

# Chapter 1

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## Getting at the oyster

### One of many lessons from the Social Support and Pregnancy Outcome Study

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In Roberts, H. (1992) *Women's health matters*, London: Routledge.

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Textbooks of sociological methods commonly assume that research is a systematic, linear process. In this textbook model, research projects begin with a list of hypotheses to be tested and end with neatly data-based conclusions. Epistemological critiques of sociology over the past three decades have posed a certain challenge to the simplicity of the textbook model. But awareness of the need for 'a sociology of the research process' (Platt 1976) imposes on researchers the further requirement of building up a picture of how research is actually done. This 'housework' of the research process includes the genesis of research ideas in the life experiences of researchers, the pursuit of research funding, theoretical, methodological, practical and ethical problems encountered in carrying out the research, and a discussion of choices, techniques, ethics and consequences of research dissemination.

Like housework, social research in many countries is embedded in a culture of impermanence. Contract research as a form of labour thus tends to be sharply sensitive to the practical exigencies of life (see Bernstein 1984). This structural context within which research is done forms an important backdrop to the issues discussed in this chapter. Its purpose is to consider one of the 'housework of research' issues – that of research funding – in relation to a research project concerning the provision of social support for childbearing women. The point of the story is not to complain about unfair treatment from a funding body, but instead to document the process by which a research project moves along the path from idea to 'reality', and to expose the difficulties encountered by research which does not easily fit into conventional models of what research 'is'. The study has been, and is being, written up and published elsewhere (see Oakley 1989a; Oakley 1989b; Oakley forthcoming; Oakley, Rajan

and Grant 1990; Oakley, Rajan and Robertson 1990); the story of funding difficulties told here ultimately had a happy ending, albeit one which entailed a series of moral and other lessons of its own.

Although the research project concerns reproductive health, the relevance to women as subjects of study of the wider epistemological and ideological issues raised in this chapter may appear more tenuous. As Dorothy Smith (1987) has argued, there is no 'knowledge' of any kind that is not mediated by the experience of everyday life. Not only are such experiences themselves gender-differentiated, but the *relationships* of men and women to these experiences are also differently shaped by cultural factors (Miller 1976; Chodorow 1978). In the context of social research, it could be argued that women's role as houseworkers, both in general and within the discipline of sociology, results in the insight of considerable disjunctions between the actual experience of research and the model of how research is supposed to be experienced. A sensitivity to the practical and other nuances of everyday life is likely to heighten one's awareness of the 'ivory tower' nature of ideal-type paradigms. The different socializations and positions of male and female in the doing of research interact with one another to produce a view, not only of the gendered nature of society, but of social science. I return to this point later.

### Origins

The Social Support and Pregnancy Outcome (SSPO) Study had its origins in six sets of observations, with somewhat different ontological statuses, about social relations. These were that:

- 1 Science, including medical science, may be regarded as a 'social' product – its content and practice reflect the social backgrounds and motives of its practitioners, rather than existing in some pure, uncontaminated ahistorical mode.
- 2 The professional ideologies, status and organization of the medical profession militate against recognition of the universe and impact of the 'social' in health care.
- 3 The survival and health of mothers and babies is consistently worse in socially disadvantaged than in socially advantaged groups.
- 4 Differences in social position and experience, especially as mediated by stress, are linked with different fates of mothers

and babies.

- 5 Social support is good for health.
- 6 Being researched may in this sense be health-promoting (though it is also the case that being researched may not be experienced as supportive).

These sets of observations derived from my experience of research over a long period of time. Two projects were particularly important. The first was one on women's transition to motherhood (Oakley 1979; Oakley 1980), which led to insights about both the stress-producing character of much modern antenatal 'care' and the extent to which research interviews might be construed as supportive experiences for those 'being researched'. The second area of work concerned perinatal medical issues, specifically a project on the development of antenatal care as a screening programme, in which I learnt how much of modern reproductive care must be called 'unscientific' in the sense that it has not been systematically evaluated and shown to be effective, appropriate and safe (see Oakley 1984; Chalmers *et al.* 1989).

