1 Sociology and the aetiology of depression

It is common for social investigators to justify their work in terms of the extent and importance of the problem they have tackled. Although their assertions are for the most part politely accepted, in time, as claims accumulate scepticism is inevitable. We are therefore diffident about beginning by asserting the particular significance – in scope and severity – of the condition we have chosen to study, clinical depression. But since we do believe that it is common – at least in urban centres – and that it is peculiarly unpleasant we feel obliged to confront the understandable scepticism that now tends to be evoked by yet another indictment of our way of life. There is good reason to believe that depression is not just another problem but a central link between many kinds of problem – those that may lead to depression and that may follow from it. It is not only, for instance, closely linked to poor housing; it is also highly correlated with a whole range of serious physical disorders. Our claim for the significance of the condition is, therefore, based on its pivotal position in the explanation of what is wrong with our society. For, as we will argue later, while we see sadness, unhappiness, and grief as inevitable in all societies we do not believe that this is true of clinical depression.

It was in order to establish clearly that depression did hold this pivotal position that we chose to study it. There is a long history of sociological concern with the aetiological role of psychosocial factors in medical conditions. C. Wright Mills in *The Sociological Imagination* argued that the relation of 'personal troubles' to 'public issues of social structure' was *the* central feature of all classical work in social science. Sociology has been concerned not only with the workings of social systems as a whole, but with the impact they have on individuals caught up in them. One thing sociology seeks from its collaboration

with medicine is new ways of looking at such effects – how far is psychiatric or physical disorder the result of living in a particular form of domestic, economic, and political society? This interest can, of course, be seen in Durkheim's *Suicide* published in 1897 and in Marx's concern not only with an understanding of capitalist society but with its influence on the health and well-being of the individual.

In seeking to relate 'personal troubles' to 'public issues of social structure' there has been a difference of emphasis. There is a long and highly complex series of links between economic, cultural, and political systems and their eventual impact on particular individuals. We have concentrated on demonstrating that there *is* a link between clinical depression and a woman's daily experiences, in the belief that once this is done we will be in a stronger position to sort out the intricate links with wider structures. Other writers have shown, understandably, a greater initial concern with these wider links – say the relation between social mobility, ethnic background, and disorder (Dohrenwend and Dohrenwend, 1969). But it is a matter of emphasis – our work, we believe, already indicates something about these wider ramifications.

Given that we start with a woman's immediate experiences, where should a sociologist look for social 'facts' capable of influencing psychiatric and physical disorder? Cigarette smoking is without doubt linked to lung cancer, yet there is an important social component that increases a person's chance of smoking – that is of contacting the aetiological agent. A schoolchild, for example, may take to cigarette smoking not because it is enjoyed but because of the value it is given by his friends. One way to proceed, therefore, is to study the process by which such factors influence chances of contact with a pathological agent. The model of disease underlying this approach suggests that social factors influence alcohol consumption, cigarette smoking, and sexual behaviour but thereafter play no role in the aetiology of cirrhosis, cancer, or venereal disease – these are brought about by essentially mechanical means. A person's *awareness* of the environment plays no causal part once contact between agent and host is made. Enthusiastic gardening may reduce a man's risk of coronary disease; but it will do this whatever his feelings – whether he loves, hates, or is indifferent to the activity (Morris *et al.*, 1973). It is only important that he should do it.

This is not the only way to proceed. Consider a study by Meyer and Haggerty (1962) of sore throats in the children of fifteen middle-class families. Throat cultures were made every two or three weeks over the course of a year and at times of any obvious throat infection. By means of regular three-weekly interviews and diaries kept by the mother,

events that disrupted family or personal life were recorded. Three measures of 'disorder' were then used: illness associated with streptococci, a new streptococcal culture *without* overt illness, as well as non-streptococcal respiratory infections. All three were about four times more likely to be preceded by than followed by a distressing event, such as witnessing a bad road accident or a father losing his job. They were also more likely in the context of 'chronic family stress'. Since events were recorded independently of any knowledge of the acquisition of streptococci *without* illness, the result is unlikely to be due to bias and there is thus a case to be made that emotional and physical factors combined in some way to produce illness. It is not just that social factors in some way brought the child in contact with streptococci, but that they lowered his resistance to them once contact was made.

We are not in this study concerned with the way social factors lead to increased risk of contact with a physical agent but with the possibility of a more direct involvement of the social environment in disorder. With such an approach it is not even essential for there to be a physical agent such as streptococci. Physical and psychiatric disorder may be produced by cognitive and emotional response alone (although there must, of course, always be a bodily basis to the disorder itself). Sociologists, we believe, have most to contribute in this second approach to aetiology: they can investigate whether something such as enforced rehousing can raise the chance of developing psychiatric disorder, irrespective of the physical changes involved. They will be concerned not just with the fact that as the result of rehousing a housewife walks further to the shops or sees fewer friends *but with how she perceives and reacts emotionally to these changes.*

Sociologists are not alone in taking this perspective and no one has found it easy to translate into effective research. It may therefore be useful to begin by discussing aetiological research in psychiatry in general terms. At the risk of drawing too sharp a distinction we will refer to the intensive or clinical, and the survey or epidemiological approaches. Since we will argue that the two should be brought much more closely together, we begin by emphasizing their differences. The clinical or intensive approach concentrates on the individual. Detailed knowledge of the person and his disorder allows the investigator to make sense of the meaning that the symptoms have for him. This is its strength. The snag is that this very detail makes it difficult to move beyond the individual case. The statistical survey has the reverse problem. Comparability tends to be maintained at the expense of ignoring much of the complexity of the individual. Sociology has traditionally been concerned both with the outside world (for example,

with rates of migration) and with inner experiences (what the migrant makes of his new country). It has, therefore, straddled somewhat uneasily both intensive and survey approaches, although throughout its history there have been strong pressures from within the discipline for it to move one way or the other.

The clinical approach

Freud serves as an example of the strengths of an intensive approach. He was concerned with biographical detail. Consider the way he dealt with the question of aetiology in his *Introductory Lectures on Psychoanalysis*. (He is discussing a woman who had developed a delusion of jealousy about her husband after receiving an anonymous letter.)

> 'There are delusions of the most varied content: why in our case is the content of the delusion jealousy in particular? In what kind of people do delusions, and especially delusions of jealousy, come about? We should like to hear what the psychiatrist has to say about this; but at this point he leaves us in the lurch. He enters into only a single one of our inquiries. He will investigate the woman's family history and will *perhaps* give us this reply: "Delusions come about in people in whose families similar and other psychical disorders have repeatedly occurred." In other words, if this woman developed a delusion she was predisposed to it by hereditary transmission. No doubt that is something but is it all we want to know? Was this the only thing that contributed to the causation of the illness? Must we be content to suppose that it is a matter of indifference or caprice or is inexplicable where a delusion of jealousy arises rather than any other sort? And ought we to understand the assertion of the predominance of the hereditary influence in a negative sense as well – that no matter what experiences this woman's mind encountered she was destined some time or other to produce a delusion? . . . But can psychoanalysis do more here? Yes, it actually can. I hope to be able to show you that, even in a case so hard of access as this, it can discover something which makes a first understanding possible. And to begin with I would draw your attention to the inconspicuous detail that the patient herself positively provoked the anonymous letter, which now gave support to her delusion, by informing the scheming housemaid on the previous day that it would cause her the greatest unhappiness if her husband had a love affair with a young girl. In this way she first put the notion of sending the anonymous letter into the housemaid's head. Thus the delusion acquires a certain independence of the letter; it had been present already in the patient as a fear – or was it as a wish? Let us now add to this the small further indications yielded by only two analytical sessions . . . She herself was intensely in love with a young man, with the same son-in-law who had persuaded her to come to me as a patient. She herself knew nothing, or perhaps only a very little, of this love; in the family relationship that existed between them it was easy for this passionate liking to disguise itself as innocent affection. After all our experiences elsewhere, it is not hard for us to feel our way into the mental life of this upright wife and worthy mother, of the age of fifty-three. Being in love like this, a monstrous and impossible thing, could not become conscious; but it remained in existence and, even though it was unconscious, it exercised a severe pressure. Something had to become of it, some relief had to be looked for; and the easiest mitigation was offered, no doubt, by the mechanism of displacement which plays a part so regularly in the generating of delusional jealousy. If not only were she, the old woman, in love with a young man, but if also her old husband were having a love affair with a young girl, then her conscience would be relieved of the weight of her unfaithfulness. The phantasy of her husband's unfaithfulness thus acted as a cooling compress on her burning wound.'
>
> (Freud, 1971: 251–53)

Although this is only one case, a 'first understanding' is possible and the interpretation of the delusion carries a good deal of conviction. And yet the questions he raises have never been systematically answered. We do not know what kinds of people have such delusions; nor do we know whether delusions arise in a particular person at some time irrespective of their experience. Nor, indeed, can we have any confidence about the causal link he asserts between conflict and delusion. To answer these questions we would have to know how often married women in general fall in love with close relatives (or were involved in some comparable 'impossible and monstrous thing') and how often delusions followed. These frequencies could show whether women with delusions of jealousy were more likely to have experienced such a conflict than those without them. *This cannot be done without studying a good number of instances of what is to be explained*. And with this we are brought to the central problem of aetiological research. If we are to deal with more than one instance, it is necessary to settle what is to count as an 'impossible and monstrous thing'. If this is not done before we begin to *test* our ideas, it is easy to fit fact to theory or at least to be seen to do so: to see 'monstrous things' when we know there is neurosis and not to see them when there is no neurosis. If psy-

choanalysis had been able to make this one step, it would have moved itself some way away from accusations of pseudo-science. But the step is not easy and has not been taken. That it has not is understandable; it would be too easy to lose the very thing that is important about an intensive approach, a sense of the uniqueness of experience for the individual. But, for whatever reason, psychoanalysis has never seriously put its mind to the task. The same could be said for much else in psychiatry; but we will stay with psychoanalysis as it illustrates all the problems of bringing together intensive and survey approaches. Freud was continually faced with the impact of the environment. Indeed, early in his work he viewed seduction and assault in childhood to be a crucial aetiological factor in sexual neurosis. A major change in his thinking occurred when he came to see the majority of these events as fantasies. At this stage his interest appears to have turned almost exclusively to matters of internal workings of the mind; he no longer appeared to be much concerned with actual experience:

> 'I believe these *primal phantasies*, as I should like to call them, and no doubt a few others as well, are a phylogenetic endowment. In them the individual reaches beyond his own experience into primaeval experience at points where his own experience has been too rudimentary. It seems to me quite possible that all the things that are told to us today in analysis as phantasy – the seduction of children, the inflaming of sexual excitement by observing parental intercourse, the threat of castration (or rather castration itself) – were once real occurrences in the primaeval times of the human family, and that children in their phantasies are simply filling in the gaps in individual truth with prehistoric truth.'
>
> (Freud, 1974: 418)

The point has often been made that the ability of the approach to explain any set of findings in terms of such general formulations gives psychoanalysis the hallmark of pseudo-science – nothing is seriously tested (e.g. Cioffi, 1970). While we believe this to be essentially correct, such criticism tends to place too little weight on the importance of the insights obtained by Freud and his followers – and the possibility, when viewed historically, that their achievements would have been impossible without some way of lightening the onerous duty of continually seeking the refutation of ideas. However, be this as it may, psychoanalysis has for the most part followed Freud's style of work and failed to subject aetiological ideas to serious test. It has continued to hold a highly ambivalent attitude to the real world – both in terms of *what* actually happened and its *timing*. There are certainly great difficulties in translating occurrences in the world into effects on the mind, but to remain largely concerned with internal mental processes must mean giving up a precious chance of subjecting aetiological ideas to empirical test.

A psychoanalyst is likely to object that it *is* the internal workings of the mind that are important, though perhaps set off by external events. Even if this is so, it is possible that internal processes can be approximately parallelled by happenings in the world. We use the term 'approximate' in the sense that generalizations may be possible when the data for a number of individuals is viewed statistically; and this may be quite enough to provide a causal framework or model to which clinical interpretations can be linked. While much could be said in defence of psychoanalysis and linked clinical work (e.g. Cosin, Freeman and Freeman, 1971), there seems no escape from the bald conclusion that case studies cannot provide a means of selecting between alternative ideas about aetiology, and the orthodoxies that have grown up about such work have veiled this issue with dogmatism. At some stage theories have to be subjected to test. If ideas about aetiology are to be tested we must have some way of moving from such therapy-based interpretations of the individual. To be more specific: we must deal with the meaningfulness of experience for the individual without leaving ourselves open to the accusation that we have simply imposed our interpretations *post hoc*.

The survey or epidemiological approach

Epidemiological surveys usually consider the distribution of a disorder within a definite population and relate it to factors such as age, sex, social class, and place of residence. Durkheim, for instance, related rates of suicide to institutional and general features of societies as a whole – marriage, family life, widowhood, and religion. He sought in them signs of social malaise arising from industrialization and the decay of traditional society. His well-known concepts of 'egoism' and 'anomie' fitted into his general theme of the weakening of social bonds. Durkheim's *Suicide* remains a superb example, despite shortcomings, of the power of the survey method to move from correlations (that depression is more common among women who are widowed) to statements about causality (that widowhood can bring about depression). Unfortunately this does not mean that the survey has no deficiencies. Curiously, one of its greatest failures is where, at first acquaintance, it might appear to be strongest: in measurement – the placing of like with like. In discussing Freud's patient with delusions of jealousy we indicated that a central problem was to be able to settle, in a systematic way, which happenings were, for instance, the 'impossible

and monstrous things' capable of provoking such conflict. Nothing need be said of the likely shortcomings of official statistics (say of suicide) so often used in survey research. But as an alternative source of material, research workers have almost entirely relied on administrating to large numbers of people some form of the standardized questionnaire, with its dispiriting pretensions to measure almost anything by means of a few, often fixed-choice, questions:

> 'Continuing refinements of the structured questionnaire with efforts to eliminate interviewer bias can lead us up a blind alley – when carried to an extreme we will conclude that the best interviewer is a tape recorder containing the questions and the best recorder of replies a second tape recorder. A questionnaire addressed to the subject of the research which becomes very highly refined and standardised in its administration so as to allay our anxieties about interviewer bias may very well throw away the most valuable characteristic of the interviewer, his humanity.'
>
> (Gruenberg, 1963: 590–91)

Although used in almost all large-scale sociological enquiries, there must be the gravest doubt about the ability of such questionnaires to collect accurate and unbiased accounts of anything complex or of emotional depth. They are of little use in tackling the task of measurement highlighted by Freud's case example. It is not too dramatic to assert that much that has been done in the social sciences must be under question, because such questionnaires cannot be trusted to have placed like with like. This will become clear in later chapters.

Failure has not been, however, just a matter of inaccurate and biased measurement. Even when entirely accurate, much that has been measured has been only a most distant indicator of what might be at work in bringing about the disorder. Suchman some years ago in discussing stress and cardiovascular disease made the point that in social research:

> 'nothing is as sterile as demographic group comparisons. Rates analysed in relation to such categories as sex, age, race, marital status, occupation and geographical region are an essential part of the social book-keeping of modern society. In and of themselves, however, these rates offer little by way of explanation. If one's purpose is to explain the relation between demographic factors and coronary heart disease, one cannot help but get lost in a morass of inconclusive correlations . . . Where does the fault lie? The answer is probably to be found in the essential meaninglessness of gross demographic population categories when viewed as "causal" variables indicative of social processes. These may be convenient, easily studied labels for subdividing populations, but they are not dynamic social ideas and cannot, except in a very limited superficial sense, represent the kind of social phenomena that may cause disease or anything else.'
>
> (Suchman, 1967: 110)

Brenner (1971, 1975), in an analysis of fluctuations in death rates for heart disease and alcohol-related disorders such as cirrhosis, shows an average two to three-year delay between peaks of unemployment in the United States and the death rate peaks for these disorders. He favours a link between rates of unemployment and the raised death rates. But as Eyer (1977) has pointed out, since the business cycle itself is about four years long on average, it is possible to see the peak of death from heart disease and death from cirrhosis coinciding with the boom of the cycle. And this, of course, is a very different emphasis. Eyer argues against any kind of delayed effect and for a direct response to the consequences of social stresses inherent in an economic boom. But this could not be tested and it is a typical impasse reached by much survey-type research. In Suchman's terms, because gross variables are used, we have no way of knowing how a boom impinged on the individual – did those who died tend to work longer hours, see less of their families, drink more, feel under conflict, show signs of strain, and so on? And how did others who experienced similar changes react? Did they also react adversely – though in other ways? But it is important not to over-state the point. Brenner's analysis does have an element of the dynamic social ideas called for by Suchman. It is not that 'demographic type' measures are of no use, it is that they are not enough. What is required is their *combination* with concepts and measures dealing directly and in detail with the immediate (not necessarily contemporaneous) experience of the individual.

Edward Shils sees the same problem from a somewhat different viewpoint. In a discussion of the history of sociology he notes:

> 'The unity which transcends specialisation in sociology rests on this common devotion to a relatively small number of "key words". That unity is very expensively purchased. The key words and the ideas which they evoke have become inexpungibly enmeshed in the sociological tradition, so much so that they can never be merely an honorific decoration. They have become constitutive of sociological analysis. But the fact remains that they weigh like an Alp on the sociological mind. Theory is recognized as such by the presence of those alpine key words in all their misty and simple grandeur. This is not good enough. Sociology needs a much more differentiated set of categories, a much more differentiated set of

names for distinguishable things. It must name many more things and name them in agreed and recognizable ways. The "slippage" between "concepts" and "indicators" must be reduced by increasing and refining the variety of "concepts".' (Shils, 1970: 819)

We need to translate concepts such as 'alienation' and 'anomie' into measures that reflect, albeit often indirectly, recognizable experiences of the individual.

So far we have concentrated on some of the weaknesses of the survey. Its ability to test ideas about aetiology is beyond dispute. Its basic design can be used to compare individuals who have developed disorder with those who have not, in order to isolate 'factors' of causal importance. It does not have to rely on an experimental design to do this, although the underlying logic of its approach is the same as that found in experimental work (see Campbell and Stanley, 1966; Susser, 1973; Rosenberg, 1968). Since the extent to which a survey can reap these benefits is largely dependent on the calibre of its measures we will turn to a less well recognized strength – the way it facilitates the generation of new ideas. Unlike the case of intensive research, this feature of the survey tends to be overlooked. Particularly important is the way new ideas can emerge as part of the *ongoing* job of testing aetiological propositions. Also valuable is the impetus it can give us to leave ideas rather than to stay with them, often the crucial step in creative work. Once developed, ideas (such as Freud's views about the role of fears of castration in neurosis) tend to be held tenaciously; without the unremitting pressure of dissenting data it is surprisingly difficult to give them up. Gruber illustrates this in his fascinating analysis of Darwin's *Notebooks*: how patently inconsistent ideas were entertained for many years, apparently obvious ideas not taken up, and obvious deductions not made. The apparently brilliant insight was so often anticipated that its eventual formulation appeared all but inevitable:

> 'If scientific thought moved more swiftly, perhaps we could single out a characteristic type of sequence of great creative import, such as "first theorize, then observe" – or the reverse. But in a long process, carried out by a living person, many different sorts of act occur repeatedly in different sequences, and, since each act may itself be prolonged and interrupted by other acts, important acts sometimes occur in parallel with each other, extended over the same period of time' (Gruber, 1974: 123)

In survey research there can be the same lengthy cross-fertilization of data and ideas. New variables are introduced, usually one or two at a

time, into existing data. Are widows living with children less likely to commit suicide than those living alone? And in the light of the answer new questions arise to be tested. What about widows who have never had children or those with children living nearby? Since possible tabulations of possible factors probably run into millions, it is never a matter of learning what is there: there has to be selection – hopefully guided by theory – of what is looked at. Worrying about data that so often appears to go round in circles can take years. One of the destructive results of the new computer technology is that it can brutally cut short this interweaving of evidential and theoretical activity.

