# Chapter 24

# Arrangements for Policy Evaluation and Improvement

We have seen repeatedly in this book that much better arrangements are required if our society is to be run effectively. But behind that lies a fundamental need for much better information in domains ranging from basic biological processes to the operation of complex socio-political systems. What this means is that our existing arrangements for collecting, sifting, and disseminating information have failed us. This stems partly from defects in the arrangements themselves and partly from a lack of appreciation of the need for - and the possibility of collecting - the kind of information that is actually required. This chapter will both discuss the administrative arrangements and expectations that are required and outline some of the studies which are most urgently needed.

### *Why are New Arrangements Needed?*

To help us to understand why our current arrangements are working so poorly and to indicate some of the developments that are needed, it is useful to draw attention to some features of the way in which the research we have summarised on the developments required to achieve a more effective educational system was conducted.

Much of it did not conform to widely accepted beliefs about how research should be initiated and funded. It was generally research which involved adventures into the unknown. In fact, 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 a felt need to find ways of coping with practical 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 made debatable inferences about invisible underlying structures and processes. These inferences about unseen processes went well beyond what could be "proved" from any one of the data sets which had been collected[24.1].

All this accords with what is actually known of the process by which scientific understanding advances[24.2], but it conflicts sharply with the image of science projected by most social scientists and, in particular, by most of those who have set themselves up as authorities on research methods and who have had a major impact on what public servants regard as "good" research and research proposals.

Because so much of the research on which we have drawn most heavily could not have been conducted according to conventional ideas about the initiation, funding, conduct, and evaluation of research[24.3], and because the same is likely to be true of the fundamental research needed to progress as a society, it is important to say a little more about some key issues in this area.

*The Role of Feelings in Science*

All science derives from feelings, in two senses. In the first place, feelings provide the germ of all creative endeavour: They impel or entice people to enquire further into certain issues. More generally, although this is not the place to argue the point[24.4], they provide the basis of all cognition. In another sense they often provide an initial basis for measurement. The history of scientific measurement is a story of making the intangible - the somehow sensed but ungraspable and invisible - tangible and visible. Faraday used his subjective impression of the strength of an electric shock to assess electric current. Ampere found a way of making it more "visible" as a result of just happening to notice that the passage of a current along a wire deflected a compass needle.

The notion that science is not concerned with feelings is deeply dysfunctional. It leads to denigration of much concern with the state of the world and the environment. Instead of denigrating the feelings which provide the basis of these concerns, we need to make the reasons for them explicit and, like Ampere, to find less "subjective" ways of indexing whatever gives rise to them.

Finding new ways of handling feelings lies at the heart of what we have been concerned with in this book: One of the great merits of market processes is that they enable people to act on their feelings without having to articulate them. The public service, on the other hand, tends to denigrate "mere subjective feelings". Yet anyone who has made the slightest study of the workings of the "objective" marketplace must know that such a position is untenable. To capitalise on the potential of the market to enable people to act on their feelings, firms have to invest heavily in market researchers who devote enormous amounts of time to making people's feelings explicit and drawing out their commercial implications. Likewise, marketing personnel invest heavily in influencing people's *feelings* - not their cognitions - in order to induce them to buy their products.

The public sector invests hardly anything in studying people's feelings, making their basis explicit, or trying to invent ways of satisfying them. The introduction of mechanisms to do this must form a crucially important part in any alternative system for running society. And one of the most important research programmes which needs to be undertaken is to make explicit the reasons for the feelings which have lead to such widespread concern with the environment.

*Illuminating a Hidden Reality*

At several places in previous chapters we have emphasised the need to illuminate, and find ways of intervening in, *systems* processes. Yet the examples we have given may have caused considerable unease because the hidden processes to which we attached so much importance will have appeared to many to have been purely speculative. Yet the history of science is an account of how certain conjectures of this sort - all of which go well beyond the available data - have turned out to be extraordinarily useful. The leap from observations of a few

scratches on rocks, the "U" shapes of valleys, and the presence of rounded boulders composed of material foreign to the locality, to the conclusion that the area was once covered by ice and glaciers appeared to most people at the time wild, even absurd.

Despite the central importance to scientific advance of generating insights into unseen processes, it has taken enormous efforts on the part of a few mavericks[24.5] to even slightly weaken the stranglehold which the authoritarian view of science holds in the social sciences and to legitimise at the margins research which seeks to illuminate a hidden reality which is assumed to exist behind the observable. This has, of course, allowed people to cynically exploit the new intellectual freedom ... but the point is that any alternative, authoritarian, approach stifles the required innovation. Quite different controls - to which we will return later - are required to hold charlatans in check - but we may here note that an authoritarian system actually fails to prevent the cynical generation of mountains of non-information.

In the end, conceptual frameworks stand or fall depending on their usefulness. However apparently unsubstantiated the specific insights into systems processes shared in this book, nothing can be more certain than the conclusion that we *must* have better systems thinking and multiple carefully evaluated experiments, informed by that thinking, if we are to find a way forward. In short we must be much more prepared to let go the myth that scientists should be able to prove their theories and hypotheses from specific data sets. Put the other way round, we need more scientists who are able to use their data more flexibly to generate new understandings of global ecological processes and societal management systems, and who can thereby provide the foundations for pragmatic action.

