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STRESSEUL LIFE EVENTS AS A CAUSE OF MINOR AFFECTIVE DISORDER: A  
THEORETICAL AND EMPIRICAL EVALUATION

By

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Thesis submitted for the  
Degree of Ph. D. to the  
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# DECLARATION

I hereby declare that this Thesis embodies the results of my own special work and that it has been composed by myself.

DAVID J. COOKE

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## SUMMARY

### The Review

Life events have long been thought to produce psychological disturbance. Systematic attempts to define this association began in the sixties. There are considerable difficulties inherent in this task, however, certain criteria for attributing causal significance have emerged and these are examined.

In the absence of experimentation, the validity of a causal proposition can be assessed in terms of four forms of validity, namely, statistical conclusion validity, internal validity, construct validity and external validity. Each form of validity is described and the life event literature is reviewed in relation to it. Certain principles of design and analysis, which would enhance the validity of any putative causal association, are presented.

This discussion of validity is followed by an analysis of the importance that additional variables have in respect to the simple link between life events and psychological disturbance. The range of variables, which have been implicated, is outlined. Theoretical and empirical advantages which may be derived from the consideration of additional variables are discussed.

Four hypotheses regarding the influence of additional variables are put forward and their implications described.

The empirical evidence in support of each of these hypotheses is critically reviewed. The work of George Brown and his colleagues is considered in detail.

The methodological and statistical difficulties inherent in corroborating these hypotheses is discussed. A mathematical model designed to aid the evaluation of each of these four hypotheses is described.

### The Study

The study has the general aim of applying the principles of design and analysis, which are discussed in the review, to the clarification of the relationships among life events, minor affective symptoms and additional variables. A systematic sample of the general population living in the West End of Glasgow was drawn. Four hundred and eight individuals were interviewed. There was a response rate of 77 per cent. Symptom and personality information was obtained principally by using self-report questionnaires. Information regarding the experience of stressful life events, social relationships, demographic and vulnerability factors was obtained by using semi-structured interviewing techniques.

There are significantly more females in the obtained sample. The adult age range is fully represented and the social class distribution is even. Comparison of the respondents with non-contacts and refusers on major demographic variables indicate few important differences.

A comparatively high rate of psychological disturbance is detected with the Zung Self-Rating Depression Scale. Responses on this scale and the Self-Rating Scale of Distress were subjected to Principal Component Analysis. Four clusters of symptoms emerge. These are interpreted and called 'anxiety-depression', 'cognitive-depression', 'vegetative-depression' and 'endogenous-depression'.

The reliability of life event ratings is assessed using inter-rater procedures and the Kappa coefficient. The quantitative but not the qualitative ratings, achieve acceptable levels of reliability. A weighted index of the degree of life stress experienced by the respondents called total life stress (T.L.S.) is derived and its association with each of the four syndromes is calculated.

A significant association is found between T.L.S. and 'anxiety-depression' in the total sample and the female sub-sample. No other significant associations are detected.

The relationships between six types of life events and each of the symptom clusters are examined. Undesirable life events correlate with 'anxiety-depression' alone. Entrance life events display a counter-intuitive relationship with 'endogenous-depression' in the male sub-sample. No other significant associations are detected. Idiographically sensitive measures do not dramatically improve the prediction of 'anxiety-depression'.

The influence which demographic and personality variables have on the

simple association between T.L.S. and 'anxiety-depression' is calculated. Respondents' sex has a synergistic influence. Age and Neuroticism predict 'anxiety-depression' in their own right.

The influence that seven measures, conceptually derived from the vulnerability factors of Brown and his colleagues, have on the simple link is assessed. Only the total number of vulnerability factors and the number of confidants available influence 'anxiety-depression'. There is no evidence in support of the vulnerability hypothesis.

The influence of social relationship variables is considered. The number of friends, the number of social interactions and the extent of confiding in relatives contribute to the prediction of 'anxiety-depression' in the total sample and the female sub-sample.

### The Discussion

The validity of the causal proposition that total life stress produces 'anxiety-depression' is discussed in the light of the four forms of validity. It was argued that a reasonable and plausible case can be made.

The influence of additional variables is considered. It is argued that they could enhance the prediction of 'anxiety-depression'. Reasons for the strong synergistic effect between T.L.S. and respondents' sex are explored.

The implications that the results have for treatment or prevention

were considered.