An interesting point made by Gruber's analysis of Darwin's *Notebooks* is that significant advances do not necessarily, or even usually, come from an entirely new idea as such. (Given the amount of speculation in psychiatry about causality it is probably difficult in any case to have an entirely new idea!) As important as a new idea is the combination of existing ideas in a novel way. The survey approach with its tradition of looking at a number of factors simultaneously in relation to the dependent variable (e.g. suicide) seems particularly well-suited to suggest such insights:

> 'It is a serious error to suppose that the main features of a complex idea are adequately characterised by the more elementary ideas which make it up. If that were the case, the discovery of a new component idea and its introduction into a theoretical structure would always be the most prominent kind of event in the growth of the complex structure.
> In fact, however, very profound changes in the nature of a complex idea may depend mainly on the rearrangement of its components to form a new structure.' (Gruber, 1974: 156)

We have discussed in very general terms some of the strengths and weaknesses of the survey; it has great potential but, with some notable exceptions, its promise has not been fulfilled. At this point it may be useful to discuss more fully particular epidemiological studies of psychiatric disorder. We have chosen two of the best and in our comments we intend in no way to detract from their importance or depreciate their achievements.

Two epidemiological studies of psychiatric disorder

The Midtown survey found that 23 per cent of a sample of 1,660 inhabitants close to the centre of New York were psychiatrically *impaired*. In the first report, *Mental Health in the Metropolis*, a series of

demographic variables including age, sex, marital status, parental and own socio-economic status, rural-urban origins, and religious affiliation were related to the prevalence of psychiatric disorder (Srole *et al.*, 1962). A second volume related psychiatric disorder to factors that are part of the biographical experience of the individual (Langner and Michael, 1963). Past influences were: parental physical and mental health, childhood health, a broken home, parental quarrelling and disagreement, childhood economic deprivation, and the way the person perceived parental character; and in the present they were: work worries, socio-economic worries, the adequacy of interpersonal affiliations, and marital and interpersonal worries. This information was collected by standard questions and is of somewhat dubious worth; the reporting of parental character and even parental health, for instance, may clearly have been influenced by the psychiatric state of the respondent, the very phenomenon under study. But, leaving this to one side, the findings in the second volume are intriguing because they show that, although biographical factors relate quite highly to psychiatric disturbance, they do not explain its association with a lower social-class background.

The various biographical factors were used in a combined stress score, which showed a consistent association with psychiatric impairment: the mere *numbers* were important rather than any particular combination (Srole *et al.*, 1962: 377). Events in the life history seem to 'pile up' bringing with them increasing impairment, rather than there being one factor that by itself automatically spells mental disorder for those who experience it. However, these biographical factors were only very modestly associated with parental socio-economic status. Lower-class persons did report somewhat more 'adult' stresses, but no more from childhood (Srole *et al.*, 1962: 151). The surprising conclusion is that those in the low-status groups show greater psychiatric impairment *even when the number of stress factors experienced is controlled*. Therefore, even if the measures are accepted without question, they fail to explain the association between social class and psychiatric disorder. Although there was a sizeable association between social-class background and disorder, there was no understanding of the reason for the correlation. In terms of our previous discussion the link between social structure and the individual remained completely open.

The second survey, of a Canadian maritime population, involved a good deal of ethnographic field-work, and yet the links made between the individual and the wider social structure are on the whole no more convincing (Leighton, 1959; Hughes *et al.*, 1960; Leighton *et al.*, 1963). In spite of the descriptive material collected in the ethnographic surveys, much of the third volume containing results consists of speculations about demographic associations. About sex differences in psychiatric disturbance they comment:

> 'Certainly an outstanding characteristic among all the welter of changes in modern times is alteration of the sentiments of and toward women and in the roles open to and expected of them. There are similar changes for men, but they are not on the whole of the same magnitude. One may also note that men are affected by the changing position of women because they are husbands, brothers, sons and fathers of the women who are touched by their problems. This latter no doubt takes its toll, but it is not the same thing as being in the direct line of fire.' (Leighton *et al.*, 1963: 366)

Altered attitudes to women were not measured and it is, in any case, difficult in such general comments to sense much hint of the dynamic social ideas called for by Suchman. Nonetheless this survey is a good deal more sophisticated than most and it does go some way to test its core notion that the social disintegration of a community or village is related to its rate of psychiatric disorder. (Indicators of social disintegration were economic inadequacy, cultural confusion, widespread secularization, few and weak group associations, few and weak leaders, few patterns of recreation, high frequency of crime and delinquency, high frequency of broken homes, high frequency of interpersonal hostility, and a weak and fragmented network of communications (Leighton *et al.*, 1963: 26)).

For a crucial test two communities were picked out (by local informants) as outstandingly high on integration and three as outstandingly disintegrated. Results were in the expected direction although one of the two integrated communities was close in its rate of disorder to the less integrated areas (see Leighton *et al.*, 1963: figure 19, 331).

The authors are fully aware that the most parsimonious interpretation of these results is that the differences are due to long-term selective processes. The disintegrated villages had been economically depressed for a long period: it is likely that more 'disintegrated personalities' failed to move elsewhere. However, it is unlikely to be the sole explanation. Alexander Leighton (1965) in a separate paper has described one of the disintegrated and depressed villages, which after 1950 gradually reached a state of comparative independence and self-sufficiency. At the turn of the century it suffered a major loss of economic support, and, although some inhabitants moved, enough remained to perpetuate the community. At the time of the first survey there was severe poverty, and the families showed a high rate of broken marriage, parental quarrelling, and child neglect. In spite of the

smallness of the village there was a surprising degree of isolation between families. After the initial survey social conditions began to improve, including employment opportunities. Correlated with the changes there was an overall reduction in psychiatric disorder so that by the second survey, fifteen years later, the community's rate was the same as that of the county as a whole. While material on the *same* individuals is not reported, it seems likely that there had been a real change in the amount of psychiatric disorder, suggesting that in spite of the possibility that the more healthy had moved away, environmental factors are important in actually producing or alleviating psychiatric disorders.

Overall reaction to the study, given the years of effort that went into it, is a sense of disappointment. Measurement of psychiatric disorder leaves much to be desired, social factors are poorly specified, and the disintegrated communities so profoundly underprivileged that the generality of any finding must be in doubt. But most of all there is disappointment that studies based on interviews with respondents (and intensive ethnographic field-work) should give so little sense of how the disorders are grounded in their day-to-day lives. Both this and the Manhattan study fail to close the gap between 'social structure' and individual disorder. Of particular interest, therefore, are studies – so far rare – that combine some of the features of survey and more intensive, case-like, research.

We have already mentioned Meyer and Haggerty's study. This was in a number of ways a landmark: while the size of the effect was modest, it demonstrated that the onset of a particular illness could be linked to the day-to-day lives of those studied. One consequence of such an approach is to force both investigator and reader into serious concern with causal issues. The issues that demand to be settled are both more obvious than in larger enquiries and apparently more tractable. Did the event really come before the sore throat? Perhaps it was the effects the crisis had on the change in daily routines (meal-times, etc.) rather than the emotional effects of the event as such for the child? Meyer and Haggerty's study can answer some of these questions and not others. It still lacks much of the convincing detail of the more clinical type enquiry; and it fails to relate the details of daily life that it did collect to wider societal phenomena such as social class. However, the impetus to explore further is there. What is needed is work on larger population groups and the development of theory about what is going on within the immediate social milieu of the person, investigating whether this links, and in what way, to broader social processes.

This sums up our review so far. Social research has by and large so

far failed to link in a persuasive and testable way broad social categories (social class and sex), intervening processes (e.g. sex roles), proximal causes (e.g. major crises such as loss of a confidant) and disorder (e.g. depression). However, a number of studies begin to show how this might be done.[1]

Should we study illness or illnesses?

So far we have tended to concentrate on the methodological weaknesses of aetiological research. In a recent authorative review Cassel allocates greater blame to theoretical shortcomings:

> 'Despite increased efforts, however, attempts to document the role of social factors in the genesis of disease have led to conflicting, contradictory, and often confusing results. There is today no unanimity of opinion that social factors are important in disease etiology, or, if they are, which social processes are deleterious, how many such processes there are, and what the intervening links between such processes and disturbed physiologic states may be. In part this unsatisfactory state of affairs is a function of the methodologic difficulties inherent in such studies, particularly the difficulties of measuring in any precise form such relatively intangible processes. To a larger extent, and underlying these methodologic difficulties are, I believe, inadequacies in our theoretical or conceptual framework.' (Cassel, 1974: 471)

While Cassel is perhaps a shade too pessimistic about what has been achieved, his argument is persuasive and it leads aetiological research in a rather different direction to our own.

He argues that we have been led astray by the role micro-organisms play in certain diseases. Following the classical model of tuberculosis and the tubercle bacillus, particular social stressors have been seen in research so far as leading to *particular* diseases. He argues instead that social stressors raise susceptibility to disease in general and we should, therefore, begin to study disease as a whole. There is evidence for this view. For instance a Swedish study looking at risk factors for heart disease, found, as expected, that factors such as raised serum cholesterol, smoking, and alcohol consumption were associated with a higher risk of death from ischemic heart disease, but they also found that these same factors increased risk of *any* early death (Tibblin, Keys and Werkö, 1971). Syme notes that although New York and California have higher coronary rates than North Dakota and Nebraska, the proportionate mortality from coronary heart disease in these states is about equal. North Dakota and Nebraska have low death rates from

coronary heart disease, but their all-cause death rate is also low. He suggests that the problem is not to explain why New York and California have a higher *coronary* rate than North Dakota and Nebraska, but why New York and California have a higher *death rate* as a whole than North Dakota and Nebraska (Syme, 1967: 176). In spite of this kind of evidence we disagree with Cassel's emphasis. First, such a general effect, even if it exists, is not without important exceptions. We will show for instance that *different* kinds of life-event influence the onset of schizophrenia and depressive disorders. We also make clear that this could not have been demonstrated if 'depression' and 'schizophrenia' had been lumped together in some general category of 'psychiatric disorder'. Much the same may well occur for physical disorders (e.g. Cobb and Rose, 1973). At present disorders seem best combined only after it is established that they have aetiological factors in common.

Our second objection can be illustrated by Cassel's own examples. He notes that a 'remarkably similar set of circumstances characterize people who develop tuberculosis and schizophrenia, alcoholics, victims of multiple accidents, and suicides'. Common to all of these people is a 'marginal status in society'. Leaving aside the question of evidence for this statement (which is poor) the concept of 'marginal status in society' is remarkably general and so vague that it could encompass both a link between, say, social isolation and suicide, and one between overcrowding and schizophrenia. Isolation and overcrowding are probably both characteristics of 'marginal status in society', but can hardly be said to represent a common social stressor. Before this could be claimed more specific causal links would need to be established – a point already made in general and in relation to the Midtown and Nova Scotia studies. The present point is that there must be a danger that only the broadest and vaguest social measures are likely to show associations across a wide range of disorder.

So far this discussion may be met largely by agreement from our colleagues in the social sciences. But our critique of Cassel leads to a position where we may well differ from most. We have concentrated largely on one psychiatric condition (depression) and in order to do this we have taken the existing framework of psychiatric diagnostic classification seriously.

Brewster Smith, a relatively moderate critic, argues that in using diagnostic categories we are captives of a metaphorical clinical terminology that, for good reasons, became current in a social context different from the one we now face, and that it

> 'leads us to a continued preoccupation with symptoms and syndromes, to a strategic commitment to the search for disease entities,

> to the appraisal of human effectiveness in terms of the sum of a person's symptom-like liabilities with inadequate attention to the concurrent sum of his strengths . . . The health-and-disease model also biases us toward a pre-emptive concern with the individual organism, so to speak in vitro, and, by extension, with intrapsychic processes. It predisposes us to neglect the context of structured social relations in which effectiveness or ineffectiveness is displayed, which contributes to their genesis, and which must be dealt with by programmes of intervention that aim at increasing the balance of effectiveness.' (Smith, 1974: 124)

The argument, as far as it goes, is convincing: concern with symptoms and syndromes has been associated with a gross neglect of the role of the current environment and a person's possible strengths in combatting it. Psychiatrists come to their work after a long training in physical medicine and most are concerned to find a physical basis for the major psychiatric disorders. Diagnostic practices have been caught up in this basic concern. But it does not follow that this is an inevitable consequence of concern with diagnosis: all that it is necessary to rectify the situation *is not to allow it to happen*. Classification (a major job of diagnosis) can be used in support of *any* explanation of psychiatric phenomena.[2] This can be seen in the continued use of diagnostic terms by some of psychiatry's foremost critics. R. D. Laing, in spite of his acerbic strictures, still uses the term schizophrenia – albeit with ill-grace. He does so, because like everyone else, he needs to communicate. Clinical work as well as research is impossible without a means of reducing the variety of psychiatric phenomena to a provisional order. This can only be done by classification. And once this is accepted, it would be foolish, in setting up a diagnostic system, to neglect the impressively detailed descriptive work by psychiatrists over the last century.

However, concern with diagnosis has created considerable confusion in psychiatry; not least in understanding the role of social factors in aetiology. Brian Cooper (1976) in a valuable review of the history of the concept of reactivity in psychiatry notes how:

> 'The 19th-Century preoccupation with specific disease-entities strongly influenced psychiatric thinking about aetiology. It was argued that, before a mental disease could be classed as exogenous, a necessary and sufficient external cause must be demonstrable in every case; if the external factors exercised a merely quantitative, or contributory effect, then the prime cause must be internal, and the disease an endogenous one. This line of argument inevitably weighted the scales in favour of an endogenous hypothesis, since

the predisposing internal factors did not have to be demonstrated, whereas any supposed external factors did: in all cases of doubt, the onus of proof rested with the environmental hypothesis.'

While medical thinking has changed a good deal, traces of this kind of argument can still be found. However, use of diagnostic terms does not need to imply any view about aetiology. For example, the use of a diagnostic classification that discusses 'psychotic' and 'neurotic' *symptom patterns* in depression need not and should not initially imply anything about the presence of precipitating events, even though psychiatrists have traditionally assumed there is a link. Any such assumption is bound to lead to circularity in aetiological research. If one of the criteria for classifying someone as 'psychotically' rather than 'neurotically' depressed is the absence of a precipitating event, then it is bound to follow that research based on this criterion will pinpoint a connection between events and neurotic rather than psychotic depression. It is important to prune from the existing diagnostic categories *any* variables which are to be considered of possible aetiological importance (Blumenthal, 1971; Ní Bhrolcháin, 1977). Another common pitfall is the notorious unreliability of the diagnostic categories, which has been documented in a series of studies and recently highlighted by the study of British and American psychiatrists: it emerged that a large proportion of patients diagnosed as schizophrenic by American psychiatrists were diagnosed by British psychiatrists as suffering from depression (Cooper *et al.*, 1972). Once again the answer to this is not to allow it to happen, but to strive for strict standards of reliability in establishing the boundaries of any category. There can be no doubt that this can be done – certainly in a research setting. If such pitfalls are avoided, the use of the conventional classification in aetiological research can be seen more optimistically as a process of refining and distilling the experience of a century of psychiatric experience. This has seemed preferable to attempting an independent contribution from outside psychiatry: one which would, even if successful, only with difficulty be integrated into the tradition and practice of those dealing with psychiatric disorder.

We therefore turn next to a discussion of the nature of the condition we have sought to explain – depression.[3]

2 Depression

It was with the considerations of the last chapter in mind that we set out to study a particular condition rather than psychiatric disorder in general and in doing so to take seriously existing psychiatric classifications of depression. We felt that, if we could guard against the obvious pitfalls we have discussed, any disadvantages would be far outweighed by the advantages of working within a tradition where our work would have meaning, and, just as important, be subject to informed criticism.

First of all we had to face the problem of 'illness behaviour' (cf. Mechanic, 1968; Tuckett, 1976). Very far from all instances of psychiatric and physical disorders receive treatment. There is also every likelihood that the reasons why some do not receive treatment are related to factors of aetiological significance (e.g. Mechanic and Newton, 1965). For instance, while caring for her young children may increase risk of various disorders it may at the same time inhibit a woman from seeking medical help. For this reason we determined to study untreated instances of depression which we expected to pick up in a random sample of the community, as well as women who were depressed and receiving different forms of medical care. It was thus not possible to rely solely on the definitions of depression of the various doctors and clinics; it became imperative to develop standards that we could use for depressed women who were not in treatment.

Another feature of the research, which perhaps needs some explanation, is its concentration on women. This stemmed not only from the decision to look at untreated depression but also from the decision to take as a comparision group a random sample of people without depression from the same community from which the patient group was drawn. In order to avoid bias, the sample not only had to be

15 Depression and loss

The time has now come to draw together the various factors we have identified as significant in producing and shaping depression. The main task of this book has been to develop a causal model of clinical depression: this has been done and, we believe, it is sufficiently well based for some attention to be paid to the theory that we have developed to make sense of it. But the two must be kept distinct: claims that we make for the causal model cannot be made as yet for the more speculative theory.

We have identified three broad groups of factors; the provoking agents, the vulnerability factors, and the symptom-formation factors. These we believe relate in differing ways to a central experience of hopelessness which develops out of the appraisal of particular circumstances, usually involving loss.

Hopelessness and depression

Recognition that loss plays an important role in depression has, of course, been widespread. While a good deal of the extensive research literature has dealt with death, Freud made the point in *Mourning and Melancholia* that the object need not necessarily have died but simply have been lost as an object of love. The way in which we have categorized events follows a similar line of thought. Basically we have seen loss events as the deprivation of sources of value or reward. We now go further to suggest that what is important about such loss for the genesis of depression is that it leads to an inability to hold good thoughts about ourselves, our lives, and those close to us. Particularly important, as Melges and Bowlby (1969) have argued, is the loss of faith in one's ability to attain an important and valued goal. But this

must not simply be equated with disappointment and adversity. Most of us (we will not bother with rare exceptions) strive to hold ourselves and those close to us in high esteem – as a good mother, or father, wife or husband, housewife, worker, friend, home decorator, and tennis player, although each of us differs in the relative importance we give to such activities and roles. Sources of value can come from a person, a role, or an idea; but it would also be misleading to see such rewards as mutually exclusive. A mother can value a child for his presence, obtain a sense of identity from her maternal role, and gain reward from fantasy about what he will become. A further point is that it is possible to hold good thoughts about them even when all is far from well with our world. The fact that we are beset by difficulties will not *necessarily* detract from our ability to feel all right about things; indeed, if we can believe we have stood up well to adversity, feelings of pride and self-worth may increase. The point is not obscure: the ability to feel good about things is not a straightforward function of the amount of 'difficulty' and 'failure' in our lives.

