

No Hiding Place: On the Discomforts of Researching the Contemporary Policy Process*

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ABSTRACT

It has never been easy to conduct research into currently sensitive policy issues, but there is now accumulating evidence to indicate that various forms of resistance to scholarly investigation are on the increase. Such a climate handicaps all social policy research, but may have the greatest impact on ethnographic projects. Yet, it is argued, ethnography is increasingly widely recognised among academics as having a particularly valuable contribution to make to the study of the policy process. Unfortunately, many policy practitioners (and occasionally some academic colleagues) perceive ethnographic research as being of questionable validity and low helpfulness. This behoves policy-oriented ethnographers to demonstrate that they do indeed have procedures for assuring validity, even if their style of investigation is never likely to be popular with government.

But in an era of bad faith, the man who does not want to renounce separating true from false is condemned to a certain kind of exile (Albert Camus to Jean Gillibert, February 1956).

BACKGROUND

The policy circumstances which most cry out for independent investigation are precisely those in which bad faith is likely to be present in epidemic proportions. These circumstances—to be described in a

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moment—constantly threaten researchers (both men and women) with exile. Fortunately the most common kinds of exile in Britain in the 1980s and 1990s are less harsh than those Camus probably had in mind. They include grant starvation, denial of access, restrictions on publication and sometimes increasing professional isolation as one is unable to come up with an institution—and career—enhancing ‘product’.

Bad faith easily catches hold where a strong government has introduced a major new policy, backed it with substantial resources and invested it with high symbolic significance in terms of the currently dominant political ideology. This has always been the case, but since the mid-1970s, with the collapse of the Butskellite consensus (such as it was), the rate of infection seems to have increased. In recent years the sale of council houses, the deregulation of buses, the reform of social security, the substitution of the poll tax for the rates, the Enterprise Zone policy, and the privatisation of certain previously nationalised industries, are all examples which meet these criteria. But organisational changes, too, may generate bad faith. The present government invested a good deal of symbolic as well as substantive importance in, for example, the Financial Management Initiative in Whitehall, and the introduction of general management to the NHS, and, more recently, the development of ‘internal markets’ in health care and education.

How and why does bad faith arise? It grows because, in an era when public sector careers are more precarious than before, and resources more scarce, many senior officials can no longer afford the luxury of ‘standing off’, of taking a detached view of the policies they are supposed to be implementing. If the policy is a prominent one they are expected to ‘lead’, to be unwaveringly positive and enthusiastic about it, at least in public and in the presence of political leaders (Exley, 1987, p.47). Their ‘professional’ views are increasingly demoted to the status of ‘private’ opinions, to be allowed out only to close friends or suitably harmless and remote strangers. Civil servants and others ‘may feel well advised to adopt the fashionable idiom’ (Fry, 1988, p.18). ‘Bad faith’ should here be understood in the Camusian sense. In other words there may be no need for crude, direct falsehood. Rather, those associated with a policy programme feel it prudent to suppress their own critical faculties and the authentic doubts of those of their colleagues, and exude, instead, unwarranted confidence and/or synthetic enthusiasm (Laurance, 1988). What is more, this mood has gradually been extended to the business of noticing, commissioning, or even allowing, policy research.

In the health care field certain kinds of research into Aids are suppressed, apparently by direct Prime Ministerial intervention (Water-

house, 1989). In social policy the government is (as Peter Townsend put it) 'steadfastly averting its gaze from the growing volume of studies reaffirming the connections between poverty or deprivation and ill-health' (Dunn, 1988). (A junior minister quickly dubbed this speech conclusive evidence that Professor Townsend's views were 'on the very far left'.) With politicians 'bad faith' may go further. Inconvenient research findings may tempt them to manipulate statistics (Waterhouse, 1989); refuse publication, (Phillips, 1988); effectively to blacklist individual researchers; or even to attack their motives. Memories of the Community Development Projects should caution those inclined to believe that such incidents are exclusive to Conservative governments, but the research community certainly has no current shortage of anecdotes of this kind. Increasingly the emphasis by state research funding agencies is on 'relevant', 'useful' research, on tightly-focused projects which are unlikely to raise 'pointless' questions about the fundamentals of the policy in question. Indeed, in the UK as in the US (Fischer, 1987), the whole notion of 'policy analysis' and 'policy expertise' is under official suspicion or outright attack—the abolition of the Central Policy Review Staff being only one example. These tendencies are just as noticeable in the natural sciences as the social sciences, and perhaps reach a peak in defence-related technological research (Hanson, 1988; Marchant, 1988). In sum, short term political agendas have further strengthened their influence at the expense, *inter alia*, of a more measured search for dispassionate assessment and scholarly understanding.