*Science and Action*

Any scientist who presses for action on the basis of his or her results runs the risk of self-discreditation because it is felt that scientists should be "impartial". This view has a host of unfortunate implications. Among other things, it permits scientists to maintain an air of indifference to the mis-application of their results. The mask of impartiality permits them to publish results which are grossly misleading and socially damaging because they deal only with one, possibly not very important, outcome of the process they have been studying and ignore more important but damaging outcomes. This not only permits them to evade responsibility for the consequences of their actions: It also means that they are unlikely to press for broadly-based, comprehensive, evaluations and, in the end, evade responsibility for pressing for an alternative - ecological - view of science.

The desire to maintain an impartial image, expressed as a reluctance to dabble in action, does more than delay the implementation of important results. More importantly, it: (i) prevents scientists discovering a host of fundamental research topics which they would rapidly discover if they sought to apply their results or were more willing to engage in appropriate ways with applied problems - for such action would force them to notice factors which prevented things working out as they had predicted; (ii) often prevents them advancing their understanding because under the circumstances this can only be done in an action context (to repeat an example already given, it is impossible to develop better measures of desired educational outcomes except in the context of curriculum changes which enable pupils to develop and display those qualities and the introduction of these developments depends on the development of a better understanding of the nature and development of competence); and (iii) detracts from the establishment of an innovative social climate in which many people are familiar with the required cycle of seeking out and sifting information, initiating

experiments based on tentative understanding, setting those in the context of broadly-based monitoring, using the results to advance understanding, clarifying the goals, and re-starting the cycle.

The dangers which some people hope to avoid by distancing science from action are best met by increasing the number of scientists working in any "one" area, promoting public debate of tentative results, increasing the number of attempts to translate results into effect, and insisting on evaluating those experiments in a truly comprehensive way. It is this open, questioning, checking, process which will advance understanding and deter charlatans, not the insistence that anything published be clean and unchallengeable.

## *Science and Ethics*

Ethics is concerned with the long-term social consequences of actions. Since a comprehensive knowledge of these effects can only come from advances in scientific understanding, it follows that science and ethics are closely intertwined. Having established what is ethical, it is then necessary to translate it into effect. This means evolving arrangements which will make it easier to give teeth to information. As we have seen, clarification of, and experimentation with, those arrangements is itself a scientific task of fundamental importance. There can therefore be no meaningful divorce between science and ethics. Evaluations and actions which are unethical are also unscientific and vice versa.

## *Public Debate and the Advance of Scientific Understanding*

Although philosophers of science agree that public debate is vital to the advance of scientific understanding, there are strong pressures to present an authoritative/authoritarian image of science. These include a desire to prevent public discussion of important social issues by claiming that those issues are not amenable to scientific study. They also include the desire of some professors to preserve their reputation. By outlawing debate and concealing division of opinion it is possible to present claims as unarguable and render a wide range of positions invisible - and therefore ludicrous if, as is normally necessary in grant proposals, succinctly expressed. In this way funding can be restricted to the work of the researcher who has cornered the area.

Also contrary to common expectations is the fact that improved understanding often stems from heated, challenging, public debate - not only about concepts and ways of thinking about things, but also about the *implications* of one's data. For example, many of the insights which have been shared in this book have arisen from discussions stimulated by giving conference papers and writing articles in which what we can now (with the benefit of hindsight) see to be utterly naive - and sometimes completely incorrect - interpretations of the (still more incomplete) data which were then available were presented. This assertion that useful understanding emerges from public debate between scientists who acknowledge that there is a good chance that they are wrong conflicts with the widely held view that individual scientists should try to ensure they are right before they say anything. When arguing about the value of scientific work it is vital to distinguish between the outcomes to be expected from the scientific process and the outcomes to be expected from the work of an individual scientist. It is the scientific process which leads to "truth" and unarguability; if we demand that significant work from an individual be beyond dispute this will starve the process of the data and insights it needs.

It is important to note that serious critique of accepted positions requires enormous creativity as well as commitment of time and effort. It is largely precluded by the publish-or-perish philosophy which dominates academe and by the current arrangements which attempt to progress policy through expert committees. For example, Trainer's critique of the Brundtland Report shows that the references in the Report to the global problems we face (and which therefore make us feel that the issues are being addressed *in* the Report) are a kind of window dressing. They are not tied in to the thinking of the committee. As a result, the Report's recommendations imply not only that things can go on much as they are but even that, if they are allowed to do so, they will actually lead to the solution of the extremely serious problems documented in the report. What we are saying is that a proper understanding of the scientific process means that we will have to pay special attention to the task of identifying serious critics with unusual views like Trainer and providing them with the time and resources they need to do their work.

### *The Role of the Universities and Research Institutes*

In the course of this book we have seen that common sense and existing "good practice" are not an adequate basis on which to build a programme for remedying the conspicuous problems of society. The causes of our problems are often deep-seated and rooted in causes far removed from their symptoms. They have major, value-laden and political components which are difficult to address in the face of widely held beliefs concerning the workings of society. But the observations of greatest significance 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 activities.