The fact that reward can be got simply from ideas means that the past, the present, or the future are involved; it follows also that scope for suffering is increased. Ideas about the future may have had only a tenuous link with reality and yet still be experienced as great loss if they can no longer be believed. This independence of ideas from place and time is important for an understanding of loss. The worth of a person or a role does not necessarily disappear with the loss of the person or the role – a widow can continue to have good thoughts about her marriage. In the same way, good thoughts will not necessarily be possible even if person or role remain unchanged. A woman deprived of a lover will not always lose good thoughts about herself or lose hope of gaining another. She may retain the warmest memories and remain confident of her attractiveness and capacity to love. Alternatively, the parting may cast doubt on what she had seen as a successful relationship and lead her to question her ability to rebuild any worthwhile relationship with a man. In other words, the implications of loss usually stretch far beyond the fact of the loss itself. Like Melges and Bowlby we believe hoplessness is the key factor in the genesis of clinical depression and loss is probably the most likely cause of profound hopelessness. But it is not just loss of a particular 'object' that has to be dealt with, so much as its implications for our ability to find satisfactory alternatives. The process of loss can be likened to a series of Russian dolls one within the other – but in a Lewis Carroll world where each succeeding doll may prove to be larger than the last. In loss it is not always just the current situation that is involved (although, we believe, the significance of this is often underestimated). The present is

bound to some degree to awaken our past. This has long been recognized: that crises will often awaken 'unresolved conflicts', memories, and emotions. Some emphasise the potential for growth and adjustment here – stressing that the crises give us another chance of dealing with the past. Clinical experience abounds with examples of individuals and families who 'rise to the occasion' when confronted with crises, thereby not only successfully mastering the exigencies of the current situation, but also dealing more adequately with long-standing conflicts that have been suppressed or repressed (Parad and Caplan, 1965: 57).

The immediate response to loss of an important source of positive value is likely to be a sense of hoplessness, accompanied by a gamut of feelings, ranging from distress, depression, and shame to anger. Feelings of hopelessness will not always be restricted to the provoking incident – large or small. It may lead to thoughts about the hopelessness of one's life in general. It is such *generalization* of hopelessness that we believe forms the central core of a depressive disorder. It is this that sets the rest of the syndrome in train. We are not the first to believe this (or at least something like it). Aaron Beck has focussed upon a similar cognitive component of clinical depression. While we do not rule out that at times physical factors may be largely responsible for clinical depression, we believe that in most instances a cognitive appraisal of one's world is primary – and it is from this that the characteristic bodily and psychological symptoms of depression arise. This is not to deny the importance of research on the physical basis of depression. Clinical depression involves profound bodily changes. What we assert is that until such work is extended to take account of bodily processes *before* the onset of depressive symptoms, it has no strict aetiological relevance in the sense used in this book; for the bodily changes may well form part of the dependent rather than the independent variable.

Our argument so far is incomplete. Why do relatively so few people develop such hopelessness? A less familiar component of our theory is that a person's ongoing self-esteem is crucial in determining whether generalized hopelessness develops – that is, response to loss and disappointment is mediated by a sense of one's ability to control the world and thus to repair damage, a confidence that in the end alternative sources of value will become available. If self-esteem and feelings of mastery are low *before* a major loss and disappointment a woman is less likely to be able to imagine herself emerging from her privation. It is this, we believe, that explains the action of the vulnerability factors in bringing about depression in the presence of severe events and major difficulties. They are an odd assortment: loss

of mother before eleven, presence at home of three or more children under fourteen, absence of a confiding relationship, particularly with a husband, and lack of a full- or part-time job. (Reversal, of course, will express them as protective factors – *not* losing a mother before eleven and so on.) We suggest that low self-esteem is the common feature behind all four and it is this that makes sense of them. There are several terms other than self-esteem that could be used almost interchangeably – self-worth, mastery, and so on. In the end we chose it because it was a term sometimes used by the women themselves (although they more often talked of lacking confidence). We were particularly interested in a few of the women who took up employment a few weeks *after* the occurrence of a severe event, none of whom developed depression. One working-class woman who had previously not worked for six years commented that 'the money was not much' but it 'gave me a great boost' and 'greater self-esteem'. The relevance for the women of the three vulnerability factors occurring in the present would probably lie in generating a sense of failure and dissatisfaction in meeting their own aspirations about themselves, particularly those concerning being a good mother and wife – this giving them chronically low self-esteem.

When discussing Henry VIII's reaction to the possibility of Katheryn's adultery we related the idea of meaning to plans of action – the King was unprepared in the sense he had 'formed neither a plan nor a preference' for another liaison. McCall and Simmons in *Identities and Interactions* (1966) made the point that the major source of our plans are *role identities*, the imaginative view an individual has of himself as an occupant of a particular social position. While these are usually socially based, quite idiosyncratic ideas of oneself can be incorporated. We believe it is these role identities that are usually involved in the hopelessness that precedes clinical depression. McCall and Simmons see the identities as woven into various more or less cohesive patterns.

> 'The basis for this clustering is ordinarily that several role-identities involve similar skills, have the same persons "built into" their contents, or pertain to the same institutional context or period of one's life. These clusters may themselves be linked more or less closely with other clusters or may be quite rigidly "compartmentalized" or "dissociated" from others.'
>
> (McCall and Simmons, 1966: 76–7)

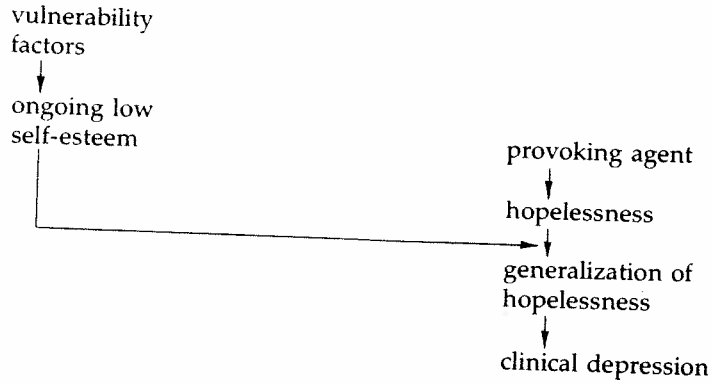
The more a woman has committed herself to a given identity or cluster of identities the more her 'assumptive world', in Parkes's sense, will be caught up in it and the greater the severity of a crisis that deprives her of an essential part of it. Our concepts of general and

specific appraisal were based on the way women respond to 'external' events and difficulties – the way they put them together in their mind. In general appraisal there is a simple addition of distress as if the thought 'oh yet another thing' is the final cause of breakdown. In specific appraisal, additivity of events rests on the particular implications of the first event for the second. It is not, however, easy to move from these ideas to the impact of provoking agents on role identities. It is possible to imagine the general appraisal of a number of 'unrelated' severe events influencing just one role identity – learning that one's child is in trouble with the police and that another has failed an important examination may be quite unrelated in an 'exteranal' sense but jointly have a devastating impact on a woman's notion of herself as an effective mother. It is also possible to conceive of a specific appraisal of 'one' event influencing several identities – and even more complex possibilities. Since little is known about the organization of these identities, we can only speculate. In our various discussions of the 'additivity' of provoking agents we have suggested that it is hopelessness about restoring a particular source of value that is usually crucial. This may be something that was once had (a husband's love) or something which was only wished for (a new place to live). It is now clear that this must not be taken too literally; although the hopelessness usually starts from a particular focus, just what is involved in terms of 'psychic additivity' will depend on the underlying role identities. A general appraisal may relate to a specific identity and a specific appraisal to a number of quite disparate identities. However, on the whole, some broad comparability between the two would be expected. It is possible that what is *left* of a role identity or identities after a provoking agent will determine vulnerability. If important role identities are left, a woman will have more on which to build for the future; and one way of viewing vulnerability is in terms of the hope a woman can bring to her situation.

It is not difficult to see how three of the factors we have identified as enhancing vulnerability may relate to role identities or how their reversal (having a full or part-time job, a close relationship with husband, not having three or more children under fourteen at home) can be protective. In the case of employment, not only does the role identity of worker become available to a woman but her extra social contacts will often provide her with new interpersonal identities. The existence of an intimate relationship most probably acts by providing not only a role identity but also one that is likely to be appraised as successful, and thus a source of self-esteem. In a similar way it is probably usually easier to perform successfully in the role of mother when there are fewer than three children under fourteen, and it is

easier for a woman to spend time outside the house building new role identities if she has fewer children who can be left with neighbours or relatives, or even accompany her more easily.

We therefore suggest that the vulnerability factors play an important role because they limit a woman's ability to develop an optimistic view about controlling the world in order to restore some souce of value. Of course, an appraisal of hopelessness is often entirely realistic: the future for many women *is* bleak. But given a particular loss or disappointment, ongoing low self-esteem will increase the chance of a general appraisal of hopelessness:



In this view loss itself is less important in producing depression than feelings of profound hopelessness, which may or may not stem from loss. The fact that major loss *is* often present before onset of depression means no more than that it is the most common way in which sources of value can be placed in jeopardy.[1] But hopelessness may occur simply in response to thoughts about a possible loss. The hopelessness of the divorced women who became depressed when her young daughter returned from holiday apparently stemmed from her pessimism about her future loneliness and this was triggered by her dwelling on the time when her daughter would finally leave home.

Loss of someone much loved will not necessarily lead to hopelessness. Even with great grief and distress there may be awareness of new possibilities and an underlying sense of hope. In our research we have been forced to neglect 'personality' factors as it was impossible to collect them accurately enough when dealing with women already depressed. If they play a role, it is probably substantially related to such a dimension of optimism-pessimism.

So far we have speculated that the common feature of three of the vulnerability factors is their association with feelings of low self-esteem and lack of mastery. Since they all concern the present, it follows that it is the current environment that is the most important influence. Intensive studies of the life of women in London are now needed to move beyond this. Recent studies of housework and attitudes of women with young children towards employment outside the home begin to do this (Gavron 1966; Oakley 1974; Ginsberg 1976).

For example, Susannah Ginsberg argues that work outside the home becomes a source of self-esteem for many women within a society where status accrues to economic gain. A woman may have spent years caring for her children but still not be considered qualified for any job – child-care or other – on the basis of this experience. The relatively low status attributed to the work of child-care can sometimes be internalized by the women in terms of feelings of low self-esteem. Although work outside the home may share the monotony and boredom that, as Ann Oakley describes, many women find in child-care and housework, it at least gains recognition in terms of economic reward. This may be one reason for the protection provided by paid employment. Of course, this does not rule out the more obvious assets of a job outside the home – social contacts and respite from the demands of small children.

The presence of loss of mother before eleven among the four vulnerability factors is more difficult to explain. It may lead to enduring changes in personality of the woman herself or, given that such a loss is correlated with low intimacy and three or more children under fourteen, it may impart a greater risk of undergoing certain adverse experiences, it being these that leave her more vulnerable. Both, of course, may be at work and indeed may be so in the same woman. The women may have greater dependency needs as suggested by Birtchnell (1975); and these may make them more likely to marry the 'wrong' man, if only because they are more likely to settle with the first man willing to live with them. While we have no means at present of a direct test of these possibilities there are some clues.

We have already noted that loss of mother before eleven is related to low intimacy with husband and to having three or more children under fourteen. *Table 1* goes further by showing that even when one or other of these factors is present, loss of mother is still associated with a greater risk of depression for women with a provoking agent: two-thirds of the women with low intimacy *or* three or more children under fourteen became depressed if they had lost a mother before eleven compared with only a quarter of those without such a loss ($p<.05$). This suggests that early loss of a mother leads not only to certain adverse experiences (e.g. low intimacy and three or more children under fourteen) but also to changes in the personality of the woman herself. It is certainly not unlikely that

loss of mother before eleven may have an enduring influence on a woman's sense of self-esteem, giving her an ongoing sense of insecurity and feelings of incompetence in controlling the good things of the world.

But although depression among women with loss of mother before eleven is extremely common, we do not suggest that the experience is inevitably linked with psychiatric disorder and unhappiness. With luck (or wisdom), especially in choice of a husband, such women may do particularly well.[2]

Table 1 *Women in Camberwell developing depression by whether they had (i) low intimacy with husband/boyfriend or 3 or more children under 14 (ii) lost a mother before 11 and (iii) a severe event or major difficulty*

	low intimacy with husband or *three or more children under 14*			*high intimacy with husband* and *less than three children under 14*		
	loss of mother before 11			*loss of mother before 11*		
severe event or major difficulty	*Yes*	*No*		*Yes*	*No*	
	%	%		%	%	
yes	*64* (7/11)	*23* (18/78)	$p < .05$	*0* (0/4)	*11* (8/71)	n.s.
no	*0* (0/6)	*3* (2/71)	n.s.	*0* (0/9)	*1* (2/169)	n.s.

If this general view of loss of mother as a vulnerability factor is accepted, it is still necessary to explain its two special features. First, that the loss of any other close *relative* does not increase risk of depression and second, that loss of mother *after* the age of eleven plays no part. The first can possibly be explained by the fact that the mother will usually be the largest source of appreciation and support. A father's or sibling's disappearance is likely to be a less painful experience for a child. The second point might be explained by the fact that until a child is about eleven the main means of controlling the world is likely to be the mother. Thereafter the child is more likely to exert control directly and independently. The earlier the mother is lost, the more the child is likely to be set back in his or her learning of mastery of the environment; and a sense of mastery is probably an essential component of optimism. Thus, loss of mother before eleven may well permanently lower a woman's feeling of mastery and self-esteem and hence act as a vulnerability factor by interfering with the way she deals with loss in adulthood.[3]

The writings of John Bowlby on the infant's reaction to separation lend support to this view. In his two volumes *Attachment and Loss* he has built an impressive theoretical edifice reshaping classical psychoanalytic instinct theories in the light of the studies of ethologists, particularly Lorenz and Hinde, and of child development psychologists, such as Ainsworth, who have studied mother and infant in the first years of life. He elaborates a dimension of the security-insecurity of the child's attachment to a principal figure (usually, but not necessarily, the natural mother), which he sees as the basis for the growth of self-reliance as an enduring personality characteristic. Following Ainsworth's data he argues that closeness and 'relatively much physical contact in the earlier months . . . does not make [an infant] into a clingy and dependent one year old; on the contrary it facilitates the gradual growth of independence' (Bowlby, 1973: 405). It follows that the converse, abrupt severing of an attachment, can interfere with the growth of self-reliance. This idea has many echoes in the writings of psychoanalysts. Bowlby's 'security of attachment' seems clearly to refer to the same feature of infancy that Benedek (1938) refers to as 'relationship of confidence', that Klein (1948) refers to as 'introjection of the good object', and that Erikson (1950) refers to as 'basic trust' (Vol 1: 339). Bowlby also cites the object-relations theories of Fairbairn (1952) and the works of Winnicott (1958): 'maturity and the capacity to be alone implies that the individual has had the chance through good-enough mothering to build up a belief in a benign environment' (Bowlby, 1973: 409). But what Bowlby has done is to transfer these maxims developed through clinical experience to the centre of a carefully thought out theory of human attachment which can be, and to some extent has been, subjected to the criterion of confirmation and falsification so necessary to any scientific idea.

In his two volumes so far, Bowlby has devoted less attention to the role of separation from principal attachment figure in childhood in leading to depression in adult life and more to its effects in childhood itself and its relationship with later agoraphobia. He does, however, argue plausibly for a depressive component in certain types of agoraphobia (1973: 352–4). The weight of the evidence he presents supports his contention that 'separations, threats of separation and losses . . . divert development from an optimum pathway (for personality development) to a suboptimum one'. Although the separations upon which he focusses are mainly before the child's third birthday, there is nothing in his model to rule out a similar role for separations between the ages of three and ten.

It is clear that our notion of a sense of mastery and self-esteem is very close to Bowlby's concept of self-reliance. Indeed when he discusses

the conditions that contribute to the development of secure attachment he cites work not only by Ainsworth but by David and Appell (1969), Sander (1962, 1964), and Bettelheim (1967) which suggests that one important aspect of self-reliance is an environment so regulated that an infant can derive a sense of the consequences of his own actions – a feature clearly related to the development of a cognitive set involving a feeling of being able to control things, that is of not being helpless.

Bowlby's two volumes represent the most concerted theoretical presentation of this viewpoint, but the work of Birtchnell among psychiatric patients, on the loss of parents in childhood, has also collected important data. Although, as we have earlier discussed, his results on early loss of parents are at best conflicting, there is one study which bears directly on the issue of vulnerability (Birtchnell 1975). Using a scale developed by Navran (1954) to measure dependence from a personality measure (MMPI) he found that women who had lost their mothers before the age of ten were significantly more dependent than women who had not. Later examination of the profiles of women who had lost their mothers between the ages of ten and nineteen revealed a degree of dependence intermediate between the other two groups.[4]

The role of the dependent personality in the onset, and especially the perpetuation, of depressive conditions in women has been highlighted by Weissman and Paykel's study of depressed women (1974). But they conclude that 'dependency' is a result rather than a cause of the depression since it 'disappears' when the depression clears up. It is difficult to assess the importance of this finding, since it is unclear from their measurement whether the dependency completely disappears or is merely reduced. Nor do they give information about loss of attachment figures in childhood, which leaves this issue unexplored.

Another important account of how early experience can crucially determine later reactions to stress is Seligman's comparison of depression with 'learned helplessness'. Using animal experiments he has shown that uncontrollable and unpredictable trauma tends to lead to passive resignation – what he calls learned helplessness. He sees this as primarily a cognitive disposition which, once established, increases the chance of an animal passively undergoing a traumatic situation (such as receiving electric shocks) rather than seeking a solution. 'Absence of mother, stimulus deprivation, and non-responsive mothering all contribute to the learning of uncontrollability . . . Since, however, helplessness in an infant is the foundational motivational attitude which later motivational learning must crystallize, its debilitating consequences will be more catastrophic' (Seligman, 1975: 150–1). The argument has obvious relevance for the study of depression, but we do not believe (as Seligman argues) that loss of mother before eleven is specifically related to so-called 'reactive' or 'neurotic' depression. We have seen that loss of mother before eleven, like the other vulnerability factors, raises chances of *any* form or severity of depression. Further we do not view learned helplessness essentially as depression. We see it as a factor predisposing a person to a depressive reaction along the lines of vulnerability factors. But like Seligman we believe that this helpless predisposition, which is the obverse of Bowlby's concept of self-reliance, can be the result of trauma in early life.

At this point we have said nothing about the most obvious aspect of a major loss – that is grief. It is not, of course, a single 'emotion'. Tennyson's *In Memoriam* and Patmore's *Odes on Bereavement* vividly document the psychological and emotional complexity of the painful search for meaning and acceptance of the loss. Costello (1976) has argued that in evolutionary terms the function of the emotions of depression and anxiety is to force us to think about our lives. Certainly these poems, and grief itself, are as much about thoughts, meaning, and purpose as about feelings. In terms of our previous discussion they explore at length other losses entailed by the primary loss.

There are many fine literary accounts of grief – for instance C. S. Lewis' *A Grief Observed* and Susan Hill's *In the Springtime of the Year*. Work by psychiatrists and social scientists is impressively consistent with such accounts. Lindemann in a classic paper published in 1944 described a syndrome of symptoms found in acute grief; since then Bowlby (1961) and Parkes (1970) at the Tavistock Institute have done much to confirm and amplify his scheme. Colin Parkes described the most characteristic feature of grief not as prolonged depression but acute and episodic 'pangs' – episodes of severe anxiety and psychological pain (1972). They are particularly common soon after loss in what Bowlby has called the phase of yearning and protest (1961). Parkes views such pining as the emotional component of an urge to search for a lost object and believes that in bereavement there is the same impulse to search as shown by many species of animal. With it there is also commonly preoccupation with memories of the lost person. Irritability and anger are also common at an early stage. As primary grief diminishes there seems to follow a period of uncertainty, aimlessness, and apathy which Bowlby (1961) has called the phase of disorganization and despair. The characteristic emotion now is depression. Bowlby and Parkes make it clear that there are in reality no clear-cut phases – that elements of each phase (Bowlby adds a third,

reorganization) persist into and alternate with elements of other phases. Although such basic patterns of response to bereavement can be discerned, grief may be delayed, exaggerated, or apparently absent and there is some evidence that these 'distorted' reactions are associated with increased rates of physical illness.[5]

Peter Marris in *Loss and Change* (1974) has related grief reactions to a variety of changes other than death. He describes in situations such as enforced rehousing and the rise of educational elites in East Africa the characteristic need to deny the change and also to accept it, outlined by Bowlby and Parkes; what has happened has to be accepted and some meaningful continuity recognized between past, present, and future. Adjustment to a major loss is therefore likely to be both painful and erratic.