PLAN OF THE PAPER

This background is discouraging to many—perhaps all—types of research which touch upon sensitive policy areas. It will be argued here, however, that in combination with other factors the heightened incidence of 'bad faith' damages certain species of investigation more than others. The present climate is especially uncomfortable for one particular type of research, one which we see as having a distinctively valuable contribution to make to policy studies. We refer to the ethnographic approach. This was once thought of as mainly the province of anthropologists investigating remote cultures, but now it is becoming widely recognised across the academic world as possessing advantageous features with respect to the analysis of the contemporary policy process.

Yet alongside its particular advantages, the ethnographic approach has at least one special vulnerability. Often it has been seen by academics

themselves as insufficiently 'rigorous' or 'objective', at least compared with explicitly experimental methods, or with the impressive quantifications of modern economics. Suspicions concerning the validity of ethnographies may be even more prevalent among policy practitioners, who usually desire (seemingly) 'hard', 'scientific', politics-free research findings on which to stand their ground.

In the remainder of this paper we seek to connect these issues of, on the one hand, 'bad faith' and, on the other, the applicability and validity of different types of policy research. First, we rehearse the reasons why natural science/experimental models of research are so frequently of limited usefulness for the investigation of contemporary policy. Second, we similarly rehearse some of the limitations of currently fashionable varieties of economics. Both these sections summarise what should be fairly well-known epistemological characteristics, so they are very brief. Third, at slightly greater length, we elaborate the distinctive features of the ethnographic approach. This is followed by a few observations on 'practitioner theory', and finally by a case study of a fairly large scale piece of ethnographic research into the NHS. This case should illuminate some of the problems with 'bad faith' and with practitioner expectations, some of the problems and possibilities surrounding the validity of ethnographic research, and the inter-reactions between them. In the light of this illustration we attempt a few concluding observations.

THE LIMITATIONS OF THE NATURAL SCIENCE MODEL AND OF NEOCLASSICAL ECONOMIC APPROACHES

These limitations begin with a huge one. For most contemporary policies, no randomised controlled experiment—or even quasi experiment (Chen, 1988, pp.3–5) is possible. The practical, ethical and methodological problems of such an idea are overwhelming. In those comparatively rare cases where experiments are possible the hypotheses to be tested tend to be of narrow scope, whereas the public, practitioners and many academics are frequently more interested in broader issues—'did policy X work?' Furthermore, 'we enter a policy arena not at the beginning, but always in mid-current, acting always against a background of experience and precedent, structured expectations, the manifest workability of the going concern' (Anderson, 1987, p.33).

Additionally, the 'natural sciences' model has serious philosophical limitations, at least in the context of social policy. Insistence on a deductive–nomological procedure, and emphasis on prediction and control 'inevitably gives rise to an emphasis on social engineering and

instrumental rationality as the best methods of identifying and attaining policy goals' (Healy, 1986, p.383; see also Fay, 1976). A positivistic concern with the identification and manipulation of causal variables fails to 'take adequate account of the inter-subjective cultural context which renders social action meaningful' (Healy, 1986, p.386).

The practical and philosophical inadequacies of positivistic natural sciences-type approaches have been commonplaces of the academic literature for well over a decade (Bernstein, 1976; Young and Mills, 1981). More recently, however, a new approach has rapidly gained favour, especially with policy practitioners. A recent editorial in the journal *Policy Sciences* refers to the 'challenge of the new "economic imperialism"—neo-classical economics, especially of the heavily mathematical kind' (Ascher, 1987, p.5). This approach often achieves considerable rigour and no little 'relevance', but at a high price in terms of (over?) simplification. It frequently asserts or assumes, a generalisable preference ordering. It presumes some sort of macro-level equilibrium. It also takes a narrow view of human motivation, assuming the widespread existence of rational utility maximisation and only reluctantly acknowledging the existence of other cognitive procedures such as habit, loyalty, or allegiance to some collective or co-operative ideal (Charles and Webb, 1986, p.69). Despite, or because of, these limitations neo-classical economic analyses are relatively popular with policy practitioners. They appear to be 'objective', they are quantified, they usually have a 'bottom line'. They are also ideologically acceptable to right wing neo-liberal politicians.

Despite the limitations indicated above, approaches using natural science or neo-classical economic models have frequently been preferred to qualitative, and especially ethnographic research. For example, in a review of nursing research from 1975–86 Robinson and Elkan comment on the paucity of ethnographic work and the frequency with which research consisted of 'huge slabs of data ... followed by one or two minor recommendations' (Robinson and Elkan, 1989, p.12). The academic referee of this review commented that:

Positivistic science tends to be more convincing than qualitative or action research. Hence it is not surprising that nurses carrying out and commissioning research have tended to favour the more quantitative methods (Robinson and Elkan, 1989, p.123).