The universities clearly have a major role to play in carrying out such research. Nevertheless, collecting the information and developing the understandings and tools required to solve the problems facing society are difficult and adventurous activities. As a result, the kind of research required is not easily reconciled with that characteristic of university-based social science over the past 50 years. This has tended to be unadventurous, non-controversial, and literature-driven instead of problem-driven. It has most often been of a "disciplinary" nature and more concerned with securing the promotion of those who conducted it than with advancing understanding or solving social problems[24.6]. Problem-driven research requires a wide variety of full-time researchers and institutional arrangements which enable those concerned to contribute in different ways to team-based work conducted with a sense of urgency. Nevertheless, it also involves considerable fundamental research - which can, however, often only be undertaken in an action context.

### *The Relationship Between Research and Its Users*

A great deal of research fails to result in the kind of development to which it points. We have identified eight main reasons for this:

1.  The enquiries which were expected to provide a basis for action were insufficiently broadly based. They failed to draw attention to crucially important contexts. One example is researchers' failure to draw attention to the role which the sociological functions of education play in deflecting the system from its manifest goals.

2. Insufficient attention was paid to the kind of research required or the processes required for its effective initiation and conduct. By and large, it is the insights gained in the course of research - rather than the specific information collected - which turn out to be useful. That is to say, it is the true scientific contribution - with that word properly understood - of the work (rather than the "facts" which turns out to be crucial. To carry out adventurous research capable of generating new insights, researchers require considerable scope to identify the problems to be tackled with the concepts and tools they have available. To do this, they need close links with forward-looking policy makers so that they can be helped to discern the mountains to be climbed. But they have to be left free to decide for themselves on the particular mountain to scale with the tools they have available, with their own specific competencies, and with such information as they already have. They need to be able to decide for themselves which route to take in their efforts to make the ascent. It is important for them to have opportunities to follow their insights through into evaluated and monitored action so that they can revise and refine their understanding as they work on evaluation tools and are forced to confront previously overlooked aspects of the problem.

   Very few policy R&D units have been established with the required balance of links with, and independence from, the public service. On the contrary: Policy research units have tended to be grossly under-funded and their staffs have been viewed, not only as mere hands hired to collect the "data" which public servants felt they needed, but as agents to be employed to vindicate policies already in force. Indeed, the data they have collected have often been tailored to that end: Researchers are often under strong pressure to falsify their results to vindicate established policies. The notion of broadly-based and genuinely independent research has proved an anathema to most political parties who feel they need to be seen to be authoritative and appear to *know* what needs to be done. This in turn stems, at least in part, from acceptance of the myth that the role of government is to tell the public service what to do and the role of the public service is to do it.

3. Although, as the work of several investigators[24.7] has shown, it is vital for the researchers to be involved in the implementation process if the results of research are to be used, it is, as we have seen, widely believed that the scientific integrity of researchers is called into question if they become involved in campaigns of the required type.

4. The efforts made to "implement the results" were too narrowly based. They did not involve network-working and parallel organisation activity. Too few people who possessed relevant information were involved. The importance of evaluating and debating the results of a variety of experiments based on different priorities and definitions of the problem with a view to finding ways of reconciling conflicting interests was not recognised. There was too little emphasis on using  research - especially action-research - to clarify goals. And the iterative, cyclical, model of active experimentation, learning, re-consideration of both goals and strategies, and adaptation advocated above was not followed.

5. There was a failure to assign responsibility for initiating innovatory action to any particular individual or group. However, even if this had been done there are no appropriate procedures to hold individuals or groups accountable for carrying the action through into pilot programmes, monitoring the results, and taking appropriate action to ensure, not that the original recommendations were implemented, but that the action based on them was, in some sense, successful. All too often, those who are responsible for initiating actions are not around when the effects become apparent.

Indeed those effects have typically been obscured by subsequent "innovations" based on different assumptions and carried out by different personnel.

6. A persistent reason why little action has been taken on the basis of good information has been the power of ideology to over-ride the truth. We may recall the numerous examples Thurow has assembled of politicians imposing ideologically-grounded policies despite abundant evidence of their failure and good information about what would in fact work.

7. Perhaps more important than ideology has been the fact that many policies have, deliberately or as a result of the operation of hidden systems processes, not been about what they have been said to be about. The research was a mere distraction, window-dressing, or epiphenomenon. We have seen, for example, that the policies which have been imposed on the educational system, apparently on the basis of ideology, may well have been not about improving education at all, but about legitimising a social order, shifting the blame for failure from societal managers to teachers, pupils, and parents, and entrapping everyone in a competitive system which disempowers all.

   It is important to note, however, that attributing failure to implement the results of research to ideology, conspiracy, or systems processes is an avoidance of the issue. What it really means is that the research itself was insufficiently broadly based. This in turn shows that it was not conducted in the context of appropriate understandings of the scope and methods of research. (Which is, itself, partly a product of the operation of systems processes or Machiavellian intervention.)