> 'It provokes a conflict between contradictory impulses – to return to the past, and to forget it altogether. Each, in itself, would be ultimately self-destructive, either by denying the reality of present circumstances, or by denying the experience on which the sense of self rests. But their interaction forces the bereaved to search to and fro, until they are reconciled by reformulating and reintegrating past attachments.'
>
> (Marris, 1974: 151–2)

Marris argues that whenever people suffer loss their reaction reflects a conflict that is essentially similar to that seen in the grief processes experienced when an individual loses a person close to him. Parkes (1973; 1975) has also taken this view in a detailed study of reactions to a loss of a limb as has Fried (1965) in a study of a large-scale enforced rehousing programme in Boston.

It is essential at this point to emphasize that the need for meaning and a sense of continuity in our lives is not the same as the need for routine. Major change in routine and interpersonal contacts as such do *not* increase risk of depression. It may indeed be welcomed as long as there is continuity of purpose. (Many seek 'eustress' in dangerous sports and the like as a way of bringing variety and a sense of achievement into their lives – see Bernard, 1968.)

As already argued, it is loss of important sources of value, not change, that is crucial (although it is possible to envisage persons so sensitive to loss that almost any change is resisted). What then is the role of grief in depression, bearing in mind its 'distorted' forms? Since only 11 per cent of patients and 14 per cent of onset cases had experienced a death of someone close in the nine months before onset compared with an expected rate of 4 per cent, bereavement is not a great help in explaining depression as a whole.[6] If grief is significant it

must be because it is a common response to severe events and major difficulties – not just to bereavement.

How then does this grief relate to the model we have just outlined in terms of hopelessness and low self-esteem? In chapter 2 we claimed that particularly intense grief reactions might develop features that were additional to the central mourning experience and that corresponded to the symptoms described by Feighner (1972) and his colleagues in St Louis as characteristic of clinical depression. We based this conclusion partly on the results of the surveys of widowing by Clayton and her colleagues (1972), although their conclusions about what was and what was not 'real depression' differ from ours. (It may be remembered that they found that one month after the bereavement one third of the widows manifested enough symptoms to be definitely or probably depressed and a year later the figure was 16 per cent.) Following a parallel line of reasoning to the one developed earlier in this chapter we believe that particularly intense mourning reactions can lead to a generalization of the hopelessness that follows the loss. 'Working through' of grief usually forestalls such a generalization: as mourning proceeds the bereaved usually find hope that they can carry on without the lost person, but occasionally the process is so intense or so prolonged that it can no longer be viewed as 'normal' in the sense of within the range of the average reaction. At this point, we would maintain, grief may quite fairly be called clinical depression.

The concept of 'working through' grief is central; it is the process by which alternative sources of value can be found and accepted, and by which hope can be revived. It is however painful – the extent of pain relating, of course, to the importance of who, or what, has been lost. It will also depend, we now argue, on how 'vulnerable' a woman is, that is how easy it is for her to find alternative value sources. Sachar and his colleagues (1968) have suggested that many symptoms of depression may be regarded as helping a patient avoid this pain, by denying the loss or its significance. This idea will be familiar to those trained in psychotherapeutic techniques designed to assist patients to face losses which they have hitherto 'denied'. While it may not at first seem applicable to those patients who are tearful and sleepless, continually preoccupied by their loss, it can plausibly be applied to those patients who say they are unable to cry, who sit withdrawn and retarded, as if cocooned against too much emotion. Consider Tennyson:

> Home they brought her warrior dead:
> She nor swoon'd, not utter'd cry:
> All her maidens, watching, said,
> 'She must weep, or she will die.'

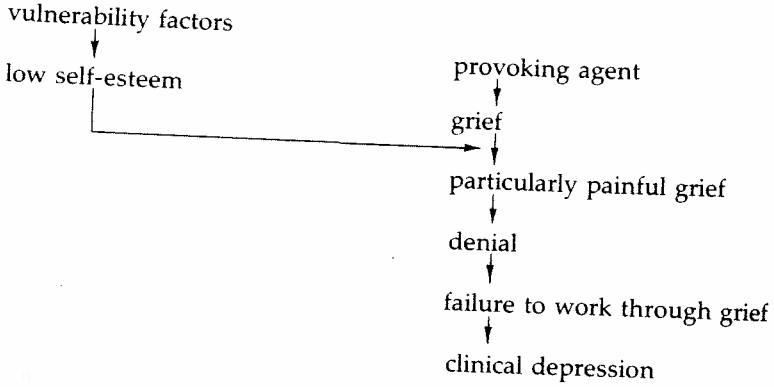
Then they praised him, soft and low,
Called him worthy to be loved,
Truest friend and noblest foe;
Yet she neither spoke nor moved.

Stole a maiden from her place,
Lightly to the warrior stepp'd,
Took the face-cloth from the face,
Yet she neither moved nor wept.

Rose a nurse of ninety years,
Set his child upon her knee;
Like summer tempest came her tears –
'Sweet, my child, I live for thee.'

(*The Princess*, Book VI)

The last verse suggests the conditions under which 'working through' grief becomes possible; crucial is the presence of hope and the child, with his future stretching before him, continuing his father's line, restores some meaning to his mother's future. There is something of the warrior that has not been lost and can become an alternative source of value. (Perhaps the poem also alludes to vulnerability factors in the role of the aged nurse as a person to whom emotion can be safely shown, recalling features of the measure of intimacy.) We therefore suggest a *second* putative process involving vulnerability and low self-esteem:



This model relates the response to present loss; but the importance of successful working through of grief perhaps receives indirect support from the material on early loss of mother. There is some evidence

that until children are about ten years old they do not readily mourn although they can be taught how to do so (Furman, 1974). If we hypothesize that the way a person will react to later losses will be influenced by their earlier experiences of loss, it seems plausible that a woman with loss of mother before eleven will be more likely to fail to work through her later grief in the way we have suggested. Thus early loss of mother may raise the risk of depression in a second way, by impeding the successful working through of grief, in addition to imparting enduring feelings of low self-esteem and mastery.

At this point we have outlined a theory that we believe accounts for the evidence we presented in earlier chapters about the types of event and difficulty acting as provoking agents and the background factors that make women vulnerable to these provoking agents.

These ideas are sufficient to explain the aetiological role of recent loss and disappointment in the form of severe events and major difficulties and also the way apparently quite minor events can 'bring home' to a woman the hopelessness of her position. They also deal with vulnerability factors that act to increase risk of breakdown in the presence of a provoking agent.

Loss of self-esteem has been given prominence before as a critical intervening variable in the aetiology of depression by a psychoanalyst, Bibring (1953). Although it had been foreshadowed in the writings of Otto Fenichel (1945), Bibring's argument was a radical reshaping of previous psychoanalytical ideas. Such psychoanalytic discussions are apt to focus attention upon a person's internal psychological resources but consideration of the issue in terms of role identities relates it to the social structure, which is where we think it belongs. For it is in the perception of oneself successfully performing a role that inner and outer worlds meet, and internal and external resources come together. Ernest Becker (1964: 111), in commenting on Bibring's views, has suggested that 'nothing less than the full sweep of cultural activity is brought into consideration in the single case of depression'. We agree: our results have pinpointed the importance of class and lifestage in making women not only open to severe events and major difficulties but more vulnerable to their impact. Nevertheless our discussion of vulnerability has so far tended to stress particular factors rather than the 'full sweep of cultural resources' implied by categories such as social class and life-stage. This emphasis has been partly dictated by the evidence itself: there was, for instance, no relationship between the educational level of either the woman herself, or that of her husband, and risk of depression once we had taken account of social class. But there were many 'cultural' factors of which we have not taken account. One is 'intelligence' itself, although it may be objected that this is an

internal rather than an external resource. But more important than any such omission was our failure to rate what can be crudely summed up as 'savoir-faire'. Knowing a lawyer, holding an insurance policy, being on good enough terms with a bank manager to be permitted an overdraft, indeed having a bank account in the first place, are external resources in the sense that they can be helpful in certain crises and are obviously closely related to one's class position. Softer, though related measures, would include the network of contacts a person could mobilize, that is, the ease with which they could approach someone with expertise to help them such as a local councillor, a doctor, dentist, accountant, or builder. Here, once again, we face the overlap between outer and inner resources; the ability to mobilize such support will depend not only on the availability of the network of contacts but also on the confidence of the person to approach these contacts, and their ability to build such a network in the first place. Yet again we are faced with the fact that mastery, with which such a network will be associated, is a factor where social and psychological influences are inextricably intermingled. Clearly some of our results are relevant: we have noted how difficulties associated with housing, money, marriage, and children take longer to be resolved when occuring to a working-class woman. We also noted that women usually had little chance of controlling the untoward consequences of the majority of severe events – this indeed is fairly clear from the simple listing of some of them in chapter 10. However, it is important not to go beyond our material.

One glaring inadequacy is our treatment of 'positive' events. We took account of all events to which women had reported a positive emotional response, combined with the number of incidents mentioned in reply to a direct question about pleasant experiences. Although we went beyond our basic life-events schedule, the measure showed only a small association with class position and even less with risk of onset of depression.[7] This result would suggest that positive experiences before onset are not themselves protective. There are various possible explanations of this; one is that only positive experiences *after* provoking agents and before onset of any symptoms can act protectively; another more probable explanation is that not all positive experiences relate to self-esteem or hope, just as we found not all severely threatening events were loss events. Yet another explanation is that most of the positive events we measured were only short-term in their effects and a measure of long-term positive effect would indeed have produced a result associating such events with protection. Finally, again, we would be faced with the same issue we confronted with severe threat: the possibility of a discrepancy between the contextual and the subjective positive rating. If a woman reports as positive the fact that her husband has already survived two months after a dangerous operation, does it mean that it is the event, rated as a positive experience, which is important or is it only because her internal resources of hope are so great that she picks this out as a positive incident to report in the first place? Since in our view hopelessness is crucial to depression it is difficult for us to believe that events that bring hope and a sense of achievement play no role in preventing onset or in improving the course of depression. Like the issue of coping in general the question remains to be explored adequately.

The wider cultural context of self-esteem: a rural study

One of the most intriguing insights that we were able to gain about the role of the 'full sweep of cultural activity' derives from a study of psychiatric symptoms in women in a different cultural setting on the island of North Uist.

In an earlier chapter we suggested that other research supports the conclusion that there is a high rate of depression among working-class women in cities. While there is a general agreement in the literature that rates of psychiatric disorder are lower in rural areas, few have carried out surveys in different areas using the same methods of measuring prevalence of disorder. As an exploratory study, preparatory to a full replication of the Camberwell research in a rural area, in 1975 we carried out a survey of psychiatric disorder in a random sample of 154 women in North Uist in the Outer Hebrides, using exactly the same methods of measurement as in Camberwell.

The most obvious difference between the island and Camberwell is that many island families are still economically productive units, though few depend any longer solely on land or sea for a living. The women were between eighteen and sixty-five years old and almost two-thirds lived in a crofting or fishing household and therefore came into close daily contact with these activities. Over a third of the women had a full or part-time job away from home; just over a third were neither employed nor made any significant contribution to farm work. Men often had other jobs in addition to crofting and fishing.[8]

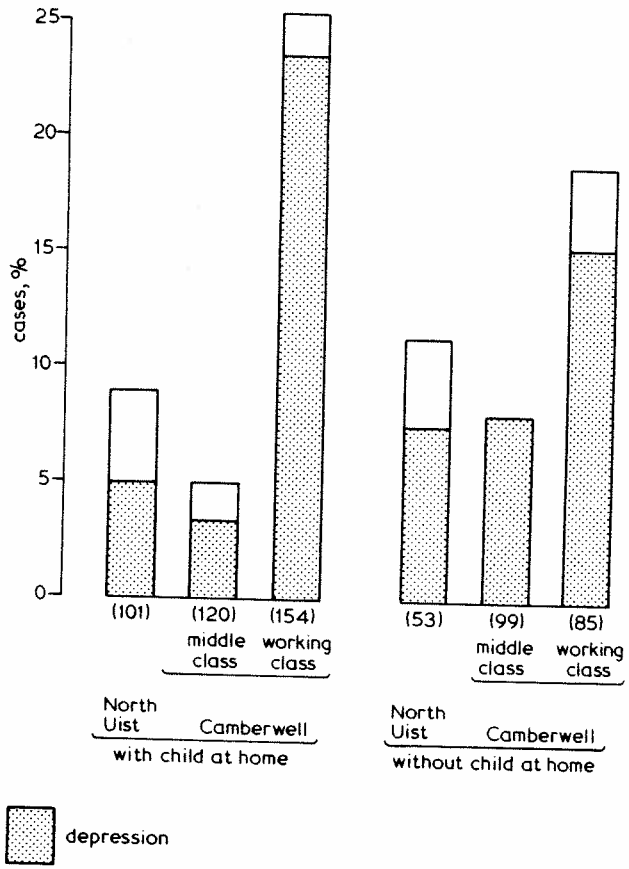
Psychiatric disorder certainly was not lacking. During the three months before interview 10 per cent of the women in North Uist were considered to be cases (compared with 15 per cent in Camberwell) and a further 14 per cent to be borderline cases (18 per cent in Camberwell), giving a total prevalence of 24 per cent of women suffering from at least a borderline psychiatric condition (33 per cent in Camberwell). However, it is only when further comparisons are made that these figures become meaningful. On North Uist there was no association between

social class and psychiatric disorder. Of course, social class must have inevitably different connotations in the two population – most crofters, for instance, would think of themselves as independent farmers although few can support themselves by farming alone. In almost a third of the crofting/fishing households the husband, or brother of the woman had another job. We therefore considered the women in North Uist in two ways. First, according to whether they lived in a crofting/fishing household (62 per cent); and second, by whether the occupation we used to classify them was (i) professional/managerial/clerical (25 per cent), (ii) skilled manual (12 per cent), (iii) unskilled (18 per cent), or (iv) solely crofting/fishing (44 per cent). In whatever ways the categories were considered, there was no suggestion that 'social class' in North Uist was related to the prevalence of psychiatric disorder. Nor did the presence of a child at home relate to the prevalence of psychiatric disorder. For the women with children at home the rate of overall caseness in the three months before interview on North Uist was quite close to that of middle-class women in Camberwell and very much less than the rate for working-class women in Camberwell. For women without children at home the difference between the classes in Camberwell was somewhat less and the rate on North Uist again approached the middle-class rate in Camberwell (see *Figure 1*).

Certain psychiatrists, writing of the North of Scotland, have had the impression of a high prevalence of depression among Highland women and some general practitioners have suggested that depressive symptoms are more noticeable among the women of the more northerly, predominantly Calvinist islands (Whittet, 1963). These beliefs were not supported by these results. Overall the prevalence of depressive cases on North Uist was about half that in Camberwell (5.8 per cent and 13.3 per cent respectively – $p<.02$). However, there was an unexpected predominance of other diagnoses on North Uist. If anxiety cases and obsessional cases are combined in a single category the three-month prevalence of such cases was 5.2 per cent on North Uist and 2.4 per cent in Camberwell – the difference does not, however, reach statistical significance. In summary, there was more depression in London and probably more anxiety and obsessional conditions without depressive components on North Uist.[9]

Since data on life-events and difficulties were not collected we were unable to look for vulnerability factors – that is by relating them to provoking agents and onset of caseness. But our data on the relationship of the prevalence of caseness and children at home had already suggested that on the island there might be different factors that were crucial for self-esteem. When we came to look at other broad

Figure 1 Proportion who were cases in the three months before interview among women in Camberwell and North Uist by whether they suffered from depression at the level of caseness or not



demographic variables we were able to obtain some clues as to what they might be.

Figure 2 suggests that, while the *overall* rate of caseness and borderline caseness may not be crucially affected by them, there are three broad demographic variables which, when combined in an index, relate to the rate of depressive and non-depressive symptomatology. This index, reminiscent of Durkheim's concept of 'integration', includes three factors related to the extent to which a person is part of, or integrated into, the island community: (i) whether or not the woman

was brought up on the island; (ii) whether or not she was living in a crofting or fishing family; and (iii) whether or not she attended Church regularly. The rate of depression in the year for combined cases and borderline cases decreased with integration; and the rate of anxiety-other conditions increased, at the extreme the conditions being predominantly ones of depression or anxiety-other.

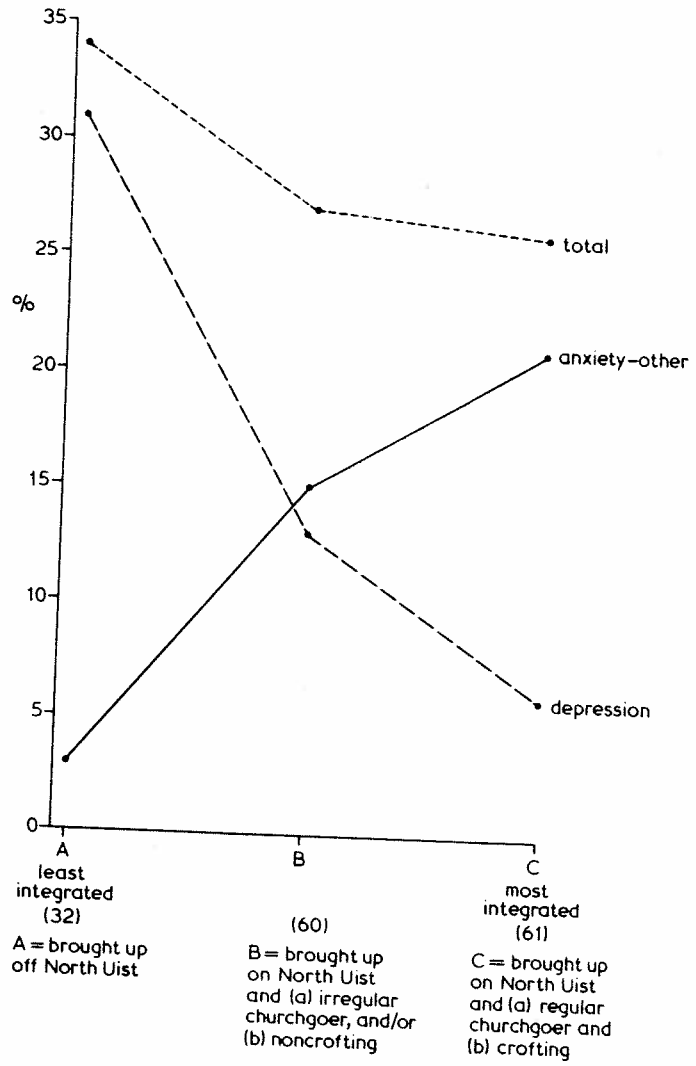
While more research is needed to draw firm conclusions about this – particularly the high rate of anxiety among the most integrated – a preliminary conclusion about the low rate of depression among the most integrated seems warranted in terms of the views we have outlined about the protective value of the successful performance of roles. A crofting housewife has more opportunity than an urban housewife to perform a range of tasks, which, barring metereological disasters, are limited and successful. Being brought up upon the island will have given a woman from her earliest years a wider range of contacts and thus interpersonal role identities. Regular church attendance offers not only a range of these identities every Sunday but also the higher self-esteem consequent upon the virtuous fulfilment of religious duty. If these are some of the factors that may account for the lower rate of depression on North Uist, there are other explanations. One emphasizes the different kinds of events and difficulties occurring in cities and particularly to the working-class in the 'inner areas'. This is just what we have documented in chapter 10.