Similarly, much research into health policies more generally has been epidemiological/statistical or economic. The dominant vocabulary of such studies has included mortality and morbidity rates, admission,

throughput and provision rates, costs per case, staffing ratios and so on. While such measures are clearly indispensable they do not, by themselves, give us access to the world of meanings, choices and resultant behaviours. We may know what happens but not, in intersubjective terms, why. Quantitative measures, to quote the DHSS's own booklet on performance indicators, 'are intended to raise questions and to highlight issues for further discussion and investigation in the light of local knowledge' (DHSS, 1988, p.7). To answer these questions will often require another kind of knowledge, local and interpretive. Thus studies of the natural science, economic and Operations Research (OR) type constitute a challenge to 'post positivists' to attempt 'greater systematisation of our study of subjectivity'—a distinguishing attribute, Ascher suggests, of the policy sciences (Ascher, 1987, p.8). It is to techniques for studying subjectivity that we now turn.

ETHNOGRAPHIC APPROACHES

We would suggest that the ethnographic approach currently offers the most obvious and promising alternative to the natural science model and economic approaches. Ethnography has a long, respectable history in sociology and anthropology (Agar, 1980; Kirk and Miller, 1986, pp.32–42) and its relevance to policy studies is belatedly beginning to receive serious attention (Bloor, 1979; Young and Mills, 1980; Ascher, 1987). We here define ethnography broadly, as that tradition in the social sciences where the method is to watch and interact with people on their own 'territory' and using their own language (rather than, for example, the technical language of the experimental psychologist or the econometrician). Within ethnography there are different emphases as to the extent to which the investigator is entitled to impose his or her agenda on the 'subjects', but the general predilection is for great caution in this respect. Even if the ethnographer goes in wanting to find out about some particular aspect of local beliefs or practices it is a requirement of the approach that space is given for the subjects of the research to redefine or reinterpret that focus. To find that the 'locals' have very different conceptions of (whatever is the focus) from those initially held by the researcher is a common and entirely legitimate outcome of ethnographic investigation. It is also part of this method that, unlike the natural science experiment, the 'subjects' of the research have some say in setting the terms of the research. Indeed, in the case of research involving high status policy actors the researchers may themselves have little choice—mandarins and cabinet ministers are not likely to consent

to filling in elaborate forms or lying on a couch and 'free-associating' (Young and Mills, 1980). In addition to these 'naturalistic' and 'participatory' methods it is also a defining feature of the ethnographic approach that it is concerned with handling highly subjective data—the individual perceptions and behaviours of the members of the group, institution or society which is being studied. In some cases it may even be the apparently most minute or trivial aspects of these which prove to be of particular interest (see, for example, Strong, 1988). The richness and 'how-it-is-ness' of this kind of data is naturally offset, from the researcher's point of view, by its untidiness, and so it is not surprising that much debate goes on among ethnographers concerning the most appropriate techniques for recording, ordering and presenting the mass of data they wrestle with.

What, then, are the strengths and weaknesses of this approach? First, it can get to the parts which experimental methods cannot reach, that is, it can still be used even where the researcher:

- (i) Cannot determine the goals of an intervention;
- (ii) cannot control the inputs to the situation being studied;
- (iii) cannot, or can only imprecisely, measure outputs (Illsley, 1980, p.115).

One or (usually) more of these conditions is present in most research into major policy innovations. This kind of research is often concerned with broad rather than narrow questions, with 'illuminative evaluation' of the policy process rather than the quantification of inputs and outputs (Weiss, 1977; Illsley, 1980, p.135).

Second, an ethnographic approach, properly handled, can be said to have ethical advantages over the orthodox natural sciences model in so far as it intends 'to foster human development through improved self-understanding rather than through strategic manipulation' (Healy, 1986, p.387; see also Fay, 1975). There is an explicit commitment to grounding the researcher's interpretations in the perception of those involved in the field of study.

This last feature has particular significance in the field of policy research. It should constitute a third advantage in so far as it helps ethnographers to elude the problem that 'evaluations based on experimental paradigms tend not to yield information useful to policy makers' (Chen, 1988, p.1). Ethnographers are much more likely to be 'talking the language' of the policymakers, because from the start they will attempt to enter the 'assumptive worlds' of those concerned (Young and Mills, 1980). On the other hand, they will usually want to talk to those who suffer from new policies as well as those who benefit, to the

powerless as well as to the powerful, and this may make their findings, though perfectly intelligible to the powerholders, unwelcome.

Fourth, there is the question of validity. Ethnographic enthusiasts sometimes claim that their favoured approach has advantages here too. Cronbach (1982), for example, argues that qualitative methods (which include ethnographic techniques) have greater potential for external validity than experimental methods. In this view internal validity, which is where the scientific experiment scores highly, is of inferior importance. (Roughly, internal validity concerns the degree of confidence that a particular input produced a particular output, whereas external validity concerns the extent to which a research finding can be generalised to factors like different settings or populations.)

