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CHAPTER 10

THE ORGANISATION AND FACILITATION OF RESEARCH

By way of introduction to this chapter, attention may be drawn to some features of the research on which this book has been based. Much of this research did not conform to widely accepted beliefs abut how research should be initiated and funded. It was generally research which involved adventures into the unknown. Several of the enquiries were initiated on the basis of feelings and impressions rather than formal hypotheses. Much of the research had its origin in sensed problems rather than the "research literature". The logical connections which gave meaning to the data (or which provided a "theoretical framework" for collecting or interpreting it) were often made retrospectively. The true significance of the data could often only be discerned if one drew debatable conclusions about underlying structures and processes which go well beyond what could be "proved" by specific data sets[10.1].

Such research could not have been conducted according to conventional ideas concerning the initiation, funding, execution and evaluation of research[10.2]. The same is likely to be true of the research needed to overcome the problems identified in this book.

The Role of Colleges, Universities, and Research Institutes

In the course of this book we have seen that common sense and established good practice are *not* an adequate basis on which to build a program for remedying the conspicuous problems of the educational system. The causes of these problems are deep-seated and often rooted in causes far removed from their symptoms. They have major value-laden and political components which are often difficult to address in the face of widely held beliefs concerning the workings of society. But the most important points to be noted here are, first, that it has only been possible to uncover many of the causes of otherwise conspicuous problems through research, and, second, that - because of their deep-seated causes - the problems will only be solved through further research-based development activity.

Although colleges and universities have a major role to play in carrying out such research, collecting the information and developing the understandings and tools required to solve the problems facing the educational system are difficult and adventurous activities which are not easily reconciled with the kind of research which has come to characterize university-based social science research over the past 40 years. This has tended to be of an unadventurous, non-controversial, literature-driven (rather than problem-driven), "disciplinary", nature and has often been more concerned with securing the promotion of those concerned than with advancing understanding or solving problems[10.3]. To tackle the problems, we need a wide variety of full-time researchers and institutional arrangements which will enable them to contribute in different ways to team-based applied research with fundamental components which aim to find ways of tackling the conspicuous problems of the educational system in a limited period of time. To help discern the arrangements that are needed we will first review the range of research that is required in slightly more detail.

The Range of Research Needed

We have seen a need for research to:

- Document the personal and societal, short and long-term, consequences of alternative educational programs.
- Develop the tools required to monitor the quality of such diverse programs.
- Develop a better conceptual framework for thinking about competence, how its varieties and components are to be nurtured, and how its varieties and components are to be assessed.
- Develop the tools needed to help bureaucrats administer the necessary variety of educational programs.
- Develop the tools which teachers need to help them think about the motives, talents, and latent competencies of their pupils, design individualized educational programs to tap those motives and nurture those talents, and assess the outcomes[10.4].
- Develop a framework for appraising principals, so that they can get credit for creating school environments which release and utilize the talents of classroom teachers.
- Develop means of assessing the work of public service administrators so that they can get credit for initiating the collection of forward-looking information, their ability to use such information as is available to make good discretionary judgments about what is in the best long-term interests of society, and their ability to translate those decisions into effect and monitor the results in such a way as to improve the policies which have been implemented.
- Clarify the competencies required by head teachers and public servants and the ways in which they can be encouraged to develop and display the desired qualities.
- Clarify the links between educational policy on the one hand and policy in other areas (e.g. differential incomes and employment policy) on the other and the steps which need to be taken to influence these wider processes.

However, the research which is most urgently needed does not lie at the level of such specific detail. It has to do with the management of society itself. Such research might take the form of experimental action programs designed to test and refine the insights of the last two chapters, or it might investigate the assumptions embedded in the organization of different societies. The research would be designed, not to test previously formulated hypotheses, but to formulate questions which ought to be asked.

The purpose of this chapter is to discuss the institutional arrangements and expectations required to undertake the full range of research required to solve the problems of education.