In recent years there has been much discussion of an 'urban crisis' involving inner city areas like Camberwell. But although there are undeniably major and specific problems of recent origin, the present situation seems best viewed as the persistence of a problem. David Eversley argues that urban crisis is a

> 'catch phrase used to draw attention to conditions which have always existed and which, if anything, are now less acute than they once were, but which have come into prominence again because of the slowing down, or even reversal, of that long process of economic growth to which we all once looked to dispose of the remnants of squalor and suffering. The "crisis" is nothing but a point in time when familiar conditions suddenly present themselves as being beyond the capacity of our present system to remedy, or at least ameliorate.' (Eversley, 1977: 18)

One of the most obvious recent problems is the loss of employment opportunities. In the seven-year period 1966–72, firms in the London area shed 217,400 people and these redundancies were mainly in the manufacturing sector. Something like 22 per cent of the total number of jobs in manufacturing industry in London disappeared in this time

Figure 2 Proportion of women on North Uist suffering from depressive or anxiety-other conditions of a case or borderline case severity in the year



(Lomas, 1975). This has been, at least in part, the result of successive governments' planning policies. For many years emphasis was placed on building new industrial areas outside the main cities at the expense of the opportunities in the inner city (Field, 1977). There is also in

London, in spite of a good deal of redevelopment, a widely recognized 'crisis in low-income housing' (Eversley and Donnison, 1973). There is simply not enough of it. Often to get council housing (assuming it is desirable) a family has to subject itself to a lengthy period of unpleasant living in barely adequate conditions (Berthoud, 1976). The existence of these problems is not in dispute and they are reflected in our material. One in six of working-class women in Camberwell had at least one 'severe' event or 'major' difficulty involving *housing* and one in fourteen middle-class women. These are disturbing figures bearing in mind the severity of the problems involved. There was also evidence of the movement of industry – 3 per cent of the married women had had a husband who had lost a job through redundancy in the year. (There was, however, no class difference here.) While we now have made a start on the epidemiology of troubles in inner city areas much more needs to be done to establish links with broader economic, political, and cultural structures. For our present purpose, it is enough to say that such links can be made.

It remains to interpret the symptom-formation factors.

Past loss and depression

There is a very large association between past loss and severity and type of depressive symptoms. This only emerges when (i) severity of the depression at admission, (ii) the predominance of neurotic and psychotic features, (iii) four kinds of loss (of siblings, child, and husband and not just of parents), and (iv) whether loss was due to death or other reasons, are distinguished. This has not been done before; but five studies have looked at loss of parent during childhood and adolescence and severity of depression. Most offer some general support for the Camberwell findings. Since they ignore the other three distinctions just listed, only a modest association between loss and severity could be expected. Birtchnell (1970c) provides results for women with a loss before twenty and there is a close similarity with our material when the same simple two-fold distinction is made: 38 per cent of the most and 22 per cent of the least depressed patients had lost a parent in childhood or adolescence compared with 37 per cent and 23 per cent in our series.[10]

It is unlikely that an artefact is involved: there is no reason to believe that there is much error in recording major losses (see Barraclough and Bunch, 1973) nor that the measurement of diagnosis and past loss was in some way confounded. There is no suggestion that age, previous admissions, or social class are biassing factors. Moreover, the role of constitutional or genetic factors seems unlikely to be great. While it

could be argued that a constitutional disposition to develop psychotic depression could be inherited and that successful suicides among parents account for the high rate of past deaths among the psychotic group, there was only one parental death by suicide among the 114 patients. It could also be argued that in the past, serious depressive disorders in parents would have led to long periods of separation when they were in mental hospital; but this predicts an association between separation and psychotic depression – quite the opposite of that found. Another possible genetic line of explanation might develop from suggestions by a number of authors that there is a special younger group among the neurotics with sociopathic tendencies either themselves or among their first-degree relatives (Paykel, 1971; Winokur *et al.*, 1969, and Perris, 1966). These sociopathic tendencies might be held responsible both for parental divorce and for the development of depression of a neurotic character in the child later on if they are hereditable. But it is far from established that such tendencies are hereditarily rather than environmentally acquired. Thus in general we find it hard to conceive of an interpretation of the association between type of past loss and type of symptomatology that does not rest on environmental rather than genetic factors.

But if the results are not the result of artefacts and, if they are not explained by genetic factors, what is involved? So far we have dealt only with the role of loss of mother as a vulnerability factor. We need now to explain the effect of past loss in general and seek an explanation that differs from loss of mother when it acts as a vulnerability factor. We have already suggested that, in terms of symptom-formation, past loss may take the form of an enduring cognitive influence; that thinking about the loss influences the way one looks at the world – and particularly the way one reacts to later losses.[11] Loss by death may be related to psychotic-like symptoms because it tends to lead to a general attitude that one's own efforts are useless; that loss of any kind becomes like death, irreversible, with nothing to be done. Such an attitude may be particularly linked to denial of the implications of a loss and to greater 'bodily' expressions of symptoms. By contrast a person who has lost a parent and knows he or she is still alive will be likely to feel the situation less irredeemable. It is not as if an outside fate had been at work. This may give a less passive cognitive set than the death of a parent. It may also cause the separation to be seen as a rejection; if the parent is still alive and somewhere else it may seem that they have chosen to leave because the child is not lovable. Such an interpretation could prove the foundation for a life-long expectation of failure, which could become self-reinforcing. This distinction between the psychotic's sense of abandonment and the neurotic's sense of rejection

would fit quite plausibly with the traditional ideas of the typical forms of psychotic and neurotic depression.

The types of enduring cognitive set predicted for the two kinds of past loss have much in common with John Bowlby's characterization of the stages of development of the child's reaction to loss of principal attachment figure: first protest, then despair, and finally detachment (1973). After a separation, it can never be ruled out as impossible that protest will finally bring about a return, however unlikely this may be. After a death it is much clearer that protest will be ineffectual. An examination of the association between individual symptoms and types of past loss does suggest that a more *protesting despair* is associated with separation and a more *retarded hopelessness* with death. Of the symptoms, retardation was one of the most important items in the discriminant function analysis. It was found in 54 per cent of the total sample but in only 30 per cent of the neurotics compared with 73 per cent of the psychotics. Retardation was particularly associated with past loss by death. Among psychotics 84 per cent with a death were retarded but only 36 per cent without such loss ($p<.01$). Among neurotics with a past death, 38 per cent were retarded. Among those with a past separation 27 per cent of the neurotics and 25 per cent of psychotics had retardation. It seems likely that it is retardation that largely accounts for the more general finding we have outlined. For while 82 per cent of the patients with a past death were also psychotic, as many as 75 per cent of all those with a past death exhibited retardation.

We saw in the last chapter that the association of past loss by separation with a more neurotic score on the discriminant function could not be explained by a tendency for neurotic patients to be more anxious. This hypothesis has a certain prima facie plausability given the established view that anxiety develops under conditions of uncertainty and given that separations, unlike deaths, do contain elements of uncertainty. Our data did not, however support this interpretation.

Earlier we contrasted protesting despair with retarded hopelessness. Bearing in mind Bowlby's notion of 'protest' as a response to loss, we examined various symptom items which would be considered relevant to this theme, such as suicide gestures, verbal attacks, and violent behaviour. The latter proved particularly interesting: while only 13 per cent of patients showed violent behaviour, a third with a past loss by separation did so, and only 6 per cent of those with a loss by death ($p<.01$). There is therefore some suggestion that symptomatology among those with a past loss by separation was often reminiscent of a gesture of protest, a desperate bid for the return of lost attention. A detailed examination of the course of the disorder before admission further confirms this. A comparison of the styles of the

eleven suicide attempts among the neurotics (36 per cent of whom had had a past loss by separation) and the eight attempts among the psychotics shows that among the neurotics the attempts were less serious – often only a few pills were taken or wrists were cut with histrionic shouts but only small scratches. Most were attempts designed to be found by those close: for example, one girl who later told the psychiatrist that it had been an 'attention-seeking gesture' deliberately went to her boyfriend's flat before taking the tablets. Among the psychotics the attempts were more serious: one woman had felt suicidal for at least one month but had refrained from an attempt because she feared she would only injure herself and not succeed in dying. She finally threw herself from a fourth floor window. Often psychotics seemed to have tried to ensure that they were not discovered in time, one patient was found by chance in her gas-filled kitchen by a neighbour who did not usually call; another was only found because her daughter broke into her flat after she had not kept an appointment with a friend.

One of the items of information we detailed for every patient was the source of the idea that they should have psychiatric treatment; whether it was the husband, the woman herself, other relatives and friends, or what we called 'official' sources. The latter were probation officers, welfare officers, health visitors, the police, and doctors if the woman had not gone to them to consult about her mental state but if they had themselves noticed it and initiated the suggestion that she should seek psychiatric treatment. There were significantly more neurotics for whom the proposal had been 'officially' initiated – 22 per cent as compared with 9 per cent of the psychotics. Among the neurotic suicides as many as 73 per cent were referred by 'official' sources, while only one of the psychotic suicides was. And indeed she was very much the exception who proved the rule; she was one of the rare psychotics with a past loss by separation (there were only four) and she was not 'retarded'. Her husband, whom she suspected of infidelity, was away and she took some tablets and brandy. When they failed to have an effect she telephoned the hotel where he was staying and told him she would not be there when he arrived home. The police were informed, and came to see how she was; her cousin was summoned and then stayed one night. After her husband returned, she was taking him some tea in the morning and when he said 'thank you darling' it was too much for her: she ran straight out into the road, trying to get run over. It was only because an ambulance driver was passing that she was taken to a psychiatric department.

Referral by such 'official' sources is not only linked with what we have called the 'protest' theme among those with a suicide attempt.

Among the patients as a whole, 15 per cent had such referral; but 47 per cent of those with a past loss by separation did so and only 10 per cent of the rest. Twenty-six per cent of the officially referred neurotics were violent (as compared with 13 per cent of the rest of the neurotics). Among the psychotics with such a source of referral, however, none were violent, but the rate of retardation was lower than for the total psychotic population – 33 per cent as compared with 73 per cent. This perhaps reflects the other side of the same coin: retardation, being more akin to conventional notions of sickness, will provoke relatives and friends to suggest treatment, whereas a patient less retarded, more in the phase of protest, may be considered troublesome rather than sick by her normal contacts and only recognized as in need of psychiatric attention by someone with some expertise such as 'official' personnel.

The picture, which begins to emerge of those with past loss by separation, is in many ways consistent with the data now emerging in the neighbouring field of the study of delinquency and conduct disorders in children. Numerous studies have shown an association between a broken home in childhood and the development of anti-social problems (e.g. Bowlby 1969; Rutter 1971 and 1972). There is evidence too that broken homes are associated with attempted suicide in adult life, especially for those with personality disorder (Greer, 1964; Greer and Gunn, 1966). Frommer and O'Shea (1973a and b) found that women whose parents had separated before they were eleven had more feeding and management problems with their first babies than others, whereas loss of a parent by death at that age was not related to these aspects of infant care. Our data were insufficiently detailed to throw light on the important debate that Michael Rutter has opened up in his comprehensive review of maternal deprivation: is the experience of separation as such important or is it the discord in the home, with which such separations are highly associated, which produces the characteristic 'protest' symptomatology? Rutter and Madge's conclusion in favour of the second view (1976: 205–8) is persuasive but they are discussing the development of non-depressive antisocial disorders. In the case of depression the element of loss itself is likely to be of more crucial importance than it is for delinquency; discord alone might therefore not be expected to produce the same cognitive set of 'rejection' which we have hypothesized stems from separation. An examination of the case notes of those neurotic patients with symptoms of violence, suicide attempt, or official referral, who had not had a past loss by separation provided evidence of definite childhood experiences of rejection in five of the ten protocols; evidence of discord without such rejection appeared in only one. More work is needed to clarify

this issue, but it seems plausible to suggest that it may be the very combination of the two elements, loss and tension in the home which is important in producing neurotic depressions in later life, rather than that either one or the other is crucial on its own.

It is of interest in the light of this argument about the effects of past loss that the *recent* events provoking the depressive episode under study did not relate to the type or severity of the woman's depression. It may be remembered that about three-quarters of provoking events among the patients involved a clear-cut loss. Psychotic patients were no more or less likely to have had such a recent clear-cut loss than neurotic patients. (And if age is allowed for they were only a little more likely to have suffered a loss by death in the year.) Moreover, a woman who had had a recent loss by death was no more likely to have had a past loss brought about by death. The lack of an association between the types of past and recent loss is just what would be expected if early established cognitive schemes influenced reactions to later severe events. For a woman who has earlier lost an important person by death, the emigration of her child may be seen to have death-like qualities.

One would also expect some kind of primacy effect on the way past losses influence symptom formation. If, for instance, the first loss is by death, a person will tend to see all other losses in these terms. It is possible that losses by separation would in time attenuate or even reverse the original perspective; and, of course, since we have thoughts about the past, the perspective that is important need not be formed at the time of the loss. David Copperfield lost a father before he was born:

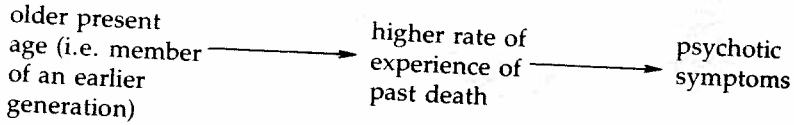
> 'My father's eyes had closed upon the world six months when mine opened on it. There is something strange to me, even now, in the reflection that he never saw me; and something stranger yet in the shadowy remembrance that I have of my first childish associations with his white gravestone in the churchyard, and of the indefinable compassion I used to feel for it lying out alone there in the dark night.'

The measure of past loss we have used is still crude and the assumptions made about primacy will need to be tested on larger numbers. For instance, loss by death was rated for the experience of one woman whose mother died in her first year and whose father, after a struggle to cope, sent her to foster parents at the age of two; they in turn returned her at the age of seven at her father's remarriage. This experience could with equal plausibility be considered as loss by separation. It is also, of course, important to extend the work by considering a wider range of experience. What, for instance, about war-time evacu-

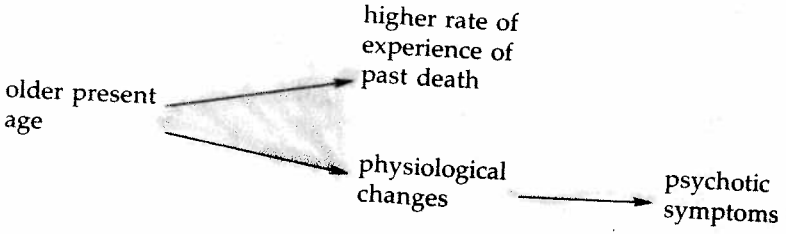
ation, periods in hospital of less than one year, broken engagements, and so on?

Age and previous episodes of depression

We have already discussed a striking feature of psychotic as compared with neurotic depressed patients, namely that they are older. We have so far suggested no reason for this; it has, of course, usually been considered to be a purely physiological effect but it is by no means absurd to consider that environmental factors might also be implicated. Women in Camberwell who were fifty to sixty-five years old were much more likely to have experienced past loss than those aged twenty to thirty-five. One possible explanation is that the older women had had more time to lose husband and children; but since the majority of the past losses occurred before eighteen and the findings concerning past loss held whatever the age at loss, this can be ruled out (see *Tables 4* and *5*, chapter 14). It is more tempting to interpret this age difference as, at least in part, the result of a generational effect; improved diet, medical care, smaller families, and lack of major wars have meant that the younger generation experience far fewer deaths of close relatives in their childhood. This would be more consistent with the model:



than with:



If this is true we might be experiencing a generational change in the form taken by depression – psychotic-like conditions becoming less frequent. While there is some suggestion that this may be occurring (Paykel, Klerman, and Prusoff, 1970) and that there may be secular changes taking place in the form of psychiatric disorder (Hare, 1974),

there are obvious alternative explanations. Since there can at present be no direct test in the absence of evidence for a physiological link, the possibility must remain speculative. Once the present cohort of twenty to thirty-five-year-olds are fifty or over we can expect to have a clearer picture. Insofar as other kinds of major loss may be increasing, particularly from marital breakdown, there is another reason to expect a secular change in the form of depression.

We confess that we have no satisfactory interpretation for the association of a previous episode with overall severity (there is no association with the psychotic-neurotic distinction). It is of interest that it only related significantly among women without a past loss, which might be seen as suggesting some form of priority for the role of past loss. But the possibilities are too many to conclude anything other than that more research is required.

Symptom formation in the general population

At this stage we had done much better than we had dared to hope in tackling the question of symptom formation among patients treated by psychiatrists. We had entirely explained (at least in a statistical sense) the differences in overall severity between out-patients and in-patients in terms of past loss, previous episode, and severe events occurring after onset. We had further shown a remarkable association between psychotic and neurotic forms of depression in terms of type of past loss. However, when we turn to the thirty-seven women who developed depression in Camberwell we must largely report failure. We were handicapped from the start by our omission to collect information about previous episode for the whole 458 women. But leaving this aside we did not find anything to explain either differences in severity among the cases or between them and the patient series. For example, past loss did not relate to differences in overall severity among onset cases. Nor were we able to find more than a slight tendency for onset cases with 'psychotic-like' symptoms to have had a past loss through death.[12]

While in no way wishing to sidestep these negative results nor their possible implications, we do not deem them as necessarily inconsistent with the patient results. Past loss is an extremely crude measure. It is almost certain that it is not *any* past loss that is significant but loss under particular circumstances – say in terms of the kind of mourning that was possible. It might well be that in practice only a small proportion of past losses are capable of having an effect on the expression of symptoms. It follows that if a group is selected in terms of the dependent variable at issue, that is severity, by definition the subgroup of

past losses will be selected that are effective in influencing severity. This is just what we have done by taking the patient group. Because they are patients, they are on average more severely depressed than cases in the general population.[13] Under these circumstances our inability to repeat our results on an unselected random sample of depressed women and therefore with an unselected group of past losses is understandable. It may well only prove possible to repeat the result on a random sample when we have identified just what aspects of past loss increase severity and influence psychotic or neurotic features.[14]

There is a second possible reason for our inability to replicate the patient results in the community series. Even if our measure of past loss had been more refined, results in the general population might have been less clear as a result of another form of selection bias. We will spell out this possibility because it has interesting implications: patients when compared with general population cases will be selected not just for overall clinical severity but for other features relevant to psychiatric referral. We saw earlier how much higher the rate of 'official' referral was among 'neurotics' than among 'psychotics' and one can speculate that this may indicate that 'neurotics' in psychiatric treatment are even less representative of community cases with depressions of more neurotic type than are psychotics in psychiatric treatment of community cases with depression of the more psychotic type. Since 'official' referral was closely associated with symptoms of protest, suicidal attempts, and past loss by separation, it is not surprising to find a much smaller proportion of the community cases than of patients characterized as 'neurotic' exhibiting these 'acting-out' symptoms. Most 'neurotics' in the general community would be identified by the absence of specifically psychotic symptoms such as retardation, or early waking or by their lesser severity. In the community, therefore, one would expect results where the neurotic depressions were related more to a *lack* of past loss by death rather than to the *presence* of a past loss by separation. A very large initial sample would thus be required to include a fair number of this particular sub-group of 'acting-out neurotics', without which the results for community cases could not be expected to replicate those for patients.