8. There is a general failure to understand the process of innovation and apply that understanding to public policy. It is widely believed that committees of experts should be able to generate a blueprint for action, instead of merely suggesting a direction in which a solution might be found - and leaving the evolution of the details to committed, enthusiastic individuals charged with (and held accountable for performing) the task of implementing pilot programmes, monitoring the results, and taking corrective action (reformulating both the goals and the means to be used to achieve) as they go along.

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The points to be drawn out of this discussion are that:

1. The information required if action is to be taken is frequently *not* "precise answers to administrators' questions" but much more broadly-based information, the importance - or even relevance - of which is often not suspected at the start.

2. The process of collecting and acting on information requires changed expectations of researchers, and changed relationships between researchers and decision-makers. It requires wider recognition that the latter are frequently not "powerful", "central" figures but, for example, nurses, teachers, subsistence farmers, and parents. This has major implications. It means, for instance, that instead of seeking to prescribe the actions of people on the ground (thus usurping their roles and responsibilities), the "central" policy makers and administrators' primary responsibility is to find ways of tackling wider systemic processes and to release a surge of effective innovation. Both the job descriptions and the tools of accountability which would be required to get central administrators and all the other policy makers (citizens) involved to behave in appropriate ways have been missing. The development of a better understanding of the required arrangements and of the necessary staff appraisal tools thus emerge as among the most important tasks facing researchers.

3. It will be necessary to create very different mechanisms for the initiation of research and, in particular, to ensure that marginal groups take part in a way which does justice to their perspective.

4. We will need to change the way research is conducted and evaluated. Clearly the universities, as they are currently organised and run, do not offer a good base. Researchers need to be appraised neither in terms of the quantity of their publications nor in terms of their failure to evoke criticism. Instead, they need to be appraised against the criteria we have recommended for other public servants. Research teams and units need to be appraised in terms of whether they are characterised by a climate of commitment, innovativeness, resourcefulness, and support for innovators: Are the diverse talents of different people being identified, developed, and harnessed with a view to generating new ideas, finding new ways of thinking about things, testing important hypotheses, disseminating the work, trying to translate it into action, inventing ways of finding out whether the action is working (and if not why not and what undesired and undesirable effects it is having), and so on? The question is not whether those involved are spending their time, money, and resources in prescribed ways or in producing numerous publications. If there is a climate of innovation and dedication to scientific advance, one can be sure that, in the end, some, if not all, of the necessary developments will follow automatically.

*Requirements From Research*

In the this section we will bring together, and attempt to become more specific about, some of the understandings and tools we most urgently need from research.

*Understanding How to Run a Sustainable Society*

We have very little understanding of how to run a sustainable society or what such a society would look like. To get anywhere we need some tools. For a start, to move in the direction of a sustainable society we need to be able to monitor progress toward specific sub-goals. More than that, we need some form of accounting system which will enable us to trade off progress toward some of these goals against others. Since many of them cannot be formulated in monetary terms - indeed it has been the tendency to focus only on costs and benefits expressed in monetary terms which has led to the neglect of these goals - it will be necessary to develop multiple-criterion accounting systems.

At an individual level these should assess quality of life in all its aspects ranging from satisfaction with human relationships, through satisfaction with the aesthetics of the environment and opportunity to contribute to the community, to relationship with politicians and bureaucrats.

It will also be necessary to develop means of assessing costs, at individual, organisational, and societal levels. Such costs include: consumption of non-renewable resources, contribution to pollution, contribution to the destruction of the fertility of the soils, the seas, and the atmosphere, and even to the destruction of human creativity and initiative.

Then there is the question of giving people credit for their contributions. These include:

- Contributing to the management of society through network-based, "citizenship" activities. This involves such things as monitoring the quality of the environment,

supervising the public service, overseeing activities of TNCs and other companies, and contributing to public debate. The price of preventing the degradation and destruction of the planet is eternal vigilance, and those who play a major role in such activities should be able to have their contributions recognised.

- Organising their lives in such a way as to minimise environmentally destructive activities (by minimising their direct or indirect use of energy, for example).
- Contributing to the overall quality of community life in non-monetary ways - such as by providing services to the sick, disabled, lonely, and distressed or enhancing the beauty of the environment.

There is currently little understanding of how to compare the true natural or human resource costs of different ways of doing things, even in skills exchanges or "green dollar accounting" systems. For example, how is the value of time spent looking after a terminally ill relative to be compared with the value of time spent looking after children, providing professional medical assistance, building a house, or growing food? Failure to engage with such problems is the most widely cited defect of our existing financial accounting arrangements. The market does not engage with what is most important, whether in terms of costs, benefits, or needs. But clarifying what should be taken account of and weighting it appropriately poses enormous problems, and we will return to the issue when discussing the kind of experiment it would be desirable to undertake with LETS schemes.

The context of such problems should not be overlooked because that in itself poses enormous problems for research and innovation. For any information-based accounting system to work, it will be necessary to generalise it in such a way as to control international trade.

Despite the obviously considerable work needed to develop these alternative accounting systems, it is worth noting that the importance of real-resource accounting was recognised by the very people who are thought of as the architects of our present monetary system. Thus Adam Smith wrote: "Labour alone, therefore, never varying in its value, is alone the ultimate and real standard by which the value of all commodities can at all times and places be estimated and compared. It is their real price: Money is their nominal price only". Ricardo and Lincoln made similar observations. The problem is still to find ways of translating such ideas into effect.