The wider cultural context in which the SSPO project was conceived was also important. Government reports and pressure groups were, at the time, vociferously making some strangely simplistic claims about the state of Britain's perinatal health services. Britain's record of baby deaths was described as a 'holocaust' (Court Report 1976), and 'guesstimates' cited 10,000 British babies as dying or being handicapped every year as result of shortfalls in the maternity services (Social Services Committee 1980). Many of the recommendations put forward in the Social Services Committee's Report on Perinatal and Neonatal Mortality and taken up by the media and by pressure groups to correct this state of affairs, pushed for more, and especially more centralized high technology medical care; this was despite lack of evidence that these would be appropriate or effective solutions, and on the shaky assumption that medical care could compensate for or override the health-damaging effects of material disadvantage.

'Social class', 'perinatal mortality' and 'low birth-weight' were key terms in this debate. Britain's record in caring for mothers and babies was widely compared with, and seen to be deficient in relation to, those of other countries, in terms of the narrow indicator of perinatal mortality rates. Behind perinatal mortality rates lurked the importantly culture- and class-differentiated factor

of low birth-weight (LBW) babies – babies born too small (weighing less than 2,500 gm) to have a normal chance of survival. Indeed, the condition of low birth-weight babies symbolized much of the debate about the present, future and meaning of the perinatal health services in the 1970s and 1980s: arbitrarily divided from their ‘normal’ peers by the finer points of hospital scales and official statisticians’ calculations, LBW babies appeared ideologically both as exemplar and proof of biology’s operation in determining the social (obstetrical) product, and of medicine’s parallel rhetoric in claiming – in the pursuit of the ‘perfect’ baby – to repair or mask all known biological flaws.

The central idea of the SSPO study was to provide and evaluate, by means of a randomized controlled trial, the effectiveness of a social support intervention for women at risk of having a LBW baby. This goal, together with the study’s origins, dictated an uneasy epistemological position for the research: one between the two worlds delineated in Table 1.1. The pairs of words shown in Table 1.1 describe a fundamental cultural theme: a dichotomous discourse which inhabits all corners of our culture, including academic work and research funding. The very title of the SSPO study appears to confirm the message of Table 1.1’s two models divide: social support *and* pregnancy outcome. The social, the qualitative, the hard-to-measure, is on the one side, the biological, the quantitative, the easy-to-measure on the other. The province of the study was both social and medical; though the intervention to be tried was social in character, the usual health service interventions in the lives of pregnant women are medical. Though an important reason for undertaking the study had to do with social class differences in perinatal health and illness, the denominator population was to be a group defined in terms of a medical indicator – that of birth-weight. The measure of the study’s success was to be a mix of social and medical outcomes, from women’s satisfaction to use of high technology neonatal care. The evaluation of the intervention depended on use of experimental quantitative methods, but was in itself qualitative.

### Designing the study

In my early notes on the study made in 1981, the research design consisted of taking about fifty women with a previous LBW baby and twenty women without, and interviewing each four or five times

Table 1.1 Cultural Dichotomies

Social	Medical
Subjective	Objective
Experience	Knowledge
Observation	Intervention
Qualitative	Quantitative
Practice	Theory
Emotion	Reason
Nature	Culture
Feminine	Masculine
‘Soft’	‘Hard’
Women	Men
Health	Disease
Care	Control
Private	Public
Community	Institute
Value	Fact

during pregnancy. The focus of the study was to be on stress and life events, but the social circumstances of ‘high’ and ‘low’ risk mothers would also be documented. A control group would be taken to provide a measure of the ‘Hawthorne effect’. However, my notes show that I quickly moved from this proposal to the design of a randomized controlled trial. Presumably one reason for this shift was conversion by my colleagues in the unit in which I was working – the National Perinatal Epidemiology Unit (NPEU) in Oxford, later famous for its advocacy of randomized controlled trials (RCTs) in perinatal medicine. Another was the intellectual realization that the elegantly simple method of the RCT, together with its logically necessary constituent of some sort of action or intervention ‘package’ (intended to change something in order better to arrive at an understanding of it) did, indeed, have an unexplored applicability to topics of sociological enquiry.

The second draft design for the study proposed to identify a sample of 200 obstetrically-at-risk women and randomly allocate them either to receive supportive social science interviewing (see Oakley 1981; Finch 1984) or to receive antenatal-care-as-usual. The aim of the interviewing, aside from the provision of social support, was the collection of detailed social data on areas such as nutrition, smoking, work and so forth. The two aims were combined in order to avoid the putting of every available egg into the same basket; in the event of the social support intervention not working, the data collected during it

could be used to explore some of the links between social variables during pregnancy and the fate of mother and child. Study 'outcomes' were, as before, to be a combination of the biological – for example birth-weight – and the social – for example mothers' satisfaction with their experiences.

