other data in the case, not presented in extenso: for example, in this case, members of management committees in the school felt that decisions they had made were vulnerable to revision by the head since his policy of open access made him vulnerable to lobbying.

The passage quoted above and the two exemplary comments are intended to give a thumbnail sketch of a possible approach to presenting case-derived data in a style accessible to practitioners' judgement.

(c) Application to schools which are the subjects of case study.

The application of the case studies in the proposed programme would be negotiated in each case between the school and the case-study worker. The relationship between case worker and school is one which involves responsibilities on each side and should not be overly constrained by programme planning. Some suggestions may be helpful, however.

It is probably important to get the issue of application clear early in negotiations. A school might use the work as a springboard for self-evaluation, or problem diagnosis or the generation of a development plan. Some case workers may take the view that it is a condition of their working in a school that it has a clear and expressed intention as to how the work should be utilized. (See p.10 above)

The initial assumption here is that case records themselves would not be easy reading since they would be collections of raw data. The presentation to the school would presumably be either a case study based on the case record or a consultant's report written by the case worker. In the first alternative the school would be
asking for a picture of itself on the basis of which it could make judgements and formulate policy. In the second alternative the school would be asking for diagnostic judgements on the part of the case worker.

The purposes of the school should have equal weight with the needs of the research programme in shaping the agenda of study for the case worker. He will have to pay attention to the teaching of remedial groups or the linkage of science and technical subjects if the school asks for this, even though these topics are not among those chosen by his fellow case workers as survey topics.

Because of the intimate and reciprocal relationships of schools to their studies, the utilization of the study by a school requires mutual responsiveness between school staff and case worker. The communication - unlike that at the two other levels of application - is a face-to-face one. It is not easy to anticipate all the problems of utilization (though some are familiar to us), and provision is made in the follow-up part of the programme for study and reporting of the experience of utilization in order to provide guidance for future case workers and their schools.

8. The Structure and Organization of the Programme

The programme is intended to involve ten institutions selected so that (a) they are in geographically contiguous pairs and can thus maintain continuous consultation; (b) the interlocutors, who are senior staff and responsible for the contribution of their institutions to the programme, have a range of interest and experience, and (c) a variation in institutional research experience is represented (schools of education with strong research traditions and others trying to build such traditions).

Seven interlocutors have been approached; but the move of Professor Wragg to Exeter, which is not expected to be a participant institution, reduces this effectively to six, to which can be added the principal investigator. Those approached have all responded positively, and four of the six very enthusiastically.
The pattern of institutional participation proposed is as follows (Interlocutors approached named in brackets):
University of Newcastle-upon-Tyne and University of Durham;
University of Leicester (Professor Brian Simon) and University of Nottingham: Cambridge Institute of Education (Miss Joyce Skinner) and University of East Anglia C.A.RE. (Lawrence Stenhouse);
University of London Institute of Education (Professor Denis Lawton) and University of Sussex Education Area (Professor Colin Lacey);
University of Oxford Department of Education (Dr. Harry Judge) and University of Reading School of Education (Professor Jack Wrigley).

Discussions with potential interlocutors seemed to make it clear that members of staff in the institutions could be realistically expected to contribute their research time to the project over two years. They would get from the programme both experience in case-study and rights in a monograph treating a topic across the cases. In some institutions it was felt that the programme would provide a good framework for staff of quality who had not found a satisfactory research identity. The case for including a member of staff of a linked or neighbouring college of education was also mentioned.

Subsequently to the consultations with interlocutors I became impressed with the importance of developing younger researchers in the proposed research methods, and I have consequently included ten post-doctoral fellows in the programme. I believe that this investment would be well worthwhile, but the structure of the programme itself is not dependent upon it except for the study of schools' utilization of the work, which could be differently organized.

Apart from the post-doctoral fellowships, the cost would be in secretarial salary, travelling expenses and equipment. Secretarial support is to fieldwork of this kind what computing time is to statistical work. Without it, transcription and ordering data drags on interminably. Travelling expenses are necessary both for consultant and conferences and because schools are likely to be at some distance from the participating institutions, and in some cases this may be preferred; for example, an institution may prefer to avoid case-studying its practice schools. Equipment is restricted to three portable tape recorders for the three field workers, a dictaphone taking standard tape-recorder cassettes for the secretary.
to transcribe tapes, and, for each institution, one microfiche reader and one IBM typewriter which both provide sans serif types, the preferred standard for microform, and allows different kinds of material to be coded by different types.

In each institution the interlocutor will be responsible for recruiting the case workers and will be accountable in the programme for the completion of the work in his or her institution, as well as for arranging, in collaboration with the paired interlocutor, inter-institutional consultation. It is important in this respect that the keeping of running transcripts and notes towards a public case record does much to insure against loss of data should a case worker move or drop out of the programme for any reason.