*New Tools to Provide and Evaluate Diversity*

In areas like health, education, and housing, we badly need to offer and evaluate diversity and provide people with information on the consequences of the options which are offered.

The provision of meaningful choice and variety involves:

- Studying the population's needs and *deliberately* creating variety *within each geographical area.*
- Collecting and disseminating information on *all* the personal and societal, short *and* long-term, consequences of each of these options. (An activity which benefits the individual may harm society; an activity which confers immediate benefits may, in the long run, be harmful.)
- Demonstrating that each of these options is of high quality.
- Documenting what is actually happening on the ground and providing means of stepping in to influence this. (An example may help here. Although educational programmes of a

particular type may generally nurture certain qualities - such as the skills required for active citizenship - what is actually happening in a particular school or classroom claiming to implement the programme may be very different. Consequently, there has to be some means to influence what is going on.)

Tools are needed to:

- Evaluate each of the options from the point of view of each interest group.
- Provide institutional accountability: Are the services being provided effectively?
- Monitor organisational climate: Are the organisations concerned dedicated to establishing and achieving innovatory goals, characterised by a climate of innovation, and staffed by committed, energetic, and enthusiastic people?

It is important to note that collecting comprehensive information - i.e. information all short and long-term, personal and social, consequences - (a requirement repeatedly emphasised in this book) means sacrificing some concern with accuracy. Likewise, collecting information on the long-term consequences for such things as the probable future economic development of the community means that it is necessary to adopt an "illuminative" approach to evaluation. One would study the processes currently operating and infer what their long-term effects on numerous outcomes are *likely* to be. "Documenting the consequences of options" therefore involves building up an *understanding* of social processes. It involves *fundamental* research which has, however, to be conducted in an *action* context. Yet this notion seems to many people - who are steeped in current understandings of research - to be a contradiction in terms.

*Experimentation with LETS Schemes*

Despite the fact that local currencies and other Local Employment and Trading Systems (LETS) have obvious difficulties and do not engage with many of the wider problems discussed in this book, many insights could be gained from experimenting with them.

As has been shown, the basis on which exchanges are made in LETS schemes is not pre-determined. Exchanges can be based on current monetary values, on equal "pay" for equal time, or on any other agreed basis. Each alternative merits careful evaluation. The effects will be different in societies making different assumptions. Thus equal pay for equal time (which, in effect, ignores different levels of skill and discounts the time required to develop the skills needed to carry out the task in the first place) will be less well received in a society which accepts the myths about differentials being necessary to motivate work and reward high-level talent. Even more "absurd" is the possibility of not counting the cost at all. Yet this variant merits particular study. Although, given our Western mind set, it is difficult to believe that people might be willing to give of their labour without counting its cost and expecting reciprocity, Goldsmith has claimed that many vernacular societies worked in this way, and, in some ways more importantly, that a great deal of behaviour in our own society is of this sort. For example, very many wives and mothers relate to their families in this way. They do not attempt to calculate what they will get from their children in return for many of their actions: They just do what needs to be done because it is the right thing to do.

In considering the kinds of experiments needed with LETS we should underline the need to explicitly set out to initiate schemes which provide a basis for developing the community care arrangements required to replace drugs-based health care and commercial insurance.

More generally, experimentation with LETS provides an opportunity to explore how to move toward a society in which the most important, quality of life enhancing, public provision is explicitly managed on the basis of information - without reference to finance - and a token system is adopted for personal discretionary spending. As has been emphasised, mutual support networks have great advantages over formal health care and social work arrangements not only in terms of their cost but also because they result in attention being paid to the *most* important determinants of quality of life - like companionship - of the recipients on the one hand and enhance the quality of working life of those who provide the services by giving them the satisfactions which come from doing work which is really required and offers real benefits on the other. Explicit experimentation with LETS schemes which took these wider possibilities and benefits seriously might lead to the invention of ways of overcoming some of the main defects of monetary accounting and at the same time help us to move toward the more general adoption of moneyless public provision, involving extensive variety, choice and experimentation, but administered almost entirely on the basis of information and a parallel system of tokens to facilitate the meaningful exercise of choice between options in limited supply.

While advocating experimentation with developments in LETS and related schemes it is important to recognise their limitations. These are that they: (1) Do not address other huge problems of formal economies - such as the twin facts that, on the one hand, formal economies fail to take account of a large number of benefits which currently derive from the informal economy and, on the other, externalises huge costs; (2) Do not focus central attention on the decisions that are taken and the arrangements for taking those decisions. LETS schemes, like the monetary economy, are *not* self-actuating. Their operation and effects depend on endless decisions, and it is on the considerations which enter into those decisions and the way they are activated that we most need to focus in finding a way forward. Using the word "experiment" in the way in which it has come to be used in earlier chapters, experiments with LETS schemes, if conducted in the context of a focus on what can be learned from the problems and dilemmas that are encountered, provide us with an important opportunity to bring these vitally important issues to the centre of attention.