These speculations about treatment-selection factors set the results on the association of type of past loss with the psychotic-neurotic distinction in a particular perspective. We prefer to emphasize the association of past death with the diagnosis of psychotic depression, seeing it, in part, as a confirmation of Aubrey Lewis's thesis that the two-fold diagnostic distinction is really a dimension of severity. Despite the fact that our measure of overall severity was only moderately associated with this distinction, we agree with his conclusion, emphasizing the relevant concept of severity as more akin to Engel's notion of 'giving up-given up' than to the simple idea of intensity of any pattern of symptoms which underlay our measure of overall severity (see Ní Bhrolcháin, Brown, and Harris, 1977). In this respect we place particular emphasis on retardation. This symptom and several measures of severity of symptomatology are not only the factors that relate most closely to past loss by death but are also the best discriminators between the two diagnostic groups. In these terms neurotic symptoms reflect the lesser degree to which the patient has withdrawn. Thus the association between past separation and the diagnosis of neurotic depression can be seen within the same perspective. For just as a separation is less final than a death, so neurotic patients stop short of the depth of depression of psychotic patients whether they have 'acting-out' symptoms or not.

16 A model of depression

In the last chapter we outlined a theory that accounts for the results presented in chapters 6 to 14. In diagrammatic form model and theory are shown in *Figure 1*.

We will not repeat the arguments but further comments may help to clear up ambiguities – or at least set limits to them.

The model does not refer explicitly to personality dispositions. We decided that any attempt in our cross-sectional survey to distinguish personality traits from psychiatric disorder was unrealistic and we made no attempt to measure them. As academic sociologists, for understandable reasons, we gave priority to external factors such as social relationships and employment. But nonetheless cognitive set plays a critical part in our theory, and in psychological theory this would rank as an enduring personality feature. Although in the diagram separate headings have been given for 'cognitive set', 'response', and 'psychiatric disorder' it should be clear that we see cognitive set as merging into the emotional responses to events and these reactions blending with symptoms. This applies to our view of personality as a whole; while for some purposes personality traits are fittingly treated as 'background variables', it is quite in order in other circumstances to treat exaggerated traits as 'symptoms'. It is exactly this transitional position that we allot to self-esteem; as a background factor, low self-esteem can both predispose a person to a depressive reaction and, when exaggerated, become a prominent feature of the depressive disorder itself.

The model and theory are more a summary of what has emerged from our work than a claim to account completely for depressive phenomena. Nevertheless we feel the results justify considerable attention to the model, limited as it is, in the handling of depression.

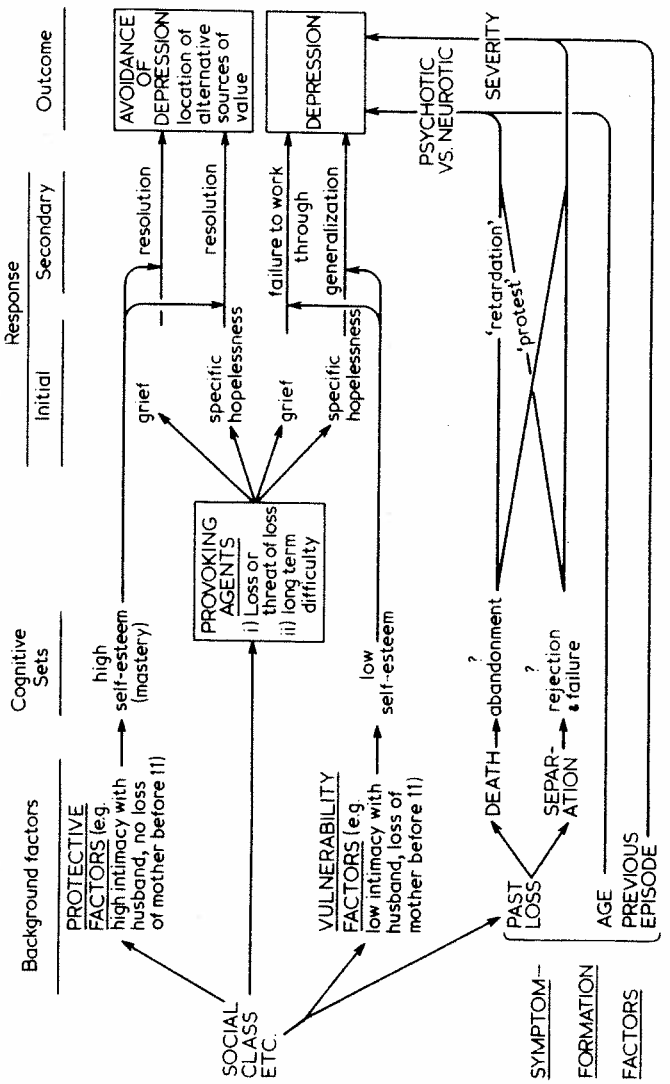


Figure 1 Schematic outline of causal model (capitals) and theoretical interpretations (small type)

For if the role of social factors is as clear as suggested by our material, attention to a person's environment may turn out to be at least as effective as physical treatment. All the same there is always likely to be a small group of depressed women for whom relevant social factors cannot be identified.

The model may be considered to beg the question of the direction of causality between self-esteem and the four vulnerability factors; causality could work either way. Women low on 'mastery', for example, might tend to choose a life-style that does not involve them in work outside the home or might fail to take action to restrict their family size; their low self-esteem may have predated the 'vulnerability factor' rather than resulted from it. Women low on self-esteem might well choose to marry men with whom it is difficult to have a confiding relationship just because they feel they will never find anyone more suitable who is willing to stay with them. Other personality features may mean that certain women find it difficult to maintain good personal relationships, even with the equable and reliable men they have married. Obviously such influences could act in both ways and be mutually reinforcing. But the question of such temporal priority should not be confused with a choice between 'social' and 'non-social'. Aspects of personality will often be the result of earlier social experience. The question is better formulated in terms of the contribution of the current environment relative to all other influences, leaving open the question of whether their source is social or non-social. In these terms the Camberwell material is enough to discredit any easy rejection of the current environment as a major influence. We have noted, for instance, the drop in 'intimacy' between wife and husband once a working-class woman has children, suggesting the importance of current influences. Equally suggestive is the finding that working-class women are only at increased risk of developing depression when they have children at home (although working-class women are more likely to have chronic conditions in all life-stages). Enough is already known about the organization of working-class family life, especially concerning the division of work and patterns of communication between husband and wife, to be confident that a good deal of the class differences in the vulnerability factors must be due to long-term cultural influences. While intensive work, ideally on a longitudinal basis, is required, it would be foolhardy meanwhile, in both scientific and practical terms, to underplay the role of the current environment – acting, we have suggested, particularly through limitations of role identities and of opportunities to find new sources of positive value. There is, in any case, the possibility that even if a woman is at risk as a result of long-standing 'personality' traits, changes in the current

environment could still lessen this risk. And, of course, a strong case can be made that the higher rate of severe events and major difficulties among working-class women with children and the fact that their difficulties are of longer duration is in a large part a feature of their environment. In most instances they seemed to have been drawn into troubles not directly of their own making and about which they were relatively helpless.

Susceptibility to depression

There remain the patients who developed depression without a provoking agent – twenty-eight of the 114 – and the possibility of a fourth factor to explain such exceptions. It has been commonly argued that quite trivial stimuli can at times produce illness. While we have defined vulnerability only in terms of sensitivity to major troubles and crises, it is more usual for the term to be used to describe development of a condition with the minimum of provocation. We will use the term susceptibility to describe this tendency and thereby clearly distinguish it from vulnerability. Some of the most interesting current ideas about depression emphasize such susceptibility. Aaron Beck discusses what he calls the vulnerability of the depression-prone person as

> 'attributable to the constellation of enduring negative attitudes about himself, about the world, and about his future. Even though these attitudes (or concepts) may not be prominent or even discernable at a given time, they persist in a latent state like an explosive charge ready to be detonated by an appropriate set of conditions. Once activated, these concepts dominate the person's thinking and lead to the typical depressive symptomatology.'
> (Beck, 1967: 277)

Although somewhat akin to our notion of symptom-formation factors, they are clearly different as they are considered to increase the likelihood of depression. He goes on to suggest that the depressive-prone person has become sensitized in childhood and adolescence to certain types of life-situation – these are responsible for establishing the original negative attitudes and are the prototypes of specific stresses, which may later activate these constellations and lead to depression. He indicates that the incidents that set off the feelings and in turn depression are usually quite minor – at least in terms of the conception of threat developed in the Camberwell research. It is perhaps surprising that we have found little need to discuss such susceptibility; perhaps the nearest we have come is in terms of minor threatening events 'bringing home' the implications of a major problem. But, of

course, this does not mean that the idea is irrelevant. The twenty-eight patients who did not have a provoking agent – at least as defined by us – are obvious candidates for this kind of susceptibility. And, indeed, the majority did experience something that might well have 'triggered' the depressive disorder – the woman who, for instance, had been told she had 'incurable' arthritis.

There is, in fact, evidence we have not so far mentioned that offers some support for this notion of susceptibility. Past loss, previous episode, and age of forty or over were related to overall severity at admission. We now add that the presence of all three (but not less then three) is strongly related to *absence of a provoking agent*.[1] Since the index relates to overall severity of depression, the intuitive link which many readers will have made between the notion of susceptibility and severity is supported. If necessary, room could be made in the model for a fourth factor: we would tentatively place it in the lower half of the diagram which shows the connections between these three background factors and severity of depression.

Some of the patients therefore appear to have been 'susceptible' in this sense. There are two obvious interpretations of the concept. First that some women have had their threshold lowered by a previous episode or past loss and so do not need a current loss or disappointment in order to reexperience feelings of hopelessness. Moody (1946) has reported on a patient who while in the army had been bound by a rope with his arms behind his back. He managed to escape with bound wrists and before his recapture he spent several hours with his arms tightly bound. A number of years later in the course of abreactive therapy under narcosis he developed weals with a clear pattern of rope marks around his arms. The phenomenon was observed twice and once under conditions of close scrutiny. There may in depression be some kind of 'induced sensitivity' that makes subsequent episodes more likely along the lines of this kind of 'body memory'.

A second interpretation is 'over-reaction' to particular stimuli – perhaps along the lines argued by Aaron Beck. It is of interest here that among the patients without a provoking agent those highest on this index of past loss, previous episode, and age had a smaller proportion with a 'minor' event in the three weeks before onset – 13 per cent (2/16) versus 42 per cent (5/12). The difference, however, does not reach statistical significance and its interpretation must in any case be uncertain. While the fact that those high on the index had so few minor events *could* be seen as a sign of their extreme susceptibility, it might equally be argued that they should have been more likely to succumb to minor events. The only other evidence we can produce is also unclear (though not without interest). It will be recalled that among the

severe events some were considered to be more threatening than others. And yet there is no sign of any interaction between either susceptibility scores or vulnerability scores and degree of threat; that is, marked events were no more or no less likely than moderate events to have occurred to those who were more or less susceptible (or more or less vulnerable). That there is no association is somewhat surprising, although it is consistent with the general tenor of the results that the provoking agents act in a unitary manner. It is not as though moderate events less often provoked depression among the less vulnerable or less susceptible women than did marked events. But while provoking agents (either marked or moderate) were more often absent among the more susceptible women, this effect was not related to their vulnerability in the sense we have defined.

It is therefore best to end on a note of caution. While susceptibility almost certainly exists, it is at present a theoretical rag-bag – it is impossible to rule out a role for a whole range of bodily and environmental factors; and it is not unlikely that many factors will be found to be at work in such a residual group. The patients without provoking agents were too few for us to do much more than underline it as a theme that will need at some time to be incorporated into our model.

17 Summary and conclusions

Sociological studies have two broad purposes. One is to chart the impact of societies upon their members; a second is to study the societies themselves in terms of the impact of their institutions upon each other and how these relate to the impact on individuals (Boskoff, 1971). The study of psychiatric disorders can fulfill both functions. On the one hand it seeks to answer the question of whether particular political, economic, and family structures influence the rate of disorder through their impact on the individual. On the other hand knowledge about rates of psychiatric disorders can further understanding of the workings of the social systems in which they occur. In this connection depression is particularly relevant: it is not only relatively common, but it is fundamentally related to social values since it arises in a context of hopelessness consequent upon the loss of important sources of reward or positive value. A woman's own social milieu and the broader social structure are critical because they influence the way in which she *thinks* about the world and thus the extent of this hopelessness; they determine what is valued, as well as what is lost and how often, and what resources she has to face the loss.

Psychiatric disorder is common among working-class women in London but not in a rural population in Scotland. The disorders almost all involve depression or anxiety and in the urban setting, at least, are predominantly depressive. However working-class women were only at higher risk of developing depression in the year of our study when they had children at home. In explaining such findings we have viewed clinical depression largely as a social phenomenon and have developed a model which in terms of the presence and absence of three factors explains a good deal about the aetiology of *all* forms of depression. The *provoking agents* influence when the depression occurs, the

vulnerability factors whether these agents will have an effect, and the *symptom-formation* factors the severity and form of the depressive disorder itself. The model tells us only that in some way the factors are causally linked to the disorder. It does not tell us how or why. It is important to know that a woman who has a confiding relationship, particularly with a husband, has much less chance of developing depression once a provoking agent occurs; but it is possible that the reason might have little to do with confiding as such but with, say, the way she is able to think about the marriage and value it. If this were so, confiding could only be used in the model, at least in London, because it happened to be highly correlated with aspects of marriage that were protective. We have therefore also attempted to interpret the model theoretically. What is it about confiding that is important? This distinction between creating model and theory is made for the sake of exposition. They are in practice highly interrelated activities. We have, for instance, done our best to include in the model factors that we believe are of theoretical relevance. In this sense the model is best seen as an early and relatively crude theory; it enables us to get some direct sense of what is going on and provides a framework in which we can think and test new ideas. The distinction between model and theory is, however, important in the sense that the investigator can include certain factors in the model without knowing how it is they work, so long as there is no reason to believe that their association with other factors is the result of some artefact. While it is desirable to have theoretically interpretable factors, all that is required is that they should be methodologically acceptable. In our own model we have variables that range from those about whose theoretical status we feel fairly confident (e.g. life-events) to others about which we know little (e.g. age).

In the design of the study itself we were concerned to increase as much as possible the generality of our findings. With this in mind we studied several samples of depressed women – women treated by psychiatrists for depression as out-patients and in-patients, women with depression, seen by general practitioners and women found to be suffering from affective disorders in Camberwell in 1969/71 and in 1974/75 and in the Outer Hebrides in 1975. It is helpful to bear in mind that for the task of describing the distribution of psychiatric disorder we have relied on the population surveys of Camberwell and the Outer Hebrides and for work with the model we have used the various groups of depressed women, employing each to check on the others. These independent checks of the model have given remarkably similar results with two exceptions. Not all the vulnerability factors were important for the patient groups. We have not seen this as casting

doubt on the model but as reflecting the influence of an additional set of factors influencing who receives psychiatric treatment. These factors have not been incorporated into the model itself as they do not strictly concern aetiological issues. Nonetheless we give them considerable importance, not least because some of the vulnerability factors in the model also appear to influence whether treatment is received. They do so, however, in a reverse direction. That is, experiences that increase risk of depression then act, once a woman is depressed, to reduce her chances of receiving psychiatric care. A pointer to this was the finding that, in Camberwell, women with children at home were both more likely to develop depression and less likely to see a general practitioner about it. This explains why some of the vulnerability factors are not found to occur more commonly in the patient group. The second exception concerns the symptom-formation factors. These are extremely influential among the patients but much less so among the comparatively less disturbed women not receiving psychiatric care. As yet we are uncertain about the significance of this exception.

To give the results in more detail: 15 per cent of the women in Camberwell were considered to be suffering from a definite affective disorder in the three months before we saw them. About half were *onset cases* who had developed a disorder in the year before interview and practically all were depressed. In contrast *chronic cases* had been disturbed more or less continuously for more than a year. All the *cases* had experienced symptoms of a severity which, if they were to have presented themselves for psychiatric out-patient treatment in the Camberwell area, would have been sufficient, in our judgment, for them to be considered as suffering from a psychiatric disorder and to have been given treatment. An additional 18 per cent of the women were suffering from a *borderline case* condition – that is from definite psychiatric symptoms which were not considered to be severe enough to rate as cases. The ratings of *cases* and *borderline cases* were reliable, although there is clearly an arbitrary element in distinguishing the two groups. In neither were we concerned with distress, dissatisfaction, and unhappiness, although these states undoubtedly overlap a good deal with the conditions we have studied.

Our main task, the development of a causal model, could not be done without close attention to methodological issues. Only in this way could a reasonable claim be made that the factors in the model were *in some way* involved in bringing about depression. Obvious biases had to be ruled out and objections that would trivialize the model met. Measurement can be viewed in terms of meeting methodological demands (is a causal interpretation possible?) or in

terms of accuracy (is the world like this?). We have been critical of a good deal of measurement in the social sciences because it manages to do badly on both counts – reliance on the narrow, though superficially attractive, technology surrounding 'standardized questionnaires' produces results about which there can be no confidence either that a causal link is present or that anything meaningful about a person's life is reflected in the data. We have devoted four chapters to questions of measurement and a summary will not be attempted here. We believe that enough has been done to rule out possible sources of bias for the claims we make about causal processes to be taken seriously. However, a general comment about our approach may be helpful. Our argument that it is change in *thought* about the world that is crucial has an awkward corollary: what is perceived by X as a change will not necessarily be seen as such by Y. In a study of children handicapped after poliomyelitis – surely a major life-event – Fred Davis notes that:

> 'despite the obvious and sometimes abrupt changes in the family's scheme of life occasioned by the child's handicap, it was remarkable how little conscious or explicit awareness of such changes the families demonstrated. Each time a parent was interviewed, he was asked whether anyone in the family felt or acted differently toward the handicapped, whether the child acted or felt differently about himself. Almost invariably, although sometimes after a puzzled silence, the answer came back that nothing had changed . . . Were it not for the parent's incidental remarks and unreflecting reports on specific events and situations, one might not have surmised that there had been any significant alteration in their lives.' (1963: 162)

It is clear we must accept that what is going on in a person's life, a person's perception of this, and the way change and perception of change are reported by him may all differ. The question therefore arises whose perspective do we take about life-events and the changes they entail? Is any perspective more true than another? Much will depend on what we are trying to do but it is difficult to contemplate any way of dealing with such multiple perspectives without the investigator at some stage imposing his *own* viewpoint on the world. He must use his judgment not only for methodological reasons (discussed in earlier chapters) but because the world is capable of having an impact irrespective of the meanings a person brings it. A widow of forty-five with three children faces a different world, if she wishes to remarry, than one with no children; and the meaning of her widowhood will often only slowly emerge as she faces these contingencies.