I set about the task of obtaining funding for the SSPO study in late 1981, four months before my current research contract was due to end (it was in fact, and happily, extended for a further year). I first wrote to Raymond Illsley, Professor and Director of the Medical Research Council's Medical Sociology Unit in Aberdeen, long-time researcher on social aspects of reproduction, and Chair of the Social Affairs Committee of the Social Science Research Council (SSRC) asking him to comment on the study outline. A letter in reply said,

I agree that an approach of the kind you suggest would be valuable. Since our early in-depth studies of first pregnancies in the 1950s there has been no serious attempt to chart the events of pregnancy in a comprehensive fashion taking into account the various parameters of behaviour, nutritional, income and expenditure and psychological influences.

He went on to say that,

My major reservation applies to the size of the sample. However valuable your descriptive account of the experience of pregnancy, given your initial hypotheses and the design of the study, its potential value will be judged upon (1) its ability to demonstrate an intervention effect; (2) its ability to interpret the meaning of any intervention effect. I do not believe that this can be done using only 100 index and 100 control cases.

As to funding, Illsley observed that,

the study fits awkwardly (like many good ideas) between MRC and SSRC. My first resort would be DHSS, who are likely to be both informed and sympathetic.

Taking account of Illsley's comments, and in particular increasing the proposed sample size, I redrafted the outline, prefacing it with a fairly extensive review of some of the social factors and pregnancy literature. In January 1982 I sent it out to the eighteen members of the Board of Advisers to the NPEU. Twelve replied, many with detailed comments. In addition, I sent the proposal to a number of

other people in this country and abroad including paediatricians, epidemiologists and social scientists.

The comments received at this stage do, I think, throw a good deal of light on the context within which the research came eventually to be done, and which, in important ways, limits the kind of research it is possible to do in this field. Again, we come back to the limits of the cultural discourse represented in Table 1.1.

Two of the three social scientists I consulted felt positive about the study, though each raised important limitations of the proposal as it stood from a social science point of view: the fact that low birth-weight 'is many things', so that its use to define 'risk' mixes different groups, at least some of which will prove not to be 'at risk' at all; the need to understand the *processes* that link social factors and the fate of pregnancy; social variation in medical definitions and terminologies themselves; and possible undercosting of the study in terms of the amount of time the intervention would require. The third social scientist, well known for his work on the aetiology of psychiatric disorder, was sceptical about the notion of an *intervention* study, on the grounds that the precise role of psychosocial factors in pregnancy needed first to be established. A long letter from a representative of one of the maternity services user-organizations raised a different, but very valid, point about the content of the proposed social support, observing that, from the point of view of the study women, practical help with housework and child care was likely to figure prominently under this heading: would the intervention in fact cater for such practical needs?

The more medically-oriented comments ranged from the highly technical to the pessimistically practical. One over-committed medical statistician began his letter with,

Your draft proposal was put on one side until I had a nice quiet train journey. . . . I have read this with interest, though wonder whether you will be producing a somewhat shorter version before submission – I say this because many referees are extremely busy, not necessarily motivated to read lengthy material, and require it to be presented to them in a way that it is easy to absorb.

Viewing the proposed study within the context of complaints made by women about maternity care, an Australian epidemiologist underlined the importance of considering medical care itself a source of stress in pregnancy. She noted that,

I've come across several women in the past month who didn't come for any antenatal care in the present pregnancy – one because she did last time and still had a stillbirth; the others because they kept being admitted for suspected fetal growth retardation last time (causing enormous family problems each time) – and gave birth to infants of normal weight!

In a similarly realistic vein, from a British community physician, came the comment that:

It seems a most important proposal and I very much hope it can be funded. My experience of medical bodies makes me pessimistic, but perhaps the SSRC might be more hopeful.

### 'The problem with the research design'

As a result of these comments, the proposal was revised in a number of minor ways before being submitted to the SSRC at the end of April 1982 for a 1 May deadline; the decision would be taken in November. The final decision to send the proposal to the SSRC was taken following informal discussions with the DHSS, who provided core funding for the NPEU, and, after careful consideration, said it would support the proposal to the extent of committing itself to a half-share of the funding. However, the process of sending the proposal out to referees for comment prior to a funding decision was to be left to the SSRC.