*New Government Structures*

Some of the most important developments we need have to do with the creation of open governmental structures. At present it looks as if these should be organised around the ideas of Toffler or Schon (who have written about "networks") and Kanter (who has written about "parallel-organisation" activity), with a view to supervising the operation of public and private sector organisations more effectively and inducing them to act more in the public interest.

The objectives of these new structures would be to bring to light, and publicise, issues of public concern and help to ensure that any decisions and actions take full account of their probable long-term social and environmental consequences.

Comprehensively evaluated experimentation with, and development of, these arrangements is of vital importance. These experiments could be introduced by citizen action - without any government initiative - in education, urban planning, etc. (although those concerned should prepare for reactions like that of the Polish government when it squashed early attempts to re-introduce Semjicks). It would be valuable to try out alternative ways of inducing citizens to

participate in the monitoring arrangements. These might include requiring employers to release (as with Jury Service) employees on full pay to participate in activities which are, after all, social wealth-creating or they might involve the provision of "benefits in kind" through a skills-exchange mechanism. Insights into new arrangements of this kind might also come from carefully conducted cross-cultural studies examining what is done in such countries as Norway and Japan.

*New Procedures for Staff Appraisal*

One of the most important developments is the creation of new criteria and procedures of staff appraisal. The two most fundamental shifts required here are, firstly, from an emphasis on whether staff have achieved outcomes which someone else has determined to whether they are engaging in activities that are *likely* to lead to innovation. And, secondly, from an emphasis on appraisal as a tool to decide on who will be promoted to an emphasis on appraisal as a means of recognising the contributions people have in fact made and identifying motives and talents as a basis for future placement, development, and utilisation.

If these objectives are to be achieved we need to ask such questions as: What are the idiosyncratic motives of each person and what have been their most important contributions *in this area*? Are they initiating the collection of, seeking out, sifting, and acting on information *in this area* in an innovative way in the long-term public interest? Are they striving to release the energy, know-how and initiative of others? Are they trying to establish organisations which do this? Are they attempting to clarify systems constraints on what can be done and helping to set up and evaluate experiments designed to influence them?

Clearly no one person could be expected to do all of these things. But we need to discover the ways in which each individual might be strongly motivated to contribute to a climate of innovation. New tools are needed to do this. New tools are also required to discover whether an organisation as a whole is characterised by a climate of innovation and contributing as it might to the creation of such a climate in society.

Currently, our society is extremely reluctant to invest in the institutions which would be required to develop such tools. This is partly because few people know how it is to be done. But it is mainly because few understand even as much as did Rothschild[24.8] about the nature of the scientific process and the arrangements which are needed to conduct it. To these matters we now turn.

*The Organisation of Research*

The comments we have made have important implications for the organisation of research:

1.  We need to have many more teams researching each topic from different perspectives. It is *not* a "a waste of public money" to have a number of teams working on "the same" topic. We need to initiate projects grounded in a much wider range of viewpoints and assumptions and to ensure that there are many more replications of what looks like the same study. It is only in this way - and not through an authoritarian insistence on "quality" in science - that we will be able to detect fraudsters and discourage "sloppy" work from being presented as science.
2.  We need to ensure that being involved in controversy does not lead to the scientists concerned being branded as "unsound" and their work discredited.

3. We need to ensure that at least some members of research teams possess the competencies required to conduct public debate in order to both advance scientific understanding and stimulate improvements in policy and practice.
4. The interface between researchers, sponsors, and clients needs to be carefully re-assessed.

As Rothschild[24.9] noted, administrators, like most academics, 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 before being willing to fund work - and they are still more unwilling to commission work if they do not 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. Prior knowledge of methods and probable outcomes fosters security on all sides, but it leads to trivial research that fails to advance understanding.

Administrators have insufficient contact with a field to know that something is *possible*. They do not know they have a need for something until they see the product. These are classic problems which can, to some extent, be solved by market mechanisms. However, they are not usually solved in this way because, since the likelihood of a profit is too uncertain, most research has to be funded by government departments. The typical way forward involves neither market processes nor application for a research grant. Instead, as we saw earlier, innovative research is typically carried out with the aid of funds syphoned off from "defence" budgets.

Decisions about the research to be undertaken need to be more directly under the control of researchers and those who have, through parallel organisation activity, become aware of problems which need to be researched. Funding should be allocated on the basis of the competence of a given research team to initiate and carry through important innovatory projects. Clearly, such a scenario must also provide some means whereby administrators, a wide range of those involved in implementing policy, and the public can alert researchers to issues requiring investigation. The links - network and parallel organisation based working arrangements - to be established between researchers, administrators, practitioners, and the public, then become a topic for research and discussion in their own right.

The arrangements that do and do not need to be made can be illustrated from one Scottish Council for Research in Education project - a study which resulted in the book *Pupils in Profile*[24.10]. The work was initiated because a number of *head teachers* - not administrators - were concerned that current forms of assessment limited the educational programmes they could offer their pupils. Despite strong opposition from administrators, the project went ahead and, despite its inability to deliver the tools the head teachers wanted, documented many of the barriers to developing them and produced a system of "profile" reporting forms which have since been widely used. Unfortunately, the Ministry of Education then changed the arrangements for funding research at the Council in such a way that all work was funded on a contractual basis. As a result, the researchers found themselves unable either to capitalise upon their insights - or even to 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 in the US. These were designed to satisfy administrators' (changing) requirements. Despite the investment, they

almost completely failed to advance understanding of the issues. Ironically, the new understanding which *has* been achieved in the area comes almost entirely from very poorly funded work conducted by Levenstein[24.11], McClelland[24.12], and Sigel[24.13].