Unfortunately, it is by no means clear that Cronbach's confidence in the superiority of 'naturalistic'/qualitative methods can be sustained. Some of the arguments in favour of these methods appear imprecise and 'inspirational' rather than logically convincing (Chen, 1988, p.14). Nor can the downgrading of internal validity be accepted without qualms. It is not necessary to demolish entirely the value of experimental approaches in order to establish that qualitative methods may have a distinctive contribution to make. Different approaches may be necessary to answer different kinds of questions. Where the experimental method can be used it may well have considerable internal validity advantages over ethnographic techniques. Certainly it has been a weakness of some ethnographic projects that they have not been able to describe their own methods of observation and enquiry in sufficient detail for internal validity to be assessed. 'Inside dopesters' accounts of policy formulation and party intrigues are frequently in this category—though that is not to say they are wholly valueless, especially if they are all that is available.

Fifth, although ethnographic approaches may be couched in the everyday language of the policy practitioners, they also seem to cause them some bewilderment. It is not that practitioners fail to understand what ethnographers are saying—witness the fact that the academic treatment of Whitehall most widely read by civil servants themselves during the 1970s was almost certainly Heclo and Wildavsky's *The Private Government of Public Money* (Heclo and Wildavsky, 1974). Rather, it is that often practitioners cannot immediately see what to do about the 'findings'. Characteristically ethnographic analyses do not conclude with clear recommendations for action, because that is not the spirit in which they are undertaken. Nor are they likely to contain the sort of numbers which provide a basis for action by managers and politicians whose concept of 'research' is a market survey, a financial

analysis or a controlled trial. Ethnographies are in a sense rather disturbing documents—they may ring uncomfortably true but they offer no specific remedy.

PRACTITIONER APPROACHES

If the paper thus far has given the impression that policymakers customarily choose between research findings generated by the experimental model, economic models or ethnographic approaches, that must now be corrected. Experimental and (more recently) economic approaches may be preferred to ethnography, but the strongest preference of all is frequently for no research of any kind. Instead, experience is taken as the trustiest guide—to solve a policy problem simply tap the advice of those who have extensive practitioner experience of the process in question. Some governments have relied more on civil service practitioners, others more on practitioners from the worlds of finance, business or commerce, but all seem to have shared a limited view of the role of social scientist (Bulmer, 1982, pp.27–29).

The fact that 'practitioner theory' has often proven contradictory, impractical or downright mistaken does not seem to have diminished the preference for pragmatism (for a small example see Pollitt, 1984, especially pp.150–165). John Maynard Keynes was one of the first to point to another of the limitations of this intellectual habit when he wrote that 'practical men who believe themselves to be quite exempt from any intellectual influences are usually slaves of some defunct economist' (Keynes, 1936). What is more, in the field of social policy, policymakers have often had their erroneous views on the avoidability of 'theory' reinforced by the willingness of researchers to fall in with empirical, positivist and piecemeal approaches (Bulmer, 1982, p.28).

During the 1980s the particular version of practitioner theory which has been held in highest regard has been that of managerialism. The solution to all manner of policy problems has been seen to be 'better management', and the gurus of better management have been private sector corporate managers and management consultancy firms (Pollitt, 1990). The hallmarks of this approach are in many ways particularly antipathetic to ethnography. Managerialist policy analysis is characterised by tightly-drawn terms of reference, short time scales, reluctance to explore 'political' issues and power relationships, and an overwhelming commitment to the rapid production of recommendations for action/intervention. 'Theory' is out, so in practice a curious mixture of unacknowledged and implicit theory is—inevitably—deployed, often in a highly rationalistic/positivist manner.

THE ETHNOGRAPHIC APPROACH IN THE CURRENT CLIMATE: AN ILLUSTRATION

Thus far we have argued that qualitative, interpretive approaches to policy research have usually been accorded unwarrantedly low status, especially by policy practitioners. The usefulness and validity of such studies have been under suspicion, and governments, to some extent aided and abetted by sections of the academic community, have preferred narrow, 'scientific' studies, economic analyses or pure 'experience'. This methodological bias is set against a background in which the climate for all research-based policy analysis may well be deteriorating relative to 'quick and dirty' practitioner approaches.

Some of these points can now be illustrated by reference to a project on which we have been engaged since 1985. The origin of the project was a perception on our part that the introduction of general management to the NHS was a fit topic for an implementation study. After all, everyone seemed agreed that this was a change of major proportions and the Griffiths Report (on which the changes were based) foresaw a major beneficial impact (Griffiths, 1983). The NHS Training Authority had commissioned some research but this appeared to be quite narrowly focused, being mainly concerned with collecting the views of a sample of District General Managers (DGMs) and turning them into training material. In these circumstances three of us, each fairly experienced in 'elite actor' research and well-versed in the specifics of NHS policy-making, set out to put together a research project. In what follows we discuss, first, some of the theoretical and methodological problems we faced and, second, some of our more 'political' difficulties. Before discussing these issues, however, we should stress that we do not consider ourselves to have had a particularly difficult set of experiences. Certainly one hears of far more bruising episodes on the academic grapevine —after all, in the end we did get some money, and we did get good access, without restrictions on our freedom to publish. This case is intended, therefore, not to illustrate an extreme, but to focus on the kinds of problems routinely faced by ethnographic approaches to contemporary policy change.