In our own research we recorded what women said they felt about incidents (including our assessment of any feelings spontaneously

expressed at other points in the interview). But for methodological reasons we have placed most weight on 'contextual' ratings which were designed to record what most women would have felt given the particular circumstances – past and present – of the individual woman. But while we did this primarily for methodological reasons we also believe such judgments on the part of the investigator are essential for social science research. It is unlikely that a situation can be satisfactorily described relying only on what a person is ready and able to state about it at a particular point in time. At a minimum the investigator must take account of feelings expressed in the interview (but not necessarily recognized or freely discussed by the person) and his judgment about the situation as a whole. Our approach in no way detracts from the importance of a person's experience of his or her world – indeed it is just this that is central to our theoretical ideas about the aetiology of depression. Further, we are fully aware that we have not exhausted ways of looking at what we have studied. In future work, for example, we need to look in far more detail at the way women think about and experience their depression. It is important to know, for instance, how many women describe characteristic symptoms such as slowness and lethargy in apparently moral terms such as laziness. In our work we have attempted to provide the foundation for such studies.

We began developing the model by studying the role of life-events, building on earlier research with schizophrenic patients. There are substantial causal effects in all five main samples of depressed women – that is among in-patients, out-patients, general practitioner patients, and the first and second Camberwell samples. It was not just any life-event however unpleasant that could bring about depression. Only certain *severe* events involving long-term threat were capable of doing so. Nineteen per cent of the women in Camberwell who were not cases had at least one such event in the nine months before interview compared with 61 per cent of the out-patients and in-patients in a comparable period before onset. Since some of the latter would be expected to have had an event that was not involved in bringing about the depression, we applied a correction formula that made allowances for this. This showed that the proportion of patients with a severe event of causal importance was 49 per cent. Using the same correction for onset cases in the general community gave a figure of 53 per cent. The distinctive feature of the great majority of the provoking events is the experience of loss or disappointment, if this is defined broadly to include threat of or actual separation from a key figure, an unpleasant revelation about someone close, a life-threatening illness to a close relative, a major material loss or general

disappointment or threat of them, and miscellaneous crises such as being made redundant after a long period of steady employment. In more general terms the loss or disappointment could concern a person or object, a role, or an idea.

There has been a good deal of debate about what it is about life-events that plays an aetiological role in illness. Some have argued that change itself is enough (e.g. Dohrenwend, 1973; Rahe, 1969) but probably most have emphasized the importance of the meaning of events (e.g. Lazarus, 1966; Mechanic, 1962). The Camberwell research gives an unequivocal answer – at least for depression. Change in itself is of no importance – everything turns on the meaningfulness of events. Furthermore, events involving only short-term threat although they could bring considerable emotional torment (a child nearly dying), did not contribute to the onset of depression, at least in the sense of increasing the proportion of depressed women who were involved in a causal effect. It was only events involving long-term threat, lasting at least a week but usually a great deal longer, that were significant. No other aspect of events was found to be significant once such a threat had been taken into account. Detailed measures of changes in routine and in social contacts were unrelated to risk of depression – that is the proportion of events shown to be involved in a causal effect could not be increased over and above that obtained for severe events alone when these aspects were taken into account. For change to be important it had to be associated with long-term threat which in turn usually involved loss and disappointment.

Earlier we reviewed studies that have looked at this issue; the work of Paykel and his colleagues in New Haven comes closest to meeting methodological requirements and is in general fully consistent with these results.

A good deal of previous research has assumed that the effect of life-events is additive – that two events are more likely to bring about illness than one, and three more than two. This notion seems to derive from emphasis on the importance of change rather than the meaningfulness of events. It is easy to visualize a change of residence and a change of job as summating in their effect; less easy when it is known that the job was eagerly wanted and the house-move regretted and resented. To our knowledge the question of additivity has not been systematically tested. Indeed we had a good deal of difficulty in doing so – in our case because our measure of threat of a particular event took account of other relevant events. In rating that a woman had learnt she was pregnant, we would consider not only the event itself but the fact that her husband had not long left her. In terms of this example, since the separation from husband had been taken into account in the rating

of the pregnancy, 'adding' the two events at the stage of analysis was probably superfluous.

Fortunately, there were also plenty of 'unrelated' events where this kind of overlap did not occur (death of a brother and a son being sent to prison) which could be used to test for an additive effect. This showed that while there was some evidence for an effect, it involved only a handful of the patients and none of the community cases. But at this point it is important to be clear that all we had done was to demonstrate that additivity was of little significance for events having no meaningful implications for each other.

In addition to events, ongoing difficulties were common in Camberwell and certain of them were capable of playing an aetiological role. We have called them *major* difficulties; all were markedly unpleasant, had lasted at least two years, and involved problems other than health. If major difficulties are considered with severe events, using the same correction formula, the proportion of women with depression having *either* one *or* the other of causal importance increases from 49 to 61 per cent for patients and from 52 to 83 per cent for onset cases. Difficulties, although of lesser importance than events, do play a definite aetiological role in depression.

More than one major difficulty did not increase risk of depression; nor was there an increased risk when they occurred with a severe event. It is as though it is only the meaning of *particular* losses and disappointments that bring about depression. The point made earlier when discussing the additivity of events still holds: events and also difficulties often act together to produce depression but only *when they have meaningful implications for each other*. Because of the way we had measured threat we could only demonstrate this in a series of special analyses. Pregnancy and birth, for example, were associated with a greater risk of depression but it was only in the context of an ongoing difficulty, particularly bad housing or poor marriage, that risk was increased. Other examples hinged on the way women adjusted to long-term adversity. Quite minor events could at times bring about depression if they served to 'bring home' the implications of some ongoing major loss or disappointment. A woman in Camberwell, for instance, living in bad housing became depressed four weeks after her sister's engagement. Another example was the failure of long-term health difficulties, no matter how serious, to bring about depression. It was only a crisis stemming from them – a husband's stroke, for example – that did so. Indeed it almost seemed as if without such a crisis the act of caring for someone gave women a sense of purpose which might even have reduced risk of depression. In summary, a special class of event, often quite minor, could force a reassessment of the meaning

and purpose of life. Adaptation and accommodation to a major loss or disappointment might go on for months, if not years, perhaps with some elements of denial and then an event, sometimes quite trivial, would 'break through' to underline the hopelessness of the position. It had been impossible to demonstrate this role of minor events in the main analysis because they, for the most part, *only* acted in the presence of a major difficulty or severe event; they could not contribute to the estimate of proportion of events and difficulties involved in a causal effect as their role was subsidiary to more obviously threatening events and difficulties.

These severe events and major difficulties are the *provoking agents* of our model. But it was insufficient to establish a causal role for such events. It was also necessary to establish just what kind of cause was involved. It was still possible to assert, for example, that our findings were of little fundamental significance as the events for the most part merely triggered a depressive disorder about to occur in any case. Fortunately a clear answer could be given for events and, by implication, for the role of major difficulties.

The 'brought forward time' index (described in chapter 7) enabled us to show that the role of severe events is formative in the sense that they bring about a condition that would not have occurred for a long period or, more likely, not at all without them. The picture for schizophrenia is different. Here a wide range of events can bring about a sudden onset of florid symptoms. The arousal of *any* strong emotion, positive or negative, appears to be enough. Events probably bring about a disorder that would have occurred before long in any case. Results for schizophrenia therefore come closer to the view that it is change itself that is important – although even here it would certainly be misguided to underplay the role of meaning. Indeed, we suspect, though cannot demonstrate, that many of the events involved in bringing about schizophrenia have a particular symbolic significance for the patient, which is often far from obvious. The basic conclusion must surely be that different aspects of events will be significant in the aetiology of different conditions. Measurement has to be flexible enough to demonstrate (not just assume) what these are. Depression and schizophrenia are unlikely to represent the only possibilities – for instance, while streptococcal infections of the throat apparently respond to much the same pattern of factors as schizophrenia there is some hint that diabetes, hypertension, and myocardial infarction as well as peptic ulcers are unduly common among those having close responsibility for the lives of other people, such as anaesthetists and air traffic controllers (e.g. Cobb and Rose, 1973). Also of relevance is the fact that severe events and major difficulties were significant in the

onset of all *types* of depression, including psychotic and neurotic forms. The regrouping of patients according to the presence of a provoking agent (i.e. as 'reactive' or 'endogenous') did not reveal different clinical syndromes associated with the presence of such an agent. There was a slight hint that a sub-group of depression might exist which was without a provoking agent and which also had a high chance of having one or two of the classic somatic symptoms of endogenous depression – early waking, and appetite loss. There was also a suggestion that severely threatening events which did not involve actual loss might be associated with a marked anxiety component in the depressive episode. But at best these were small tendencies – the over-riding conclusion must be that the presence, type, and frequency of provoking agents did not relate to the form or severity of depression – essentially the same findings held for in-patients, out-patients, patients seen by general practitioners, and those seen by no one.

These findings have at a number of points been anticipated in the research literature. A more challenging claim of the model is that a provoking agent is rarely sufficient on its own to bring about depression – although it does largely determine when a disorder occurs. The need for a second kind of aetiological factor in addition to a provoking agent is illustrated by the way depression is linked to social class in Camberwell. Among those with children at home, working-class women were four times more likely to suffer from a definite psychiatric disorder (about one in five in the three months before we saw them compared with about one in twenty of comparable middle-class women). They were also a good deal more likely to experience a provoking agent. And yet surprisingly the latter explained relatively little of the class difference in depression. This can be seen in the way working-class women with children were four times more likely than similar middle-class women to develop a depressive disorder *when they had a provoking agent*. It was this greater vulnerability once an event or difficulty had occurred that had to be explained if the class differences in incidence of depression were to be understood.

It was clear that other factors, also related to social class, must be at work, and we therefore looked for the *vulnerability* factors of our model. We found that if a woman does not have an intimate tie, someone she can trust and confide in, particularly a husband or boyfriend, she is much more likely to break down in the presence of a severe event or major difficulty. Similarly she is more at risk if she has three or more children under fourteen at home and if she has lost her mother (but not father) before the age of eleven. None of these factors

are capable of producing depression on their own but each greatly increases risk in the presence of a provoking agent. When they are considered together the importance of yet another factor, employment, emerges. *Table* 3 in chapter 11 summarizes our findings. Women with a confiding tie with a husband or boyfriend are protected in the presence of a provoking agent whether or not any of the other three vulnerability factors are present; although risk is not negligible – one in ten with a provoking agent compared with one in 100 without. Where such a tie is lacking, risk increases when there is a provoking agent. It is greatest for those with one or more vulnerability factors in addition to lack of a confiding relationship; of particular note is the way employment halves the risk of depression among those with a provoking agent but without a confiding tie with husband or boyfriend. It was the fact that the working-class women had more of these vulnerability factors that explained most of the class differences in risk of depression. This also explains why the class differences in risk are restricted to women with children.

In summary: some of the social class difference in risk of depression is due to the fact that working-class women experience more severe life-events and major difficulties, especially when they have children; problems concerning housing, finance, husband, and child (excluding those involving health) are particularly important. Incidents of this kind are the only kind of severe event to occur more commonly among working-class women and are the most obvious candidates for the 'inner city' stresses which are the focus of much current social commentary. But most of the class difference in depression is due to the greater likelihood of a working-class woman having one or more of the four vulnerability factors and not to their greater risk of experiencing a provoking agent.

So far we have considered risk of developing depression in the year. When we consider chronic conditions the lot of working-class women is, if anything, even worse. A disconcerting number were chronic cases and this may prove to be one of the most significant findings of the Camberwell survey. In keeping with this, all forms of marked long-term difficulty were more common among working-class women and physical health problems were particularly common among those with a chronic case or borderline case condition. We have made, we believe, a reasonable prima facie case that some of the difficulties, particularly concerning housing, had served to perpetuate the psychiatric conditions, but because the onset of these conditions had preceded the year of investigation we could not definitely establish that the causal link was only in this direction; we could not rule out the possibility that the depression had also served to perpetuate the dif-

ficulty; and, of course, both processes could be at work even in the same person.

There was less depression in the rural community in the Outer Hebrides than in Camberwell. Unfortunately we did not document the rate of particular types of events and difficulties on the island and thus explore the idea that urban stresses are of a different quality. Nonetheless it is important that there were no class differences in prevalence of psychiatric disorder and no tendency for women with children to be at increased risk. These differences from Camberwell could result from the greater degree of overall protection their culture and society give these women but this has still to be documented in detail.

The comparative material raises a more general point. The Camberwell results would not necessarily be expected to occur in other urban communities or even elsewhere in London. Replication will depend on how far the conditions outlined in the model are associated with class and, even if they are, whether they have the same significance. This is the point of theory. The significance of something like going out to work is bound to depend on the broader social setting. Confirmation of the Camberwell results will therefore depend on tests of the theory as well as the model. It will be necessary to show that going out to work *does* have the same significance in different settings: and this, of course, will depend on the theoretical interpretations of the factors in the model.

In addition to the 15 per cent with a definite psychiatric disorder we have already noted that almost one in five of the women in Camberwell suffered from a borderline condition. Overall these were unrelated to class or life-stage, although chronic states were somewhat more common among working-class women. Onset of borderline conditions was strongly linked to the same provoking agents that bring about case conditions; but those who developed only a borderline condition in response to a severe event or major difficulty were more protected in terms of the four vulnerability factors. Depression in this sense is not an all or nothing matter; women are more or less successful in warding off a clear-cut depressive disorder according to the degree to which they are protected.

The frequency of borderline conditions raises the general issue of the nature of the phenomena we have recorded. The commonness of case conditions among working-class women is startling. We are fairly convinced about two things: first, that the conditions we have classified as cases are comparable in terms of severity with many of the affective disorders treated by psychiatrists at out-patient level, although we do not suggest that the distribution of severity among them exactly duplicates that of the treated conditions. They do, however, fall well within the range of conditions seen and treated by psychiatrists. Second, we are convinced that many of the borderline case conditions (as well as the cases) attend general practice surgeries and probably receive the bulk of the large quantities of psychotropic drugs prescribed there. A series of research and practical issues arise at this point about which we are much less clear. Although cases appear to be closely similar to those seen in psychiatric out-patient clinics there may well be important differences – at least for some of them. Obvious possibilities concern duration and fluctuation in the severity of the condition (although it must be remembered that half the cases had experienced much the same level of symptomatology for well over a year). A further possibility concerns differences in coping: there may well be differences both between and within the groups in reaching a decision to seek treatment either from a general practitioner or a psychiatric department. There are three obvious ways in which this might occur. First, there may be treatment barriers such as having young children. Second, the women may give different explanations of their psychiatric state. As far as we could tell (although we did not examine this systematically) the majority saw their social environment as of crucial importance – under these circumstances a woman may well be less motivated to seek psychiatric help. Third, there will almost certainly be personality factors. No one who has interviewed women at random in a general population survey can be anything but impressed by the determination of some to carry on with their lives as though little was wrong in spite of an intolerable burden of symptoms.

But even if there were some tendency for treated and untreated conditions to differ, it would still be essential to study a full spectrum of affective states for the phenomenon actually seen by psychiatrists to be understood. The issue of who is selected into psychiatric care needs to loom far larger in psychiatric research and theory (see Mechanic and Newton, 1965). That there is selection is clear: in Camberwell, for example, women receiving psychiatric care are no more likely to have children than women in the population in general and only a little more likely to be working-class. The implications of such selection may prove to be revolutionary not only where it is treatment-seeking but some other form of self-selection which has defined the target sample. For instance, it will have occurred to many readers that one of Durkheim's main findings – often replicated – runs directly counter to our own (Veevers, 1973). He found that the highest suicide rate is among childless wives and not single women and that married women with children were still lower. His interpretation is well known:

'. . . in itself, conjugal society is harmful to woman and aggravates

her tendency to suicide. If most wives have, nevertheless, seemed to enjoy a coefficient of preservation, this is because childless households are the exception and consequently the presence of children in most cases corrects and reduces the evil effects of marriage.' (Durkheim, 1952: 189)

He goes on to suggest that children offer women greater protection than men because 'women profit more from children' and are 'more sensitive to their happy influence'. But it is possible that far from having a 'happy influence' the presence of children may merely determine whether suicide is or is not the outcome of an unhappiness common to all married women, whether or not they have children. A number of women in our sample told us that the only thing that prevented them from harming themselves was the need to care for their children. Similarly we found that women with three or more children at home, although as depressed as other women, were less likely to have consulted a general practitioner about their depression.

The parallel suggested between the effects of these two types ot selection, treatment-seeking and suicidal behaviour, highlights the danger of drawing conclusions about suicide. Statistics concerning suicide are usually criticized for being unreliable, but as important a deficiency is their selectivity even if completely accurate: it is not clear what those who commit suicide can be said to represent. Although it is clear they are for the most part miserable people it will not do to assume that they can be used to represent all miserable people in the community and to use them to draw conclusions about the distribution of misery in the population at large. We are so impressed by this possible pitfall that we begin to doubt the usefulness of much social and clinical research with psychiatric patients – at least with the more common conditions – without parallel investigations of those *not* receiving psychiatric care (or not, for example, committing suicide). The effects of selection are likely to be so pervasive that it is not unreasonable to ask for evidence that findings are not the result of selective processes. One area where this is particularly relevant is the study of the impact of early loss of parent; the long history of inconclusive, inconsistent, and often negative findings has probably been due to the fact that early loss of a mother not only increases risk of depression but also relates to factors that make psychiatric treatment less likely.

But we still have not dealt fully with the implications of the high rate of psychiatric disorder among working-class women. Most of the women with a case or borderline case condition suffered considerable

distress and there is thus undoubtedly a large overlap between our psychiatric categories and more traditional notions of happiness and dissatisfaction. Four in ten of the working-class women with a child at home expressed either considerable dissatisfaction with their marriage or notably little warmth or enthusiasm about it. Two-thirds of these women were cases or borderline cases compared with only 17 per cent of the rest of the women. Does this suggest that what we have been discussing is so common that it cannot be considered a medical phenomenon; being part of daily life it is just a particularly unpleasant form of unhappiness? To some extent this question confuses two separate issues: first, the mere frequency of a condition is not logically related to its medical or non-medical status. The frequency of hookworm infection in certain communities does not prevent it from remaining a sign of ill-health. Second, the overlap between unhappiness and depression does not mean that they are not logically distinct concepts. We did not pursue the issue of unhappiness not because it seemed less important or because a strictly clinical approach was in some way superior; we did not do so simply because it would have required measures of greater sensitivity and theoretical clarity than we possessed. To measure happiness it is necessary to be able to establish in some detail just what a woman values. We had neither the time to develop such measures nor to collect such material during the interview. As Donne suggests 'All men call *Misery, Misery,* but *Happinesse* changes the name, by the taste of man'.[1]

In future research we visualize taking the middle ground, looking both at psychiatric and more general concepts. Meanwhile the effect on the woman and her family of case, and even borderline case conditions, should not be dismissed. When psychiatrists call such states 'minor' affective disorders they refer to a comparison with major depressive illnesses, perhaps with severe retardation and delusions, not to the 'minor' amount of suffering involved or their impact on others. Personally and socially the impact can be serious, although the symptoms themselves may not rate as serious in psychiatric terms. To illustrate: as part of the life-events interview we collected details of any accident to those at home that required treatment by a casualty department or general practitioner: they included fractures, severe cuts, burns, and one death. Both social class *and* psychiatric disorder are independently related to accidents occurring to children under fifteen in the twelve months of the survey. While 9.5 per cent of the 420 children of the women with such a child at home had an accident, 19.2 per cent of the children of working-class women with a case or borderline case condition had one, 9.6 per cent of other working-class women, 5.3 per cent of middle-class women with a case

or borderline condition, and only 1.5 per cent of the rest of middle-class women. The accidents were not restricted to the younger children. If anything, they were more common among the older children. That a direct causal link is involved is suggested by the fact that the rate of accidents to children among women with borderline and case conditions during periods in the year *before* the onset of their symptoms was no different from that among the children of other women.[2] The point for the present argument is that the association of accidents with psychiatric state is almost as high for borderline case as for case conditions. The full personal and social cost of such conditions is still to be documented, but we probably already know enough to see them as a major social problem.