The question of how the clients of policies are to influence research deserves serious attention. The real clients of research - those who will benefit or suffer from the policies with which it is associated - are often in no position to commission it or to influence its orientation. Means are needed whereby - often marginalised - members of the public who have become aware of problems not perceived by others can bring some researchers to share their concerns and perspectives. There also needs to be some way in which those who have initiated such research can ensure that it continues to be carried out from their perspective. It is too easy for researchers to redefine the problem in their own terms (though this is often a good thing because researchers typically have access to a wider range of those on whom the policies impinge than do most administrators).

If researchers are not to be accountable to administrators, to whom are they to be accountable? The answer is the same as for (other) public servants. The work of research teams should normally be of value to a constituency which should be identifiable through the kinds of information-technology-based networks discussed earlier - that is, to a (possibly widely dispersed) group that finds the work useful. Accountability can also be provided by asking whether the research unit is characterised by a climate of dedication, high standards, commitment to finding new ways of thinking about things, new things to do and new ways of doing them, and engaging with society's problems. If the answer to all of these questions is positive, the chances are that the unit is doing work which will, in the end, turn out to be valuable.

There is a widely held - and apparently reasonable - view that social researchers should limit their proposals to those that are reasonable and realistic - i.e. to those that are likely to attract funding - for doing anything else would surely be a waste of everyone's time. They should therefore refrain from putting forward a case for developing the understandings required to make important changes which have no realistic chance of being introduced into the current system. That would both waste their time and discredit them.

The problem with this view is that our current ways of doing things (in e.g. education, economics, ecology, management) are so dysfunctional that anything which contributes to the perpetuation of those systems is unethical. The only way out of the difficulty is by developing the understandings and the tools required to do things which are "unrealistic". Failing to do so ensures that the most important actions for our future never get attempted. Contributing in a "reasonable" way to the debate contributes to continuing widespread acceptance of the framework in which it is couched and thus to an unsustainable situation. It allows those in power to control the issues which get discussed in ways which are very much to their advantage. It does not help others to get their minds round the issues.

But acceptance of the terms of discussion in the current political debate and making research proposals that are "reasonable and realistic" does more than allow those in power to determine the agenda. It connives in the process whereby the really important issues are rendered invisible for want of a *language* in which to discuss them. Those who go along with the process never call the tailor's bluff and reveal what the emperor is really up to. If they hesitate to say "No, if you *really* want to improve education, here are the topics you need to research", they will certainly quail at the thought of saying "Hey, but the proposals in this

Education Bill are not really about improving education at all ... they are about reinforcing the existing social order and creating a diversion under cover of which entirely contrary actions will be taken ..."

By couching proposals in currently fashionable - and meaningless - terms and limiting proposals to those which are likely to attract funding, one fails to provide the fundamentally important critique of the framework that is needed. There is then no development of a counter position, let alone development of the concepts and tools required to run an alternative system. There is no articulation of insights into what is actually going on. This process is unethical. It has more to do with researchers getting money for themselves by false pretences than with acting for the benefit of mankind. No wonder the public is reluctant to provide funding for the exercise.

We need instead to do the basic work necessary to find out how to do what needs to be done. Mankind would never have learned to fly by "building on good practice" (i.e. studying the feather-clad men who were able to get furthest when jumping off castle walls) or by "working with the system" when that system was completely off-beam. It was necessary to conduct the fundamental research into flight and aerofoils which led to Bernouilli's Theorem.

*The Role of Evaluators*

Specific problems confront evaluators in their efforts 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 Departments, the difficulties created by the absence of appropriate transport, deficiencies in building design, and the sociological functions being performed by the system even though these issues do not relate directly to the specifically *educational* aspects of the policy evaluated[24.14]. Day and Klein[24.15] 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 these were supported by those of other professional groups. They can also argue that their work ameliorates some problem which the evaluators have not looked at.

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 precision in the observations actually made. Failure to comment on an important consequence of the programme, 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[24.16].

A good evaluation, then, assesses how effective a programme *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 all the long-term effects of a programme, including negative social and educational ones (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 most academic research. This helps to explain the serious

problems which arise when attempts are made to locate genuine policy research in traditional "academic" institutions[24.17].

One of the most important developments needed is changed "Research Methods" which will make it possible to take account of, and capitalise upon, the constraints of real-world policy experimentation. In other words we need methods which will enable us to infer more effectively what is going on in messy situations. This means that we need to learn to piece together bits from here and there and to discount other factors which interfere. We also need to be able to predict the effects of different operating *contexts*.