There is a problem in budgeting and financial administration in this kind of programme. Probably each institution ought to budget separately and take responsibility for its budget. Whether it should then work directly with the SSRC or through the programme coordinator is presumably a matter for the SSRC.

Decision alternatives are reflected in the presentation of the budget in this proposal.

Although I, as principal investigator, will take responsibility for the programme as a whole, my role within it will be as interlocutor in the CARE team and a co-ordinator will be required.

The co-ordinator will be at the top of the lecturer grade and will be full-time. The co-ordinator's responsibilities will include:

1) the organization of the three annual conferences of the programme;
2) the organization of the presentation of the programme at BERA on three occasions;
3) visiting all the participating institutions from time to time;
4) running a news sheet of issues and experience in the programme for those within it and for interested outsiders;
5) blurring the boundaries of the programme by
providing information about it to non-participants
(but protecting case workers from this demand);
6) supervising the secretary and the indexer working
in the programme;
7) handling business with the microfiche producers,
and the formation of the archive.

The co-ordinator will be based in CARE and responsible to the principal
investigator.

The indexer will develop an indexing system for the microform archive
and develop it with computer retrieval in mind.

The programme will start with a meeting in January 1979 of co-ordinator
and interlocutors, if the timing of the funding decision makes this
possible. This meeting will be to clarify the organisation of the
programme by reference to its purposes.

Thereafter the programme continuity will be based upon annual Easter
conferences, local inter-institutional dialogue, and the communication
network set up by the co-ordinator. Through annual reports to the
BERA conference (either in conference sessions if this is accepted by
BERA or by a stall and literature) the programme will attempt to keep
an open boundary with the research community, making its work accessible
and encouraging other institutions to participate, as their
circumstances allow, in the emerging tradition and the archive.

9. The Contemporary Educational Record Archive

The case records emerging from the programme would be typed in a
uniform format suitable for microfiche reproduction. The standard
preferred is a sans serif type. Three IBM types will be used to
code materials in three categories (1) verbatim transcript of tape
(audio or video), (2) quotation from field notes and (3) later
material contributed in writing up. This format would set an ideal
standard.

Criteria for editing primary sources as case records will be discussed
in conference and standards will be adopted throughout the programme.
to the extent that this seems possible and desirable.

Microfiche masters will be made from the typescript case records by Micromedia Limited of Banbury and will be held secure by them pending decisions about the organization of a contemporary educational records archive. The foundation of such an archive could probably best be discussed with the British Library, and liaison with their research and development section would be established during the programme.

Each participant institution would receive a copy on microfiche of each case record (30 in all), and the researchers would test the feasibility of these as sources for their thematic monographs.

All case records would be indexed in a single index on a system adapted for computerization, which would be necessary should the programme lead, as might be hoped, to a computerized national archive. The indexing lexicon will be determined by discussion of the suggestions of case workers and the proposal of an indexer who uses established indexing approaches. At the programme stage of the archive a hand index will be printed for use by the participating researchers.

Until decisions have been made about the feasibility and the regulation of a national archive (possibly by a committee set up by SSRC to tackle this brief), microfiche case records would be regarded as confidential to participants in the programme. A crucial problem is to bar any case study of a school - as opposed to treatment of a topic - which might be written from a case record by a researcher who had not undertaken field work in that school.

There is an obvious problem of protecting the individuals and schools who are the subjects of case records. The obvious step of anonymization is probably desirable but anonymized accounts can be identified by members of the school itself, by those who know the school reasonably intimately and by determined inquirers.

Existing case-study work in the United States - for example, R.E. Stake's recent case-based study of science teaching - has apparently overcome most of the difficulties of this situation. It is clear, however, that schools with considerable problems ascribable to the
incompetence of personnel cannot be case-studied. A case archive will never be a basis for an evaluation of the state of the educational system, but it can provide a review of problems and of professional responses to them.

The problem of the protection of individuals and schools is one of which we now have some experience, but there is not an easy consensus view available. Accordingly, it will be a major area of discussion in the programme and the schools should be party to that discussion.

Every participant school should have a copy of this proposal and should consider this problem in its context. Provision is made in the central budget for meetings of a researcher/teacher committee to keep the problem under review.

10. Projected Plan of the Programme.

January 1979: Meeting of co-ordinator and interlocutors
Easter 1979: Conference of programme participants (about 50) Issues in case study, theoretical and practical, Planning future work.
Summer term 1979: Appointment of post-doctoral fellows (if approved). Negotiation of case-study contracts with schools.
September 1979: (Possible) Presentation of programme at BERA: offer of progress reports to all interested educational researchers.
Autumn term and 1979: Field-work, data transcription. First look at material for case records. (Spring term: preliminary work on indexing)
September 1980: Second report to BERA

September 1981: Final report to BERA

Autumn term: Coordinator chasing laggards and finalizing negotiations regarding the possibility of the archive.
References