THEORETICAL AND METHODOLOGICAL PROBLEMS

We wished to study what NHS staff perceived to be the principal impacts of general management (GM) on their organisations. The first problem with this was that there were few studies of any kind of what NHS managers and administrators actually thought and did (Hunter, 1985).

So there was no ready-made theoretical tradition to step into, and there was not even a very clear or reliable picture of 'before' GM for us to compare with 'after'. Add to this the facts that, first, no 'control group' was available (because GM had been introduced everywhere), second, that inputs were not controllable (because all sorts of other things were happening to the NHS at the same time as GM) and, third, that the objectives of the 'treatment' (GM) had only been specified in a loose, qualitative manner (Griffiths, 1983) and the sheer impossibility of an experimental approach becomes immediately apparent. Matters need not have been so closed to experimental approaches. At the time the Griffiths Report had been published some organisations had recommended a trial of general management in one or two health regions, but the government had rejected this line of advice. More recently, the government has similarly refused pilot trials of the major provisions of the 1989 White Paper, *Working for Patients* (Cm 555, 1989).

Economic approaches seemed as inappropriate as experimental ones were impossible. Many of the benefits which were supposed to flow from GM were not reducible to a financial numeraire. For example, neither close definition of personal responsibilities among managers nor getting closer to the consumer were predominantly economic objectives. 'Savings' were certainly hoped for as a result of GM, but everyone from ministers down insisted that better financial management was only one element in the Griffiths package.

Eventually we decided on a research design that envisaged GM as an attempt to alter the balance of forces within a highly complex, multi-organisational, multi-professional environment that was subject to demographic and technological change. Our conceptualisation of GM led us towards certain bodies of theory and away from others. Neo-pluralism and neo-elitism both seemed to have something to offer (see, for example, Harrison et al., 1980; Dunleavy and O'Leary, 1987), as did the bureaucratic politics literature and some more recent developments in the study of 'interorganisational networks' (for example, Benson, 1982). All of these were centrally concerned to examine dependency relationships within and between large bureaucratic organisations and to acknowledge both the influence of specific institutional arrangements and the connections with larger and deeper structures of rules and interests. We also began to explore the possibility of extending their bodies of substantive theory by use of the formal taxonomy of institutional analysis developed by Elinor Ostrom (Ostrom, 1986).

In attempting to capture the impacts of GM we chose a variety of lines of inquiry. At an early stage we decided that the underdeveloped state of

even the descriptive literature on NHS management pointed towards a fieldwork-heavy design, with most of the work pitched at district and unit levels. We adopted a theoretically structured sample of authorities, going for contrasting districts with both officially 'high' and 'low' resource levels (above and below RAWP targets); with and without major long stay institutions and both exclusively urban and urban/rural mixed. These different characteristics represented structural influences which we hypothesised might influence the nature and difficulty of the management task.

In each sample district the core of our fieldwork consisted of a series of long, loosely-structured interviews (averaging more than an hour in duration) with a wide range of senior staff. These normally included the DGM, other district officers on the district management board (DMB), unit general managers (UGMs), a selection of consultants, the health authority chair plus a couple of members, Community Health Council and trade union representatives and senior officials in the local authority social services department. In each interview we tried to begin with very broad, open-ended questions, designed to bring out interviewees' conceptualisations of the Griffiths' changes. Later, if necessary, we raised some specific issues, for example, what did they think of the Individual Performance Review system? These interviews were supplemented by attendance at a number of meetings and by study of internal minutes, plans and memoranda. We wanted to get at what GM meant to key people at district level, what they attributed to it, and how their beliefs, hopes and fears compared with the expectations embodied in the Griffiths Report itself and subsequent ministerial statements. We also wanted, subsequently, to be able to interpret their perceptions in the light of our theoretical apparatus. This was, therefore, theory-driven ethnography.

POLITICAL DIFFICULTIES

Apart from the research design the two other prerequisites for the project were money and access. Here, immediately, the researcher of a 'hot' policy encounters the politics of contrasting agendas. In our case, there were at least five significant 'players' and each appeared to have a somewhat different agenda. The five were the Economic and Social Research Council (ESRC), the DHSS/SHHD, NHS managers, consultants and ourselves.

The ESRC had just announced a 'management in government' programme which they hoped would result in successful investigations of some of the many management changes introduced by the Conservative government since 1979. They were, presumably, looking

for researchers with academically respectable designs. But the ESRC was also under pressure to do more 'relevant' research and to disseminate it better, and it seems highly likely that the 'management in government' programme was one response to this. This part of their agenda, therefore, coincided with our own. At any event, after a considerable period of documentation and appraisal they awarded the grant on which the project depended.