The Nature and Organization of Research

Fundamental research of the kind discussed above can only be carried out in the context of action. One cannot, for example, develop valid measures of high-level competencies without changing classroom processes so that those competencies are developed and displayed. But to change classroom processes it is necessary to have some means of identifying the important outcomes as part of the certification and placement process. And one cannot change either of these except in the context of new relationships between teachers and schools on the one hand and examination boards, test agencies, local and national employers, and parents and politicians on the other. In other words, one cannot expect to make much progress without careful systems analysis and systemic intervention to prevent parts of the system nullifying the effects of specific interventions[10.5].

Good research is adventurous, inventive, creative, and pays little respect to "disciplinary" boundaries. Our work on educational objectives and competence, for example, led us to enquire into the nature of modern societies and the political and institutional arrangements required for their effective management.

Improved understanding often stems from public debate about concepts, ways of thinking about things, and the *implications* of data. Many of the insights shared in this book arose through the discussion of conference papers and published articles which now appear to contain naive, sometimes completely incorrect, interpretations of incomplete data. Scientists must be prepared and expected to admit the possibility that they may be wrong in order to clear the way for productive debate. It is vital to distinguish between the outcomes expected from the scientific *process* and those to be expected from the work of an *individual scientist*. The scientific process leads to "truth" and indisputability: yet the demand that the work of an individual be beyond dispute starves the process of much-needed data and insights.

We might draw a number of conclusions from these observations. First, we need more research teams to study "the same" topic from different perspectives. For example, the view of competence, its development and assessment presented in this book is not the only one imaginable, and may well prove to be misguided. Bureaucratic claims that there is no need for a given project because "*a* project in the same area has already been funded" should be strongly resisted. Second, if scientists become engaged in controversial debate it must not lead to their being discredited. Third, we need to ensure that at least some members of all research teams possess the competencies required to participate in productive public debate.

The interface between researchers, sponsors, and clients needs to be carefully re-assessed. Academia is not a good source of research proposals, since academics generally have little experience of the kind of research needed to inform policy, and little contact with the problems actually experienced by those the policies are intended to benefit.

Administrators, also, having insufficient contact with problems and their potential solution, are often in no position to appreciate the need for relevant work. They often express the need to be certain of outcomes, or a hesitance to commission work unless they know how it is to be carried out. The fundamental problem is that administrators do not wish to be held responsible for risky activities initiated by anyone other than their superiors. Worries concerning "risky" research also lie behind the misguided principle that researchers should provide sponsors with detailed research proposals. Prior knowledge of methods and probable outcomes fosters security on all sides, but it leads to trivial research which fails to advance understanding.

Decisions concerning the research to be undertaken need to be under more direct control of researchers and those aware of problems which need to be researched. Funding should be allocated on the basis of judgments concerning the competence of a given research team to initiate and carry through important innovatory projects. Clearly, such a scenario must allow some means for administrators and the public to alert researchers to issues requiring investigation. Once again, it seems, we reach the conclusion that such research should be organized through "parallel organization" activity.

One informative example comes from a Scottish Council for Research in Education project - *Pupils in Profile*. This was initiated because a number of school principals were concerned that current forms of assessment limited the educational programs they could offer. Despite strong opposition from administrators, the project went ahead and, despite its inability to deliver the tools needed by the principals, documented many difficulties and produced a system of "profile" reporting forms which have since been widely used. Unfortunately, research at the Scottish Council then came to be funded on a contractual basis, and the researchers found themselves unable to follow-through to capitalize upon the insights they had developed and or even continue the innovative research style which led to their initial success.

It is not difficult to find examples of the futility of contract research. Two hundred million dollars were spent on evaluations of Headstart and Follow-Through[10.6] designed to satisfy administrators' (changing) criteria, yet these evaluations almost completely failed to advance understanding of the issues. Ironically, the new understanding which *has* been achieved in the area comes almost entirely from the very poorly funded work of Levenstein[10.7], McClelland[10.8], and Sigel[10.9].

A mechanism is required to enable the public to influence research activity leading to educational and other policy. It is also too easy for researchers to redefine, in their own terms, problems brought to them by the public. It is therefore necessary to create a mechanism whereby groups of people with a particular - often marginal - perspective, can find researchers who share their viewpoint and ensure that the research continues to be informed by that perspective. Such a mechanism could capitalize on the Information-Technology based networks we have already suggested to supervise public policy.