The project was costed at over £200,000. The SSRC wrote requesting a detailed breakdown of how many journeys would be made and at what cost per journey. It also informed me that I had to make a strong case for claiming an electric typewriter, and 'as regards the other equipment' (five tape recorders and accessories for the four interviewers and myself to carry out the supportive home interviewing – of which a proportion was to be taped), 'perhaps you could explain how this would be used during the research?' I wrote explaining that the tape recorders were to be used for tape recording, and to provide details of the current scarcity of electric typewriters in the NPEU.

Having sorted out these matters, the SSRC wrote on 29 July saying that it had been decided to hold a site visit for the application 'as is usual with applications of this scale'. The object of this exercise was for SSRC representatives to discuss the proposed research

with the applicant, for the applicant to answer any points made anonymously by referees, and then to revise the application before its final consideration by the whole committee. I would be sent an abstract of referees' comments in good time before the site visit, which was eventually scheduled for 7 October. 'Site visit' did, however, turn out to be something of a misnomer, as we were all asked to go to the SSRC office in Temple Avenue, instead of the SSRC representatives coming to see us in Oxford where the research would be located.

A telephone call made in early September to pursue the promised abstract of referees' comments elicited the information that even if the study was funded, it could not start in March 1983 as planned, as there was no money available; 'nothing before June', I wrote despondently in my file, noting that this would leave me salary-less for three months. The abstract arrived three weeks before the site visit. The comments of the SSRC referees were arranged under headings: Research design; Definition of variables; Methodology. Under the first heading came the following remark:

The problem with the research design is that this particular combination of an essentially quantitative question about reproduction, 'what is the impact of extra hand holding during pregnancy on final birth-weight' and essentially theoretical concerns about the sociology of confinement 'in what ways does social class operate through pregnancy' leads to inappropriate research designs for both.

My notes show that at the site visit I planned to defend myself thus:

[It] seems to me that *both* questions ideally demand *larger* sample numbers than I have proposed . . . I don't see one objective – the intervention – as quantitative and the other – data-collection on social factors – as theoretical. Both appear to me to demand a *quantitative* approach and to raise important questions about the factors mediating between the environment on the one hand and health and illness on the other, and about the appropriateness (or otherwise) of current patterns of clinical care during pregnancy to this interaction.

The next point was one about sampling – that antenatal care varies between hospitals, and Oxford (which I was not proposing to use in any case) was untypical as it 'has half the national average of perinatal deaths'. To this I responded somewhat sharply by

reminding the SSRC that, since randomization would be carried out *within* each centre any differences in antenatal care routines *between* centres should not bias the results. Additionally,

[The] point about Oxford having a lower than average PMR and a lower incidence of LBW is true, but again this misses the crucial point which is that the recurrence rate of LBW is the same in Oxford as elsewhere. Since the incidence is lower one would expect a smaller number of cases over a specified period of time meeting the criterion for the trial (a previous LBW delivery) but, once included, one would not expect there to be anything untypical about these cases as opposed to those entered from other centres.

The next objection was that the sample would not be representative *because* it was a high risk group, and the special medical care the study women would receive would be likely to invalidate the results of the intervention study. I repeated the argument of the proposal that it was not intended to be a sample representative of all pregnant women, it was supposed to be a sample representative of all women with a history of LBW delivery, and that the reasons for choosing a 'high risk' sample were to maximize the chances of showing an effect of the proposed intervention:

The grounds for choosing . . . this group [are that it] contributes heavily to the group of babies with the greatest chance of dying in the perinatal period. There are no theoretical grounds for supposing that if the intervention works in this group it will not work in the pregnant population as a whole . . . I think it's worth remembering that even after having two LBW babies a woman has a 70 per cent chance of producing a normal weight baby . . . .

On the matter of exceptional attitudes and exceptional treatment . . . some hospitals would give extra care to women with this kind of obstetric history, while others may not; however this doesn't really matter from the viewpoint of the research design since random allocation should achieve an equal distribution between experimental and control groups of whatever type of care is practised in any particular centre.

The final comment under this heading was that factors contributing to birth-weight such as maternal height and length of gestation needed to be held equal in the two groups. Again, I reminded

the SSRC of the principles of a randomized controlled design; that unless one was very unlucky, the use of random numbers to decide which women were offered the intervention and which were not should secure the same distribution of the short and the tall (and the in-between) in the two groups.