After the results of an evaluation have been disseminated and debated, problems still remain for their translation into 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 Researchers*

Researchers often find it necessary to mount political crusades in order to ensure that their work is actually applied[24.18]. This detracts from the time available to produce the publications deemed necessary in academe. It leads to doubts being expressed about the "impartiality" of the researcher. It is therefore of the utmost importance to develop appropriate career structures for those involved in research. These should offer the security researchers require if they are to enter into controversial public debate. Working arrangements should also provide the flexibility required for redirecting work as required and the time needed to mull over, and draw out more fully, the implications of observations.

A few words must also be said about the timescales needed for useful policy research. On the one hand, it is necessary to make significant progress in a limited period of time. This cannot easily be achieved in the individualistic (non-team-based) atmosphere characteristic of academe, still less in the one-third time usually earmarked for research in academic contracts. On the other hand, the timescales for useful research are much longer than typically assumed by sponsoring agencies. Researchers need to pursue problems which were not obvious and to do the conceptual, inventive work required to find ways of thinking about and tackling them. Many of society's problems are chronic, having existed for centuries. They will still be around tomorrow and are not amenable to quick solutions. 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, where the interval between the proposal, the report, and the next proposal is insignificant, where there is little time for exploratory work or for developing understanding and new measures prior to rushing into the field, or where there is a long delay between researchers identifying a problem and obtaining funds.

Scientists need to be encouraged to report work carried out with imperfect tools and imperfect methodology, and to air their impressions of the policy implications of their work. Without these, there will be no discussion of some of the most important policy issues. 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*

We have seen that a vast amount of research is required as a basis on which to run a sustainable society which will be managed using explicit information. This research is needed not only to assess the effectiveness of current policies, and develop alternatives, but also to develop the very accounting and administrative tools and organisational arrangements required to run such a society. Absolutely key developments include new tools for staff appraisal and to assess organisational and community climate, and new arrangements for the members of society to oversee the work of the public service.

The work required features elements commonly associated with both fundamental and applied research. It demands conceptual advances involving new methodology and experimentation, but it can only be carried out in the context of theoretically-based, systemic, action. Its execution demands inquisitiveness, inventiveness, and dedication to theory-building. It requires extensive network-based team work over an extended period of time, but it needs to be carried out with a sense of urgency. It 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 characterised academe.

New institutional arrangements based on a better understanding of the nature of science and the procedures required for advance are required. Both the scientists themselves and the institutions in which they work need to be held accountable using the very appraisal tools, and through the very organisational arrangements, that it would be their primary function to develop.

Although the topics discussed in this section may have seemed to be of marginal interest and concern, this has been the most important chapter in this book. While the information and ideas presented earlier may have been illuminating, without new arrangements for collecting, sifting, and empowering information, and without the tools that are required to run our society effectively, we are doomed.

*Notes*

24.1  See House (1991) for an account of the need to get behind the bare data to discern the underlying and invisible structures and processes.

24.2  Kuhn, 1977

24.3  An account of the ways in which universities stifle innovative research will be found in Nisbett (1990).

24.4  For a discussion see Raven, J.C., Raven, J., and Court (1994).

24.5  Hamilton et al., 1977; House, 1991

24.6  It has been found that out of every thousand AERA (American Educational Research Association) publications only twenty contain *new* data and in only five of these is the data substantive; the rest are written to satisfy the "publish or perish" machine which characterises all research at the present time.

24.7  Cherns, 1970; Freeman, 1973; Roberts, E.B., 1968

24.8  Rothschild, 1971

24.9  Rothschild (1971) clearly recognised (i) that scientist-initiated basic research is of vital importance and needs to be well funded because only scientists can tell what is likely to succeed (and even then only with great uncertainty), and (ii) that even the development process requires sponsors to fund activities designed to try to find ways of doing things which no one knows how to do. Rothschild wanted 10% of the *total R&D budget* - an enormous sum of money - to be earmarked for scientist initiated research. His widely cited customer-contractor principle applied mainly to the *development* area. But even here it is clear that he recognised he was dealing with a high-risk activity saying that "the contractor does it *if he can*, and the customer pays. (And he did not mean that the customer only paid if the work was successful!) In a later report (Rothschild, 1982) he argued forcefully for a much greater research budget in the social sciences so that they could undertake the kind of large-scale project envisaged here in order to help society to engage with its urgent and pressing problems.

24.10 Scottish Council for Research in Education, 1977

24.11 Levenstein, 1975

24.12 McClelland, 1982

24.13 Sigel, 1985, 1986; Sigel and McGillicuddy, 1984

24.14 For an illustration of the non educational barriers to educational innovation see Schwartz (1985).

24.15 Day and Klein, 1987

24.16 For a fuller discussion see 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 and his colleagues (1977) 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 is then 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 a student's 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 (Atkinson et al., 1988; Jacob, 1987, 1988), but it has found endorsement in the work of House (1991) and Salomon (1991).

24.17 The way in which the extraordinary requirements of effective evaluation can be approximated are hinted at in the previous footnote, and are discussed in Raven (1991). The problems which effective evaluation poses for evaluators and their deployment are discussed in several chapters in Searle (1985).

24.18 Cherns, 1970; Freeman, 1973; Roberts, E.B., 1968, 1969; Tizard, 1990