The Role of Evaluators

Specific problems confront evaluators in their effort to monitor the effectiveness of policies and find ways of improving them. Evaluators frequently find themselves dealing with issues which have little to do with their parent discipline. For example, a good educational evaluator will draw attention to such things as the effects of demarcation disputes between social work and education, the difficulties created by the absence of appropriate transport, the effects of inadequate supplies of materials, and deficits in building design, even though these issues do not relate directly to the specifically *educational* aspects of the policy evaluated[10.10]. Day and Klein[10.11] have argued that one reason for the retention of ineffective policy is that any professional group can always argue that *their* activities *would* be effective if only they were supported by activities in other professional groups. They can also argue that their work ameliorates some problem other than that examined by the evaluators.

A good evaluation gets a rough fix on all important, short and long term, outcomes of a particular policy, and some measure of the constraints on its effectiveness: In other words, it involves *systems* analysis. The hallmark of a good evaluation is its *comprehensiveness*, not the accuracy of the particular observations that have been made. Failure to comment on an important consequence of the program, or to draw attention to an important constraint on its effectiveness, constitutes a more serious defect than failure to get an accurate measure of its effect on a single outcome[10.12].

A good evaluation, then, assesses how effective a program *would* be if it were implemented with and without a context of general understanding of what it is about, with and without proper training, with and without support material, and with and without interference from those fearful of its consequences. It seeks to predict the long term effects of a program, including negative social and educational effects (such as the development of trained incapacity). Such broadly based work, aimed at achieving an approximate estimation of many variables, cutting across disciplines, and anticipating the future, conflicts with the tenor of academic research. This hints at the serious problems which arise when attempts are made to locate genuine policy research in traditional "academic" institutions[10.13].

After the results of an evaluation have been disseminated and debated, problems still remain for their translation into effective action. It is unusual for a policy recommendation to be based upon a single research finding. The numerous considerations to be taken into account typically derive from many different domains. Such considerations again underline the importance of network-based management drawing on evaluations conducted by individuals or teams with roots in more than one academic camp.

Career Structures for Those Involved in Research

Researchers often find it necessary to mount political crusades in order to ensure that their work is applied[10.14]. This detracts from the time available to produce the publications deemed necessary for an academic career, and also leads people to doubt the scientific integrity and "impartiality" of the researcher. It is vital that appropriate career structures be developed for those involved in research. These should offer the security researchers need if they are to enter into public debate over controversial issues. They should also offer researchers the flexibility they need to re-direct their work when they find they have set out in the wrong direction and the time they need to mull over and make explicit the implications of their observations.

A few words also need to be said about the time scales that are involved in useful policy research. On the one hand, it is necessary to make significant progress in a limited period of time. This cannot be achieved in the individualistic (non-team based) atmosphere characteristic of academe, still less in the one-third time academics usually allocate to research. On the other hand, the time scales involved in doing useful research are much longer than is typically assumed by many sponsoring agencies. Researchers need to pursue problems which were not obvious and to do the conceptual, inventive, work that is needed to find ways of thinking about and tackling them. Many of the problems of the educational system are chronic and have been around for almost a century. They will still be around tomorrow and are not amenable to quick fixes. Enduring issues must be addressed. Crisis type problems tend to have solved themselves (or been shifted elsewhere) by the time "useful" data relating to them become available. Useful research cannot be undertaken in a situation where "priorities" change every couple of years, in which more time has to be devoted to proposal-writing than to carrying out research, in which the interval between the proposal, the report, and the next proposal is insignificant, in which there is little time for exploratory work or developing understanding and new measures prior to rushing into the field, or in which there is several years' delay between researchers identifying a problem and obtaining the funds needed to tackle it.

Scientists need to be encouraged to report (a) work carried out with imperfect tools and imperfect methodology, and (b) their impressions of the policy implications of their work. Without reports containing such data and insights, there will be no discussion of some of the most important policy issues, and without such discussion many important policy implications will be overlooked. Only researchers who have been directly involved in the relevant research are sufficiently familiar with the complexities of a problem to recognise these issues and their implications. Contrary to conventional wisdom, the most important activity to be undertaken by social researchers is, not to feed a few unarguable facts into discussion, but to promote public debate itself.