Moving on to 'Sampling' I found myself again confronted with the objection that 'a random sample would not be able to yield details of causes and effects. One possibility might be a matched pairs design of 200 subjects to control for some of the unwanted variables'. I held my breath and refrained from pointing out that no sample ever in itself 'yielded causes and effects', and indeed it was arguable to what extent any social science research ought to be framed in these terms. Instead I reiterated the by now boring point that use of an RCT does away with the need to control for 'unwanted' variables – even supposing one has any way of knowing in advance what these might be.

When it came to the section of comments on 'Definition of variables' the referees appeared to be confused by my notion of 'socially supportive interviewing'. Their remarks indicated that they saw interviewing as interviewing and social support as something quite different. They were also concerned about the policy implications – if the intervention proved successful, how could or should antenatal services be reorganized? I replied to the effect that the model of interviewing as merely data collection was based on a fundamental *misunderstanding* of this aspect of research – on a refusal to see it as a social relationship. To the latter point I responded by commenting that this was essentially a trial of a non-clinical form of antenatal care in an era when most medical routines for pregnancy care were moving in the direction of *more* clinical care and more technology – despite the fact that these had not been shown to be effective, either in general, or in terms of caring for women with poor obstetric histories. This was a main reason for deciding in the end to use research midwives as the providers of social support in the study (under some pressure from the Department of Health which eventually provided funding, and which was also concerned about the policy implications of using social scientists to provide the support). Social care for childbearing women has traditionally been an important part of the midwife's job – and it is what many midwives find themselves increasingly unable to provide in the context of high technology hospital-based care. The first objective, therefore, was to see if the alternative approach of

non-clinical, *social* care worked – then to identify why it did, and what should be done about it. These aims could not necessarily all be achieved within the limits of one study.

The next confusion apparent in the comments was between social class and social support: first, that the social class gradient in perinatal outcome would disappear if ‘other concomitant factors’ were taken into account; second, how would the relative contributions of social class and social support to pregnancy outcome be disentangled? The first comment seemed to me indicative of the tendency to ‘reify’ social class that is so common in many social science debates. Social class tends to be taken as a ‘thing in itself’ which explains other things and is qualitatively different from them. (In this context it is possible that social support is a *component* of social class, rather than that the two need to be disentangled.) The second was mystifying, and I replied by saying we would collect descriptive data which would allow us to look at whether social class and patterns of social support were correlated with one another (see Oakley and Rajan 1991).

My responses to the abstract were conveyed round a large table at Temple Avenue to a company of SSRC representatives and delegates, and with the support of three colleagues from the Oxford Unit (Iain Chalmers, Adrian Grant, Alison Macfarlane) and of Margaret Stacey from Warwick University, representing sociology. I recall being very nervous. The outcome of the SSRC’s decision was crucial, both for me personally in terms of re-employment, and for a project that had become something of an obsession. I believed in it, and wanted to take it forward. Nothing that anyone had said to me about it had indicated that I was on the wrong track, though most people had (different) sets of reservations. The atmosphere was tense. My memory of the occasion is that within a short time of our arrival, and before our ‘external’, Meg Stacey, had come, the SSRC announced that it had decided to turn down the project in its present form. However, it was interested in funding me for a short time to work on an alternative proposal. I remember Meg being very angry when she arrived that they had caused her to come all the way from Warwick, having already made the decision. My notes on my responses to the abstract of referees’ comment refer to the conversation we had round the table subsequent to this announcement, a conversation which was all rather ‘academic’ as I was no longer defending a proposal that had any chance of being funded.

The ‘site visit’ was soon over. Iain, Adrian, Alison, Meg and I repaired to a pub for lunch. We were all in a state of shock, as this outcome had not been expected. Meg and I later went off together to Oxford Street, where I bought a garish red, green, blue and purple outfit in protest.

### Explaining to the man in the street

On 22 October I had a conversation with a member of the Social Affairs Committee Secretariat as to the nature of the committee’s notion of short-term funding for me. She suggested asking for eighteen months, and had done a preliminary costing which fitted the budget in under a £25,000 ceiling. Would I do a revised costing for the 12 November meeting of the committee? On 29 October I also discussed the situation with Raymond Illsley, who said that the central objection of the ‘site visiting’ party had been that I needed to specify what social support was first, before undertaking an intervention study to test its effectiveness. I wrote to the SSRC requesting a letter explaining the reasons that the original application had been turned down. There was no reply to this letter, so I wrote again the following January, receiving a reply from another new staff member (the third) saying that he had looked through the file and found ‘a synopsis of referees’ comments’ (the same as had been dispatched before the site visit) which he enclosed, hoping this ‘will be of some use’. By this time, the SSRC had agreed to fund me for eighteen months, from 1 July 1983 to 31 December 1984, ‘to define and operationalize the concept of “social support”, to study the literature, and to carry out the necessary pilot work’. I had been asked (by a fourth new member of the secretariat) to furnish it with yet another revised costing for the eighteen-month period, not exceeding £25,660.