Central government's agenda, however, was very different. At first, the Chief Scientist's organisation inside the DHSS seemed a promising source for alternative or additional funding, and officials made encouraging noises about our proposals. Then the NHS Management Board itself intervened, apparently insisting that no research into GM should be funded until a set of criteria had been drawn up. Reasonable enough on the surface, one might think, but now, three years later, we still have not received either information about criteria or even a definitive response to our original application. Even if this is simply a case of bureaucratic inertia, it remains a very effective means of discouraging researchers, and it is not hard to imagine why the Board might want to do just that. GM had received consistently enthusiastic backing of ministers, who had held out for the Griffiths model against quite stern (and continuing) demands for major modifications from the medical and nursing professions. The Griffiths report represented the kind of quick, no-nonsense businessman's solution that was close to the Cabinet's heart, not only in health care. Independent, published, broad-gauge research into this topic would be unlikely to raise the standing of GM any higher than ministers had already endeavoured to push it, but it might well expose blemishes which could then be seized upon by dissatisfied NHS groups and their Parliamentary allies. Our failure to secure funds, or even a proper reply from the DHSS, meant that our sample of health authorities has been smaller than we would have wished.

Our reception by the Scottish branch of central government was rather different. The research design secured the support of the Scottish Health Services Research Council (HSRC) and we were awarded funds. This happened despite strong opposition from senior officials at the SHHD. The latter even tried to suggest that the study of policy changes might be beyond the proper function of the HSRC. This episode seems to illustrate the significance of institutionally independent research-funding. There is no English counterpart to the HSRC. We have also been told, though we cannot verify this, that it was our personal track records which ensured a favourable response—the ethnographically-weighted research design did not evoke tremendous enthusiasm.

As for the NHS managers themselves, they had an obvious interest in emphasising the legitimacy, importance and potential of GM. This interest was sharpened—and the risks of ‘bad faith’ enhanced—by the introduction, as part of the GM package, of employment by short term contract. Even before we began our fieldwork we were aware of two strategies by which senior managers (and ministers) advanced the image of GM. First, there was considerable emphasis on the outward *forms* of GM: new appointments, new titles, new organisational structures. Second, there were attempts to attribute to GM almost anything ‘good’ that had happened since its introduction. The absence of any possible ‘control’ group makes this second strategy a particularly hard one to disentangle, and we will return to it later.

Yet managers’ resistance to independent research was less determined than central government’s. First, managers were only too well aware that GM was not sweeping all before it, and some of them welcomed an opportunity to talk over their problems with reasonably knowledgeable outsiders. (Many of our interviews turned into confessionals or complaint sessions.) Crucial in some cases (not all) was the guarantee we gave at the outset that individuals and health authorities would not be identified in any of our publications. It is also interesting that some of our respondents were only willing to talk freely after we had satisfied them that we were *not* agents of the DHSS. Second, some managers clearly hoped that they would pick up some useful ideas from us. They were often keen to know how things were done in other districts, how their methods compared. This was a problem because some of them had difficulty in accepting that our prime aim was not necessarily to produce practical recommendations for action. Third, there was a background cultural factor in that the NHS does not usually think of itself as a secretive organisation, but as one where the presumption is in favour of research and open comment. It is a matter for concern that current pressures may be beginning to change this. Managers have tried to bring pressure on some doctors not to speak out in public against government policy and attempted to add to new nurse managers’ contracts a clause making speaking to the media without permission a matter of disciplinary action (Harrison, 1988, Chapter 7; Laurance, 1988). Bad faith may be on the point of becoming institutionalised, which will handicap ethnographers more than other kinds of researcher.

The medical profession is different again—a law unto itself, as many commentators have observed. With respect to research into GM we found some substance in a taxonomy of clinicians enunciated by several NHS managers, namely that there are three broad groups of consultants, a

large majority who are not particularly interested in management, plus two minorities. One minority is opposed to GM and all its works: they reverse the strategy referred to above by ascribing to GM everything 'bad' that has happened since 1984. The other minority consists of those consultants who perceive an increasing need for efficient and effective management in the NHS and are conscious of their profession's limited experience and training in this regard. Getting to see the first (majority) group was not particularly easy (they had better things to do with their time). The second group was either intensely suspicious or very anxious to spread the bad news (much of which seemed to consist of fairly petty complaints concerning supplies or failures to observe etiquette). The third tended to grant ready access—and is probably overrepresented in our sample because of that.

It would be ingenuous to pretend that researchers themselves do not also have 'agendas' and that these do not extend beyond the pursuit of pure, scholarly truth. In our case we needed substantial external finance in order to pursue a subject that had long interested us. It was also a topic where we felt there was plenty of room for fresh contributions, and that, in turn, meant that it was one that would do our careers and publication records no harm. Furthermore, it was a topic which could conceivably enhance our attractiveness in the burgeoning field of management consultancy and training. All this, however, depended on getting access and money, which in turn depended on our ability to manoeuvre among the different agendas described above. Our impression was that we were fortunate to be focusing mainly on health authorities rather than central government itself—the districts were more permeable and, in parts, less sensitive about what an investigation such as ours might produce. Crucially, also, we had 192 DHAs to choose among. If one turned us down (as happened—'too busy') we could find another—with similar structural characteristics—to take its place.