The failure to replicate specific results of Brown and his colleagues is analysed. Explanations in terms of differences in the samples and measures are examined. It is concluded that the results of the study, together with the evidence from the literature review, call into question the adequacy of the vulnerability hypothesis.



## INTRODUCTION

The hypothesis that stressful life events may produce psychological disturbance has a venerable history. Cooper and Shepherd (1972), in a historical review, indicated that during Graeco-Roman times certain authors attributed abnormal mental states to the experience of adverse life events. They indicated further that this view was apparent in early texts concerned with psychiatric etiology, including *Traite Medico-Philosophique* published by Pinel in 1801.

Nonetheless, although the view that stressful life events may produce psychological disturbance is venerable, it was not until the late sixties that studies of some scientific rigour became available. The approach of Holmes and Rahe (1967) to the scaling of everyday life events acted as a stimulus for systematic study in this area. Within the field of psychological medicine the paper by Brown and Birley (1968) on acute schizophrenic breakdown, and that by Paykel et al (1969) on depression, can be regarded as seminal. Writing only a decade later, Miller and Ingham (1979) suggested that there must be a thousand or more studies that have examined the apparent association between stressful life events and different forms of disorder.

To write an extensive yet comprehensible review of such a large and growing literature would be a difficult if not a futile exercise. The following review is therefore selective and has two principal aims. The first is to describe the criteria that can be applied aims. The first aim is to describe the criteria that can be applied to make

causal statements regarding the relationship between life events and psychological disturbance. The second aim is to describe and discuss the importance that additional variables have with regard to the simple association between life events and psychological disturbance.

# CHAPTER 1    ASSESSING THE VALIDITY OF THE CAUSAL ASSOCIATION BETWEEN LIFE EVENTS AND DEPRESSION

## INTRODUCTION

Most life event research is underpinned by the assumption that life events cause illness. Dohrenwend and Dohrenwend (1978) commenting on the life event literature argued, however, that '... the belief that life stress causes illness is based on faith bolstered by some scientific evidence.' (p7). There are considerable difficulties in improving the quality of scientific evidence in support of causation in the life event field. The identification and understanding of causal processes is traditionally pursued through the use of experimentation (Campbell and Stanley, 1966). The structure of experiments provides the interpretation of causal processes, with the manipulation of the independent variable being assumed to have caused the change in the dependent variable. Ethical and practical difficulties dictate that conventional experimental designs cannot be applied to the study of life events and therefore other methods and criteria are required for inferring causality.

Going beyond conventional experimental designs can lead to theoretical problems; the epistemology of causation has been described as being in '... a productive state of near chaos' (Cook and Campbell, 1979, p 10). No attempt will be made to tackle these problems, but rather the conventional epidemiological stance of pragmatic determinism will be adopted. Following Susser (1973) it

will be assumed that one thing can cause another, this belief being essential if theory is to lead to proposed action. Cook and Campbell (1979) adopted a similar pragmatic approach to causation in their discourse on the identification of causal relationships in non-experimental situation. They detailed the principal criteria that should be used in evaluating the validity of causal propositions. They distinguished amongst four types of validity i.e. statistical conclusion, internal, construct and external validity (definitions below) and indicated that each of these four forms of validity relate to specific questions faced by researchers in non-experimental situations. Firstly, do the putative cause and effect variables correlate? Secondly, given the presence of covariation is there a plausible causal relationship between the two variables? Thirdly, given a plausible causal relationship which theoretical constructs underpin the measured variables? Fourthly, how general is this relationship across different people, settings and times?

The four forms of validity relate to these practical problems, each will be defined and examined in turn. The general principles of design and analysis necessary to enhance each form of validity will be examined with regard to the specific problems of life event research.

## STATISTICAL CONCLUSION VALIDITY AND LIFE EVENT RESEARCH

The first step in assessing the plausibility of a causal link is the demonstration of covariation between the putative cause and effect variables. The evaluation of the statistical conclusion validity of a causal link is based on evidence of covariation. Cook and Campbell (1979) stated that statistical conclusion validity '...refers to inferences about whether it is reasonable to presume covariation given a specified level and the obtained variances.' (page 41). The degree of statistical conclusion validity is therefore related to the sensitivity of the study to permit reasonable statements about the presence or absence of covariation, the quality of the evidence supporting covariation, and the strength of covariation. These three areas of concern are critically linked to issues of experimental design and analysis. The major threats to statistical conclusion validity within the life event field will now be considered.