We settled for £27,910, but my attempt to secure in writing the reasons for the original rejection continued. I replied to the fresh copy of the old comments by reminding the SSRC Secretariat that these had formed the basis for the site visit discussion. I went on to say that,

My understanding (and that of my colleagues) of the discussion that took place on that occasion was that the site visiting party appointed by the Committee agreed that a number of the referees’ comments had failed to appreciate the methodology and design of the proposed research. For example [here I listed by number the

comments in question] . . . are not relevant criticisms of a research design that is effectively one of a randomized controlled trial of social support . . . .

It is clearly important that this issue be clarified in the long run if it is my task to produce what is in the eyes of the committee an 'improved' research design. An additional problem is that . . . insofar as the main objective of the 'pilot' funding is to define the nature of social support, this objective is not likely successfully to be achieved within the time period of eighteen months – that is, it will not be possible to answer the question with an experimental design in such a short period of time.

It may well be that in such situations it is normal practice for applicants to be sent synopses of referees' comments only, rather than a documentation of the agreed opinion either of the committee or of the site visiting party. But I hope you will understand my dilemma here.

This correspondence closed with the reply that no further information would be forthcoming:

I can only repeat the problem that was initially mentioned to you, that the concept of 'social support' should be clarified and made operational.

On 18 May I received a circular letter from the SSRC concerning publicity for the pilot project. Would I send it a statement about the project for the next issue of the annual 'Research supported by the SSRC'? It would like 200 words, '*explaining it in simple terms*':

I emphasize this since it is important that the abstract should be understandable to social scientists in disciplines other than your own, and to the intelligent layman. (This last point has been emphasized by Lord Rothschild, who was asked to review our work recently. A copy of his relevant recommendation is attached for your information. On his authority therefore, I must urge you to suppress unnecessary jargon and neologisms in your abstract.)

The attached recommendation mentioned the most serious weakness of the SSRC as its failure to make known to the general public – 'the man in the street' – its own work and that of the social scientists it finances. 'The efforts of the SSRC in this respect are primitive and unprofessional.' Lord Rothschild took particular objection to the succulent bivalve syndrome ('succulent bivalve' = oyster), and

made a number of suggestions about how the SSRC's own language might be improved, including the purchase of four copies of Sir Ernest Gowers's *Plain Words* at a cost of £6.40.

My own attempt at conveying the message of the research to the man in the street went as follows:

#### *Social Factors and Pregnancy Outcome*

Social class differences in the birth-weight and survival of babies are a persisting feature of the health care scene in Britain. It is not clear why this is so, despite the fact that the phenomenon has been noted ever since national birth and death statistics began to be collected a century ago. Improved standards and techniques of medical care have not much affected the social class differences, and one reason is that forms of pregnancy care offered to date have not succeeded in lowering one major contributor to the differences – the proportion of low birth-weight babies born.

This study will examine the various explanations and evidence put forward as to why membership of different social groups should be associated with different chances of reproductive 'success'. In particular, it will look at the evidence as to the impact of social networks and supportive relationships (or lack of these) on the health of pregnant women and their babies. Studies describing various kinds of interventions (such as dietary advice and health education) carried out with the goal of improving the chances of successful pregnancy will be analysed. The aim is to design a project in which social support is provided to women at high risk of giving birth to low birth-weight babies, and the effect of this assessed by comparison with a similar group not receiving the social support.