VALIDITY

The issue of validity in ethnographic/interpretive research may now be reconsidered. How, in the midst of these competing agendas, could we strengthen our own confidence (and that of our readers) that our findings were valid? How, in the absence of good 'before and after' information, or control groups, could we rise above the 'inside dopesters's' status as a purveyor of elite gossip and anecdote?

The methodological literature does offer some general guidance. Recommendations we found particularly useful and tried (with varying degrees of success) to implement included the following:

—That the fieldwork should not be *completely* open-ended (ethnography by hanging about) but should be substantially guided by explicit theories and hypotheses (Illsley, 1980, pp.134–135; Bulmer, 1982, pp.162–167; Kirk and Miller, 1986, pp.49–51; Chen, 1988, p.10).

—That wherever possible important evidence should be checked through a variety of sources (for example, if two consultants assert that the District Management Board no longer takes any important decisions because they are now taken out of committee by the DGM, is that supported by study of the Board minutes compared with those of the preceding District Management Team?). Young and Mills (1980) refer to this as ‘triangulation’.

—That the loosely-structured interviews follow a sequence which allows respondents plenty of room to develop their own perspectives and agendas at the beginning, even if the researcher later needs to inject a minimum number of ‘standard’ questions. This reduces the chances that the researchers will impose their own prior pattern on the evidence.

—That the project as a whole observes four separate stages—research design, data collection, interpretation (data analysis) and explanation (as recommended in Kirk and Miller, 1986).

—Further, that the procedures used during the data collection phase should be fairly fully documented. ‘This must be accomplished at such a level of abstraction that the loci of decisions internal to the research project are made apparent’ (Kirk and Miller, 1986, p.72).

—That, where deliberate bad faith is suspected of a respondent, ‘phased assertion’ techniques may be used to try to penetrate his or her reticence (Kirk and Miller, 1986, p.48). Like many other interviewers we have sometimes been awarded additional information simply because we have appeared to know more than we actually did. If elite actors think the researcher has been taken into the rival’s confidence, they will be anxious to assert their own interpretation of events which might otherwise have remained undisclosed.

In addition to these general precautions, we tried to alleviate ‘before and after’ problems by the study of documentary records from the period before GM and also by careful questioning of those of our respondents who had had extensive pre-GM experience. The inclusion of two Scottish health boards in our sample was also of special value in that the GM management reforms were introduced later in Scotland, and against significantly different institutional and political backgrounds. In a very limited sort of way, therefore, Scotland could be looked upon as almost a ‘control’, at least for certain variables. We believe the potential for intra UK comparisons in policy research is still under-recognised (see also Hunter and Wistow, 1987).

Second, we made arrangements to test our description and interpretations of interviewees' perceptions by feeding back summaries to a sample of our respondents after the fieldwork phase was complete. Bloor argues that sociological descriptions are both better validated and more of an emancipatory resource 'where a definite attempt is made to make the sociologist's account available for the scrutiny of those same groups and individuals' (McKeganey and Bloor, 1981, p.59). He goes on to support the position taken earlier in this paper, viz: 'validity is not simply an issue internal to social scientific thinking but is something which obtains by virtue of a correspondence between scientific and commonsense accounts of reality' (p.60; see also Bloor, 1979). In our case there were two main 'results' from our feedback sessions. First, health authorities overwhelmingly accepted our basic descriptions of how things were (or had been, at the time of the fieldwork). Second, however, we again encountered some bewilderment at the *form* of our research. Why were we dealing in 'perceptions' rather than 'facts'? Above all, why weren't we making precise recommendations for action? In effect, some managers and doctors had discounted our earlier explanations that we were an academic research project. They wanted a managerialist treatment.

COUNTERFACTUALS PANEL

One of our validity-seeking devices is unusual, and perhaps deserves separate mention. The notion of 'counterfactuals' as an integral part of historical and social science explanation has received attention from philosophers (for example, Elster, 1978, pp.175–221) but, as far as we are aware, little application in policy research. Given the special difficulties the GM investigation had establishing anything approaching a 'control group' it seemed that a small experiment in counterfactual construction could do no harm and might yield additional insights.

Basically a counterfactual statement is an answer to the question 'what would have happened if ...?' In our case, the question we were interested in was 'what would have happened to the running of the NHS if GM had *not* been introduced?' Some clarification of this alternative scenario was necessary if we were to know which of the contemporary developments in the NHS could properly be attributed to GM, and which had arisen for other reasons. However, not any old counterfactual statements are acceptable. 'The crucial point is that the counterfactual statements must be explained in terms of some *actual* theory ... that the speaker, if challenged, could produce to back his assertion' (Elster, 1978, p.182).