Summary

The reasons for the failure of the educational system to achieve its main goals are numerous, deep-seated, mutually interdependent and interacting, non-obvious, and intractable. Finding ways of overcoming these barriers is critically dependent on the implementation of a great deal of research, and research-based development activity. However, the kind of research required, and the way it is to be carried out, differ markedly from conventional ideas and practice.

The work required features elements commonly associated with both fundamental and applied research. It demands conceptual advances involving new methodology and experimentation, but can only be carried out in the context of theoretically-based, systemic, action. Its execution demands team work over an extended period, but needs to be carried out with a sense of urgency. The work required cannot be carried out in a climate of "publish or perish" or within the short time horizons which currently dominate both universities and applied social research institutes. Nor can it be carried out in the context of the traditional leisured life, "teaching", administration, and individualistic, literature-driven, research which once characterized academia.

New institutional arrangements are needed to conduct serious policy-relevant social research. The new understandings that are necessary can only be developed through public debate between protagonists advocating positions based on uncertain foundations. And it will be necessary to establish new relationships between researchers, policy makers, teachers, and the clients and customers of the educational system.

Notes

10.1. House (1991) has provided an excellent account of the need to get behind "data" to discern underlying and invisible structures and processes.
10.2. An insightful and amusing account of the ways in which the universities stifle innovative research will be found in Nisbett (1990).
10.3. Out of every 1000 AERA publications only 20 contain *new* data and in only 5 of those is the data substantive; the rest contain non-knowledge and are churned out to satisfy the 'publish or perish' machine which characterises the entire output of research at the present time.
10.4. Gardner (1990) has likewise emphasized the need to develop summative measures of school performance so that the public can be assured that they are getting value for money from educational programs without deflecting schools into low-level activities and also the need for tools to help teachers to administer high-level competency-oriented program (although he does not, in fact, recognise the importance of more than a fraction of the competencies emphasized in this book).
10.5. Salomon (1991) has provided excellent examples of the way in which intervention in one part of the system yields unexpected benefits and disbenefits elsewhere and also of the way in which interventions at one point are cancelled by reactions elsewhere in the system.
10.6. Raven (1981)
10.7. Levenstein (1975)
10.8. McClelland(1982)
10.9. Sigel (1985, 1986); Sigel and McGillicuddy (1984)
10.10. For an excellent illustration of the non educational barriers to educational innovation see Schwartz in Searle (1985).
10.11. Day and Klein (1987)
10.12. These issues have been discussed at greater length in Raven (1991). The only way in which it is possible to throw light on the short and long-term, personal and societal, "intangible and hard-to-measure" consequences of changing processes is to adopt a variant of what Hamilton (1977) and his colleagues have termed "illuminative" evaluation. In this, personal observation, data collected through informal interviews, data obtained through the use of unobtrusive measures, and formal quantitative data are combined to yield an understanding of the processes involved. This in turn is used to generate an understanding of what the short and long term outcomes of the process are likely to be. This process is heavily dependent on theory - but it is the only approach that has legitimacy in a situation in which there are no measures of the most important outcomes of the process (such as the effects on students' ability to undertake complex and demanding activities), in which the most important effects (such as economic and social development) will take many years to show up, and in which the most important barriers to the effective operation of the system are deep-seated, non-obvious, and systemic. The approach is in flat contradiction to that advocated by the *Joint Committee on Standards for the Evaluation of Educational Programs and Policies* (Stufflebeam et al. 1981). It cuts across the qualitative/quantitative divide on which so much argument in the field of educational evaluation has focussed ([Jacob 1987, 1988; Atkinson et al 1988), but it has found unexpected endorsement in the work of House (1991) and Salomon (1991).
10.13. The way in which the extraordinary requirements of effective evaluation can be approximated are hinted at in the previous footnote, and are discussed in more detail in Raven (1989, 1991). The problems which effective evaluation poses for evaluators and their deployment are discussed at greater length in several chapters in Searle (1985).
10.14. Cherns (1970); Freeman (1973); Roberts (1968, 1969); Tizard (1990)