#### **'A change in structure for changing circumstance'?**

Some of the lessons of all this are obvious. On a minor practical level, high staff turnover within an organization such as the SSRC/ESRC is an effective barrier to communication both internally and externally. It hardly needs to be said that the fiasco of 'site visits' should not be engaged in when a decision has already been taken not to go ahead with a project. In circumstances where informal decisions are made for shared funding between research councils and government departments, the refereeing process should not be unilateral. And so on. But the SSRC was having a difficult time of its own, and so



was social science, and so were the universities. The present tense would do almost as well for all of these statements. There is also an important continuity in the theme of the vulnerability of the contract researcher, who, whilst making a significant contribution to the intellectual and scientific culture of universities, lacks a career status and rewards commensurate with this. A related issue is the problem faced by academic teaching staff, who struggle, for their part, with the nonsense of research 'on the side'. Between 1976 and 1984 contract research employment in English universities increased by 76 per cent; in 1982, when the first proposals for the study described in this chapter were being written, contract researchers made up a quarter of the UK academic work force, and the majority of them were on contracts of less than three years' duration (Advisory Board for the Research Councils 1989).

Bell (1984) and others have told the story of what was happening to the SSRC around the time it was asked to make a decision about the SSPO proposal. It is clear from these accounts that the timing of the proposal could not have been worse. Successive cuts to the SSRC's budget had been announced, and successively smaller proportions of its expenditure had been channelled in the direction of sociological research. By 1976, 91 per cent of the SSRC's expenditure on new research programmes went to work on economic forecasting, organizational decision-making and management, educational management and performance and the analysis of public sector policy (Bell 1984: 20). In the summer of 1981 the University Grants Committee decreed a reduction in social science places in universities; the heyday of British sociology was over, with contraction substituted for the expansionary wave of the 1960s, when twenty-five new chairs in sociology were established in the space of seven years (University Grants Committee 1989). Also in the summer of 1981, the internal restructuring of the SSRC was announced, resulting in the abolition of the old Sociology and Social Administration Committee, and the reforming of the old committee structure into a smaller set of multi-disciplinary committees. At the end of the year, the external survey of the SSRC's structure and activities under the aegis of Lord Rothschild was initiated. The aim of both these moves was supposedly to increase the relevance of social science research to policy, and to discourage theoretical or fundamental research whose policy implications, especially in economic terms, might be either non-existent or unclear (SSRC 1981).

My own meeting with Raymond Illsley at the end of October to discuss the outcome of the 'site visit' took place two days before a candle-lit meeting of the Sociology and Social Administration Committee members at the National Liberal Club in London, to discuss and protest about the restructuring proposals. (The reason for the candles was not the avoidance of illumination, but the power workers' strike.) Lord Rothschild's report, which surprised many people by recommending salvage of the SSRC, though not unchanged, was published the month the money for the pilot study was granted. Despite the defences of Rothschild, which recommended that the SSRC's budget be maintained in real terms for three years, Keith Joseph cut £6 million from it in October 1982. The name change to the *Economic* and Social Research Council was agreed the following year, taking effect on the first working day of 1984. Douglas Hague, Chairman of Council from October 1983, insisted that it would have preferred to be known as the Social and Economic Research Council, but as this would have resulted in the same acronym as the Science and Engineering Research Council, the idea had to be dropped. Hague maintained that the change of name

does not mean the ESRC proposes to increase its support for research in economics at the expense of any other group of researchers. The fact that funds for research will be short in 1984/5 will clearly mean very keen competition for research funds, but a balanced research programme remains an important objective.

(ESRC Newsletter 51, March 1984: 3)

Such contextual dislocations explain some of the vagaries of treatment the SSPO research proposal received, and some of the internal readjustments may (as Bell contends) add up to manoeuvres which did succeed in ensuring survival of the SSRC through subsequent financial and political attacks. But what is more difficult to explain is why a project that was not discipline-bound but firmly problem-oriented was not deemed to be 'fundable' research. As the comments quoted earlier made clear, a significant problem was the study's province and design, straddling two models of research – the social-observational-qualitative on the one hand, and the medical-experimental-qualitative on the other. One important question raised by this is the extent to which bureaucratic and discipline-bound funding bodies are able to recognize innovatory research (see Ditton and Williams 1981).

The basic (and still unmet) challenge would seem to be one of designing such an organization so that it can successfully act as the bastion of defence for a broadly-based and non-discriminatory social science, without at the same time being blinded either by narrow-minded professional imperialism or by short-term political constraints to the need for imaginative fundamental research. The background for this is Britain's poor record of research investment: alone of the major OECD countries it did not increase its expenditure on Research and Development over the period 1981–6, and a major reason for lagging behind other countries is the greater share of the R. & D. budget in the UK devoted to defence spending (Ince 1986; Smith 1988; AUT 1989). The implications of this resource distribution may be far-reaching, and include some of the public health issues raised by the Social Support and Pregnancy Outcome project. It has, for example, been shown that there is a direct and inverse relationship between the proportion of countries' GNPs allocated to arms expenditure on the one hand, and infant mortality rates on the other (Woolhandler and Himmelstein 1985). What is bad for research may be bad for health, not because research is necessarily health-promoting (even for researchers), but because the same impetus that leads governments to formulate research policies and to invest in high quality research, is also likely to generate a commitment to practices which protect the nation's health.