So far we have described the two factors in our model concerned with the risk of developing a depressive condition. Some headway was also made in understanding what influences the type and severity of symptoms once a woman is depressed. This involves the third set of *symptom-formation* factors. We have in fact described two effects. One way we tried to overcome the complexity of the different characteristics of depression was to make a judgment about overall severity, taking number of symptoms, severity of individual symptoms, and degree of interference with routine and employment into account. When this was done, as might be expected, in-patients were on the whole more severely depressed than out-patients and it was this difference between the treatment groups that, in the first instance, we set out to explain. At onset in-patients and out-patients did not differ in severity; differences in severity emerged only some time after onset. A third of the patients had one or more marked worsenings of their depression which could be dated to a matter of a few days (change-points) and the entire difference in severity between the two treatment groups was associated with these changes – in-patients had more of them and when they had them they tended to deteriorate to a greater extent.

We discovered three variables that explained, at least in statistical terms, the difference in severity of the two treatment groups. First, severe events occurring *after* first onset were capable of producing a subsequent change in condition – almost as though a new 'onset' was superimposed on the existing depression. But more important were *past losses*. These were by and large losses of the immediate family in childhood and adolescence (parents and siblings) although we also included loss of children and death of a husband in adulthood if these were more than two years before onset. There is a small overlap with the vulnerability factors here as loss of mother before eleven also increases risk of depression in the presence of a provoking agent. But

the overlap is not great. Loss of mother before eleven forms only about a fifth of the total experience of past loss in Camberwell and it is *any* past loss that increases severity of depressive symptoms. The results so far concern the patients but they can be extended to the general population in the sense that the amount of past loss was lower amongst women developing borderline case conditions than among cases; and the difference between case or borderline conditions seems best conceived in terms of severity. However when cases in Camberwell are considered there is *no* association between severity of disorder among them and past loss. While this may be partly due to the relatively small number of women in this group who were highly disturbed, it is more likely that future research will need to pinpoint special characteristics of a subgroup of past losses, such as unsatisfactory substitute care, which are particularly effective in influencing depression in later life.

Finally, experience of a previous episode of depression, for the patient series at least, was associated with greater severity. When considered together the three variables (that is, a severe event after onset, past loss, and a previous episode) entirely explained the difference in severity of depression between in-patients and out-patients.

But there is a second aspect of symptom formation – the classic *psychotic/neurotic division* made on the basis of the presence or absence of symptoms such as early waking and diurnal variation. The distinction has been central to much psychiatric research on depression during the last fifty years and requires separate study from overall severity in the sense that, as measured by us, the two were only moderately correlated. Much the most important factor relating to the diagnostic distinction was again past loss, but now viewed in a new way according to type of past loss. Psychotic depressive conditions were highly related to past loss by *death* and neurotic depressive conditions to past loss by *separation* – by, for example, divorce of parents or adoption of a child. Moreover when diagnosis *and* severity were considered, the latter was related to the presence of past deaths within the psychotic group and to past separation within the neurotic group. The associations of diagnosis with type of past loss are large and were not explained by factors such as age and previous episode. Furthermore the results were almost exactly repeated on a separate series of patients classified as psychotic and neurotic by another research psychiatrist.

We have now summarized the three factors of the model. In depression the role of the provoking agents is more than that of mere triggers of a condition whose aetiology lies largely elsewhere and we attribute its onset, with all its physiological components, primarily to experi-

ences of loss and disappointment, particularly those involving the woman's view of her own identity. The range of these experiences is far broader than the bereavements held to be associated with depression. Bereavements, in fact, formed only about a tenth of the severe events (and an even smaller proportion of the provoking agents as a whole). We have therefore extended the notion of loss to include not only that of a person in ways other than death but also loss of a role and loss of an idea. This is all fairly clear from our material but from this point our theory becomes more speculative. The feelings of hopelessness, which to a greater or lesser degree follow a major loss and disappointment, will on occasion become generalized to form the central triad of the depressive experience described by Aaron Beck as the feeling that the self is worthless, the future hopeless, and the world meaningless. In arithmetical terms this occurs to one in five of the women with a provoking agent – only this proportion develop clinical depression. In our view this is because the generalization of the feelings of hopelessness has not occurred in the other four of the five. In order to explain this we have suggested that the role of the four vulnerability factors is both to lower the ongoing sense of self-esteem well before the onset of depression and thus to potentiate this generalization of hopelessness in the context of a provoking agent. Without one or more of the vulnerability factors a woman might well be able to work through the experience of loss and disappointment and find new sources of positive value, thus keeping her experience of depression within the bounds of a 'borderline case' or more often of normal grief, simple sadness, or distress.

The role of self-esteem can be seen both in a passive and in an active sense. So far, by implication at least, we have emphasized a passive interpretation – that a woman develops profound hopelessness or alternatively finds it too painful to work through her grief because of the feelings of low self-regard she brings to the loss. But the more active possibility should not be overlooked. If a woman has three or more children under fourteen living at home or has no employment outside the home, she is less able to move into new areas of activity or to make new contacts on which she can build new sources of value. The role of isolation in depression is also suggested by the ability of an intimate relationship to reduce risk of disorder. While such a relationship is likely to help provide some women with a basic sense of self-worth it also has its more active aspect. The availability of a confidant, a person to whom one can reveal one's weaknesses without risk of rebuff and thus further loss of self-esteem, may act as a buttress against the total evaporation of feelings of self-worth following a major loss or disappointment. Furthermore, the ability to talk with someone

about one's feelings is a safe-guard against some sort of blanket defence mechanism of denial preventing the working through of grief. The particular importance of the intimate relationship being with a man raises questions about the value of sexuality (or at any rate some sort of physical intimacy perhaps less sexual than nurturant), but these cannot be answered without more detailed research. A sexual aspect is probably not essential for an intimate relationship to be protective; much is bound to depend on what is expected by the woman herself.

Our own interpretation of the husband as a confidant is that with the current organization of the family in urban centres, he is usually the main source of a woman's sense of achievement. It will be remembered that the risk of depression was greatest among working-class women with children at home, for whom a sense of achievement would be most closely related to their roles as mother and wife. She needs to be 'told' she is doing well to refurbish her sense of self-worth. Mary Boulton in an intensive study of women with children, in fifty London families, has described the extensive influence a husband can have on a woman's experience as a mother. A husband's appreciation is a reward in itself as well as supporting a sense of accomplishment in the way she deals with day-to-day problems of child care. His acceptance that there are difficulties and frustrations legitimizes the experiences of pervasive irritation and unhappiness felt by many women about the routine care of children and helps them to accept that this does not reflect on themselves. In general his support helps a woman to create a rewarding idea of herself. Boulton also documents that working-class husbands are more likely to fail to recognize the difficulties and frustrations of child care. More see it as an 'easy' and 'cushy' job thereby trivializing the work and lowering their wife's sense of self-worth (Boulton, 1977).

The fourth vulnerability factor, loss of mother before eleven, we see as working in a number of ways. First, that it may have a crucial impact, in social terms, on the course of a girl's life: she may leave school earlier, make a hurried marriage, have her first child at an early age, fail to find satisfying work when she first leaves school just because she has no mother; and all this will mean that later in life she has fewer alternatives available to her when faced with loss. Second, we emphasize the impact of loss of mother before eleven on the woman's internal resources, her psychological strength, and optimism. On the one hand it is likely to have left her with a permanently lowered sense of her own worth. In this second sense it acts as the three other vulnerability factors in terms of ongoing low self-esteem. Third, we believe the early loss may not have been 'worked through' and a later loss is therefore more likely to revive traces of the earlier

experience which intensifies the current one along the lines of a reverberating circuit; of course earlier maladaptive defense mechanisms may also be revived.

It is through this final process that we see the second and third factors of the model as overlapping, that is, loss of mother before eleven is included in the more general category of past loss, where it acts not as a vulnerability factor but as a factor contributing to symptom-formation. In order to explain the effect of past loss on symptom formation we have postulated an intervening cognitive set which not only influences the severity but also the type of symptoms. Crucial aspects of this cognitive set are its fatalism and the related degree of protest. Evidence concerning individual symptoms among depressed patients such as violent acting out behaviour and retardation suggests that such a set may be responsible for the association of past deaths with retarded depression where the patient has 'given up', in Engel's sense, more completely than in the neurotic gesture of despair. Past loss of any type can, according to this theory, influence the cognitive set and thus act as a symptom-formation factor.

The role of severe events in producing further 'onsets' in an established depression is a special instance of the role of provoking agents and while contributing to severity is probably best seen separately from the other symptom-formation factors. But it does remind us of the importance of taking into account the course of a depressive episode and of not looking at symptoms only at first onset or at the point of admission.

Emphasis in our theory has thus been upon the psychosocial aspects of loss, particularly upon the appraisal of implications of event or difficulty by the woman herself. But the model also leaves room for other factors which we have not been able to identify with any clarity. These may be other psycho-social factors or physiological ones. Indeed one of the symptom-formation factors, a previous episode, is theoretically ambiguous in this sense; at this stage it is impossible to say whether it acts upon the cognitive set, perhaps making it easier for a woman to give up more quickly just because she has done so before, or whether it is merely a sign that the woman has always been physiologically susceptible to depression or had become so as a consequence of earlier depression. The latter possibilities would probably work largely in a 'mechanical' way, independently of her appraisal, to increase the severity of the disorder once it develops. Age is similarly ambiguous. Though it plays no role as a provoking agent or vulnerability factor, it is related to the type of depression once onset has occurred, older women being more likely to have psychotic-type depressions. While there are undoubted physiological accompaniments to the aging process, a psychosocial impact cannot be entirely ignored, as the literature on the 'mid-life crisis' reminds one. Butler (1968) has speculated on the 'universal occurrence in older people of an inner experience or mental process of reviewing one's life' and believes that it can contribute to clinical depression.[3]

Leaving aside the actual processes involved, there is also room in our model for another concept, that of susceptibility. This putative fourth factor, unlike the three others of the model, implies that a woman is generally susceptible to depression (in the limiting case developing it spontaneously); but also that once depressed she will develop a more 'severe' depression. At present the notion of susceptibility has very little by way of supporting evidence although something of the kind seems highly likely. While leaving room for it in the model, we do not see it necessarily as acting only in the absence of loss; on the contrary, the analysis of the twenty-eight patients without a severe event or major difficulty suggests that *some* form of loss precedes almost all instances of depression. One important area to explore in future is just such patients without severe events or major difficulties before onset. Research might also profit from a special selection of patients with hallucinations and delusions. Under a tenth of our patients had hallucinations and the figure was only a little higher for delusions of any kind. In view of the rarity of these and of pathological guilt among the general population cases, there is much to be said for seeking out such symptoms in future work. Meanwhile it is reasonable to rest some faith in these results that have emerged from a sample of patients unselected for anything except onset within the last year. They suggest that social factors play a formative role in the majority of onsets of depression at all treatment levels and with diverse symptom pictures and that they also enhance both vulnerability to loss events and severity of the condition once it occurs; that the processes by which they have this impact work through the person's appraisal of their meaning for her and her life plans; and that the somatic symptoms of depression are the sequelae of the basic cognitive appraisal. The results as a whole focus attention on the importance of attachment theory for the understanding of depressive phenomena. But this does not mean, of course, that they just have implications in terms of individual psychology. The social class differences have much wider implications. The possibility that there exists a special group of 'inner-city stresses' with attendant inner-city disorders sets the results firmly in the context of the discussion of 'urban crisis'.

One point raised by these broader factors is the significance of the accelerating and unbalanced outflow of population from the centre of London – particularly of the more skilled and mobile. How far has it

tended to leave a relatively more disadvantaged group of people in our inner cities? It is perhaps worth recalling that our two-fold social class division is arbitrary and that three more or less equal social categories give a progressive gradation of psychiatric disorder. Our discussion of working and middle class is no more than a way of speaking; the effect appears to be a straight forward function of 'standing in society' and, as we have seen, appears to be entirely explained by differences in the distribution of the provoking agents and vulnerability factors of our model. The question of population movement can be viewed in economic and material terms or in terms of some notion of personal inferiority. The latter theme, of course, has always been present when discussing the poor and disadvantaged. Given some of its unpalatable political implications, it seems pointless to spend time on the matter until the necessary research has been done. Quite straightforward comparative research would take us a long way. Would our findings hold in 'newer', less disadvantaged populations when individual women had been matched for the experience of comparable 'factors' in the model? *Table 3* in chapter 11 is again central; we suspect that, although the distribution of women among the various risk groups would differ, reflecting the overall greater advantages of a 'newer' population, the rates of psychiatric disorder *within* the various risk groups would be the same as for the women in Camberwell. This, after all, can already be seen when comparing class groups within Camberwell itself. While we are content to leave the matter for empirical enquiry it is difficult to deny that the implication of the model as a whole is that major improvements could be obtained by changes in the environment – sadly, however, well-intentioned intervention in terms of traditional 'social work' might not prove very helpful (Geismer *et al.*, 1972; Goldberg, 1973).

These general comments overlay an uncomfortable awareness of how much there is still to be done before the model can be said to have been satisfactorily linked to broader social processes. Sociology has always had its own brands of psychology although often disguised. We have, in effect, developed a social psychological theory of depression and at the same time made a reasonable case for the relevance of wider social processes, leaving more obscure than we like the details of this influence. Just how much, for instance, of the difficulty of women with young children stems from the circumstances of their task and how much from a sense of doing work that is undervalued in a society geared to reward through employment? How many of the marital difficulties associated with working-class life stem from the physical problems associated with rearing young children and how many from wider values linked to sex-roles? It is not particularly helpful to point

out that to some degree all are probably involved. While we know enough about family life in London to give some answers – for instance, about class differences in the amount of cooperation between husband and wife in child-rearing and household tasks – it would nonetheless at present be speculative to link them with our model (see Oakley, 1974; Ginsberg, 1976; and Boulton, 1977).

It is perhaps unnecessary to emphasize in any case just how many of the theoretical interpretations that we have used still wait to be firmly established. Further research can go in a number of directions. It can expand the time dimension by documenting earlier experiences more carefully, for example tracing long-term class differences in experience more systematically, particularly in ways of coping with crises in early life. Alternatively, work can document the present more sensitively – the things a woman has and would like to have that make her life meaningful and how these interact with the 'factors' in our model. This will involve establishing how far some women are more protected, not because they avoid fundamental crises, but because of the greater variety of their lives and a greater input of 'positive' experiences, which enable them to hope for better things more easily. The middle-class woman can more often travel, visit friends at some distance, or buy a new dress; she has perhaps greater confidence and skills in seeking out pleasurable experiences; and also a stronger belief that she will eventually achieve certain goals of importance. Adjustment in adversity may prove to be largely a matter of how to sustain hope for better things.

As well as provoking guidelines for further research, these results as they stand can provide the kernel of a radical critique of the broader cultural, political, and economic system and, more narrowly, of the role of the medical and other helping professions. They have implications that concern not only the optimum organization of the family but also the role of women in the wider economy and the values given these functions by the media and society at large. They also suggest that no single avenue of intervention is likely to provide the best answer for a condition with such complex aetiology. Once it can be accepted that in many cases a combination of chemotherapy and psychotherapy needs supplementing with social changes, such as work or regular meaningful activity outside the home, the role of medical and social agencies in the treatment of depression should emerge in a new perspective. Instead of competing for priority in explanation and treatment, each would contribute information and recommendations to the other, thus giving what would be truly psychosomatic attention to a troubled person instead of just treatment to a 'sick' woman or solace to a stricken mind. Under such circumstances

the issue as to whether depression was or was not an 'illness' would become a pointless linguistic wrangle as unimportant as the discussion as to whether water at 85°F is hot or not hot but merely warm. In the end in such debates the contestants usually abandon the dichotomous label in favour of a dimensional type of description, in this example the Fahrenheit scale. At the moment it is impossible to dismiss it as such, as a sterile controversy, without appearing to be cowardly evading important moral decisions which seem to flow from asserting or denying it to be 'illness' – the decision, for instance, about when a person requires attention or privacy or compulsory treatment of some kind. And yet it is important to come out into the open and say that in some sense it is a non-issue, because by focussing on that alone, discussion can often afford to miss the details of exactly those important moral and practical issues that underlay the original furor. These are issues that concern both the genesis of the condition, and so in some sense involve the problem of free will and responsibility, and its management and outcome, thus involving stigmatization and the distribution of social resources to relieve the genuine distress of those involved. When these issues are confronted directly, the vaguer issue of illness/non-illness loses its importance. Meanwhile although our results leave no doubt of the relevance of the notion of illness for at least some with a depressive disorder, the term should be used sparingly, for it is an easy trap in which clear thinking can be snared by dualistic fallacies.[4] For it is difficult to maintain that our borderline cases should not attend general practice surgeries just because their symptoms, not having passed the threshold of caseness, do not qualify them to be considered as ill.

The results of this study suggest that the understanding of untreated depressions can be of great help in the understanding of those that have reached the treatment setting, and thus that even severely disturbed in-patients might benefit if physical treatments were supplemented by what might be called social therapy designed to raise their sense of self-esteem and increase the alternative sources of value available to them in the long term. The number of depressed women not seen by psychiatrists is large and of those not seen, or, in effect, not given serious attention by their general practitioner, is not inconsiderable. This is one, but, of course, not the only reason why the issue of treatment must be considered side by side with the issue of prevention. The implication of these results for the latter can only be the need for wider social and political changes which would mean that fewer people experienced provoking agents and fewer people were vulnerable in the first place. These findings provide backing for many reforms in our current social organization, increases in the number of

nursery school places and the number of part-time employment opportunities for women being some obvious candidates. They point, too, to the large areas of loneliness and isolation which exist amid our so-called affluence, and to the important role they play in determining family health. To combat these, and to build a sense of mastery and self-esteem, which will render every member of the community more resilient to the buffets of experience, requires more than comforting talk in a surgery, although even this is all too often not available.

While these results show that things are not well there are no grounds whatsoever to suggest matters are getting *worse*. We just do not possess the necessary evidence about the past. If we were to speculate we suspect things have improved; certainly what little evidence we have from the two Camberwell surveys five years apart could be seen as some hint of this. But for an understanding of what is going on and what might be done we have no doubt that more must be known about the effect of particular social contexts on particular psychiatric disorders (and for that matter physical disorders). After all, 'integration' into the small-scale community in the Outer Hebrides is not, apparently, without cost in terms of psychiatric disorder other than depression. We must not, in any case, lose sight of the fact that the particular form of psychiatric problems is likely to change – both in time and space. Only theoretical understanding of what is going on can help in these circumstances.

We do not suggest withdrawal from these wider concerns. Research on psychiatric or medical disorders should not be divorced from moral and political awareness. Whether we like it or not, the finding that so many working-class women in Camberwell were psychiatrically disturbed is not a sterilized neutral fact. But, at the same time, if the problems are too broadly stated there will be a danger that little will be forthcoming in the way of worthwhile knowledge about both origins of depression and treatment and preventive strategies.

Future research will need to focus on the role of the immediate social context, on individuals and their households, and on how they get caught up in a crisis or difficulty, try to cope with it, and the resources they have for this. But at the same time the possibility of spelling out broader links must be pursued. There is a somewhat uneasy division of labour here. Both individual-orientated and society-orientated studies are required; both are part of sociology and both are essential. The need is for each to remember the other. It is too easy for the broader approach to ignore the complexities of the individual's immediate social milieu and for the more detailed approach to get lost in the intricacies of the individual personality.