Our own procedure was as follows: first, we assembled a small group

of advisers, chosen on the basis that each possessed a mixture of academic and NHS practitioner experience. This panel was presented with a summary literature survey of such research findings as there were concerning NHS management behaviour before the Griffiths Report. It was asked to modify and extend this summary picture in the light of their own experiences. Next, the panel was asked to project this pre-Griffiths situation into the post-Griffiths world—as if the Griffiths Report had not occurred but all other variables had. The panel's projection was then compared with a (previously unseen) account of the empirical findings of our research. Finally, the plausibility of attributing differences between the panel's projection and our findings to GM was subject to critical discussion between the panel and the research team.

It is not our view that this experiment would, by itself, confer validity on research which was otherwise methodologically sloppy. Furthermore, if we were doing it again we might well want to modify slightly both the timing of the counterfactual exercise within the project sequence (it took place just over halfway through a two-year project) and the details of its administration. Nevertheless, in the circumstances in which much current policy research is obliged to take place it is suggested that counterfactuals exercises may serve as, at the very least, a useful additional check on both realism and coherence.

NO HIDING PLACE: SOME CONCLUDING REFLECTIONS

Broad questions concerning the implementation of contemporary policies can seldom be addressed by experimental or quasi-experimental methods. Economic approaches will often prove useful, but even their most passionate advocates would hesitate to claim that they could capture the full range of implementation effects, perhaps especially in the field of social policy. Ethnographic approaches represent an alternative and complementary model. Paradoxically, one field in which ethnographies (though they are not usually called that) seem quite fashionable is that of business studies. The wave of 'corporate culture' studies, though they may not always be well done, can be seen as one indicator of the potential of ethnography, even in a field which usually prides itself on its gritty pragmatism (see, for example, Pettigrew, 1985). Ethnography has a long and respectable history, spanning several disciplines. Consideration of this experience indicates a characteristic pattern of strengths and weaknesses, including strengths which complement known limitations or ethical problems associated with the experimental and economic models. Certainly, ethnographic approaches *can* be executed in ways which cast doubt on their validity (as can the other approaches) but some

general guidelines are now beginning to emerge from the literature which should help the policy researcher find an appropriate mixture of global direction with local open-endedness. The rich explorations which are only possible in loosely-structured, face-to-face encounters can be set within a carefully conceived and phased overall design. Such designs can incorporate an explicit, prior theoretical orientation, a representative choice of site, and considerable 'triangulation' and respondent validation.

While ethnographic research is never easy to do well, it seems particularly threatened by current tendencies towards an axiomatic and sometimes authoritarian style of policymaking. Party political polarisation increases resistance to those models of the research task which stress uncertainties, ambiguities and the need for careful interpretation of a diversity of views. Some on the right find it fashionable and convenient to dismiss the more critical of these views as no more than the semi-automatic whingeings of sectional interests (an echo, on the radical right, of the mechanistic excesses of functional Marxism). Unfortunately (or perhaps fortunately) ethnographers have nowhere else to go. Their main claims to distinctiveness and validity lie precisely in a careful mapping of this variety of perspectives and values.

To realise their distinctiveness, policy ethnographers need access and money. Money seems to depend on the continued existence of research funding bodies which are not directly controlled by the policymaking divisions of central departments. Access depends on a mixture of factors, including the track records and negotiating skills of the researchers. Both access and money are influenced by the general status of ethnographic/interpretive approaches in the academic and practitioner communities. In the academic community the maintenance of anachronistic and artificial barriers between disciplines has helped to create a situation in which policy researchers working within different theoretical traditions are seldom encouraged to pool skills and perspectives. Ethnographers struggle on, dispersed in penny numbers through departments of anthropology, sociology, social policy and political science. The unkindest blows they sustain are perhaps the occasional dismissive remarks received from narrow specialists in more 'hard-edged', quantitative approaches. (That ethnographers should sometimes strike back is understandable, if equally counterproductive for the academic community as a whole.)

The practitioner community is, as we have seen, quite various. 'Bad faith' is a general problem, still deepening, which affects research of all kinds. Ethnography, however, is a particular sufferer from other, parallel

trends. The managerialist model currently dominates so much practitioner thinking, emphasising data-driven prescriptiveness and devaluing interpretive approaches which allow a voice to 'alternative realities'. Against this, the fashionableness of the concept of organisational 'culture' may grant ethnography some new possibilities. Yet all too often practitioners want culture, too, to be treated in a narrow prescriptive manner. Misguidedly, they see culture as something which top management can and should manipulate in the service of its own immediate objectives (Lynn Meek, 1988).

Policy ethnography is therefore in great danger of falling between the two communities. Because it is to do with 'policy' it is perhaps regarded as too 'applied' for those scholars who guard the dwindling funds available for purely 'academic' research, and may also be marked down on grounds of failure to ape the (inappropriate) natural science model. Yet practitioners will also be reluctant to fund ethnographic research, in their case because it will not produce quick prescriptions and may seem likely to generate political embarrassment by revealing conflict and competing perspectives. Policy ethnography does not deserve these handicaps: the small amount of it which is done already shows that it has a distinctive contribution to make to our understanding of the policy process.

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