The tendency for social research to act on and transform the social world at the same time as studying it, has always been regarded as one of the main ways in which the 'science' element in 'social science' cannot be regarded as equivalent to that in the natural sciences. Sociologists speak disparagingly of the 'Hawthorne effect' as the best known – and certainly the most frequently quoted – example of this. However, the Hawthorne effect is much more simply a demonstration of the central thesis of sociology: that people are social beings. The workers in the study were responding to the interest shown in them by the researchers. Such findings are witness to the falsity of 'scientism'; which, as Capra (1983) says, has consistently undervalued intuition, emotion, feeling, and direct individual experience as ways of knowing. The undervaluation of these ways of knowing in both the social and natural sciences has gone hand-in-hand with the biomedical model of human beings as physical bodies subject to malfunctioning. Disease is the breakdown of the machine, the doctor's task is repair by physical or chemical

means, and the theory underpinning this sees the body as cellular or molecular biology, not as inhabiting the same frame as a psyche, an identity, a social being intimately connected to the social and material world. In aping the natural sciences, sociology thus committed itself to a biologically determinist model of behaviour – and has spent much of its (relatively short) life trying to come to terms with or escape from the inevitable problems this poses.

As authors such as Hartsock (1983), Harding (1986) and Rose (1986) have argued, the masculine domination of science and of society both result in, and are preceded by, profoundly gender-differentiated life experiences. Masculinity itself is attained by resistance to the enclosing structures of everyday domestic life; in reaching for the world 'outside' this, men simultaneously conceive of abstract conceptual experience as preferable to the concrete and demeaning (Hartsock 1983); indeed they must 'have' such a concept first in order to locate themselves within it. The enterprise of science as an 'objective, value-neutral' activity is consequently 'the pre-eminent patriarchal enterprise'. Its theories and data 'tend to legitimate the ideology and power relations of patriarchy' by insisting both that nature exists to save 'mankind' and that scientific inquiry can yield abstract and absolute truths about nature. Such premises make science the 'instrument for "man's" domination of the world' (Harding 1986).

As Harding (1986) has pointed out, a further critical characterization of the scientific enterprise in modern society that follows from the above premises is that it is 'sacred':

We are told that human understanding is decreased rather than increased by attempting to account for the nature and situation of scientific activity in the ways science recommends accounting for all other social activity. This belief makes science sacred. Perhaps it even removes scientists from the realm of the completely human . . .

It is thus

taboo to suggest that natural science . . . is . . . a historically varying set of social practices, that a *thoroughgoing* and *scientific* appreciation of sciences requires descriptions and explanations of the regularities and underlying causal tendencies of science's own social practices and beliefs.

If women are largely alienated from science thus defined, then this helps to explain the fondness of women social scientists for methods of studying the social world that are distant from the quantitative, mechanistic and manipulative model of how the natural sciences operate. Even this is not straightforward, however: the geneticist Barbara McClintock's account of how in her work on the cytogenetics of maize she came to see the Neurospora chromosomes provides an alternative version of scientific activity as it 'really' is, which is at odds with the way it is said to be (Keller 1983). Specifically, what is startling about McClintock's account is the importance of the capacity of union between the knower and what is to be known in facilitating the scientist's understanding of the inherent lawfulness of nature. The feeling of union, it is to be noted, is quite compatible with the viewpoint that nature is lawful. It is a matter of the productivity in terms of law-discovery of empathy as distinct from opposition. The union of social and natural is, in short, as integral to natural, as it is to social, science. All of which suggests that what is required as an end-point as well as a method is an 'integrated understanding of the relationship between the biological and the social' (Rose *et al.* 1984: 10).

The problem, however, is how to arrive at this point without employing the dualistic language of Table 1.1. The account provided in this chapter is only a partial attempt to arrive at the end-point in a way that seeks above all to be conscious of the epistemological routes adopted and fixes thus uncovered.

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