The ability to detect covariation is linked to the statistical power of analytic procedure used. The statistical power of a procedure may be defined as '.... the probability of rejecting  $H_0$  when it is in fact false.' (Siegal, 1956, p 10). Power is linked to sample size and the types of variable correlated as well as the general characteristics of the statistical test.

### Statistical Power and Sample Size

One of the principal factors that leads to difficulty in detecting covariation between the putative cause and effect variable is the use of small samples. Cohen and Cohen (1975) indicated that when the expected degree of covariation is small then the power of the statistical procedure must be increased by increasing sample size. Two sets of reviewers have indicated that the level of observed covariation is frequently low in the life event literature. Cochrane and Sobol (1980) suggested that about 10 per cent of the variance was shared, whereas Lin et al (1979) provided a rather more pessimistic estimate of between 4 and 8 per cent of the shared variance.

Given this low level of covariation it is clear that event studies require large sample sizes. The magnitude of required samples can be estimated by power analysis. Cohen and Cohen (1975), in their discussion of power analysis, suggested that the conventionally acceptable level of statistical power, analogous to the 5 per cent and 1 per cent conventions of statistical significance, should be established as 0.80. That is, there should be an 80 per cent probability of rejecting the null hypothesis when it is false. On this basis, to detect a population  $r = 0.30$  at the 1 per cent probability level (two-tailed) would require a sample of 124, whereas a population  $r = 0.20$  would require a sample of 286 (Cohen and Cohen, 1975, p 480).

Correlations in the range 0.2 to 0.3 are common in the life event

literature and these calculations highlight the need for large samples. Evidence of covariation has generally been provided by larger studies (e.g. Paykel et al, 1969; Brown and Harris, 1978a; Andrews et al, 1978a; Miller and Ingham, 1979) while some of the studies with smaller samples have failed to detect significant levels of co-variation (e.g. Hudgens et al, 1967; Schless et al, 1977).

### Dichotomous or Continuous Variables

Statistical power, and thus statistical conclusion validity, is diminished when the putative cause and effect variables are treated as dichotomies. Workers in the life event field frequently dichotomise variables that have been conceptualised or measured as continuous variables in order to provide descriptions of 'cases' (e.g. Brown et al, 1975 et seq.; Andrews et al, 1978a). This approach may lead to difficulties in causal inference (vide infra) but, in addition, it can lead to substantial loss of power.

Cohen and Cohen (1975) argued that reducing a graduated many valued scale to a two point scale results in a wilful loss of information. The magnitude of this loss of information can be substantial. To illustrate the strength of this effect, Cohen and Cohen (1975) indicated that '...median cuts of two normally distributed variables will result in an  $r$  squared between them ( $r^2$ ) which is only 40 per cent as large as  $r$  squared would be in the original variables' (p 300 footnote). Therefore, while nominalisation of quantitative variables may have merits in simplifying descriptive accounts of

data, it may have the unfortunate consequence of the investigator underestimating the level of covariation present.

#### Appropriate Statistical Tests

Certain statistical tests are intrinsically more powerful than others and the application of inappropriately weak statistical tests may lead to reduced statistical conclusion validity. Hays (1963) noted that non-parametric procedures '...require more evidence than parametric measures to yield comparable conclusions'. Rabkin and Struening (1976) and Lin et al (1979) argued that the majority of life event studies used comparatively weak analytic techniques based on the chi-squared statistics (e.g. Brown and Harris, 1978a; Andrews et al, 1978a; Jenkins, 1979).

Lin et al (1979) favoured the application of parametric correlational methods as they tend to be more powerful and they provide an estimate of the strength of the relationship (e.g. Garrity et al, 1977a; Kessler, 1979; Henderson et al, 1980; Miller and Ingham, 1979; Cooke and Greene, 1981). Trivial relationships may be found to be statistically significant in large samples and thus magnitude estimates are invaluable (Friedman, 1968; Rabkin and Struening, 1976). Tennant et al (1981) noted that Multiple Regression Analysis is a valuable tool in life event research as it allows the investigator to determine the relative importance of life event variables as compared to other antecedent variables. Cohen and Cohen (1975) favoured the application of Multiple Regression Analysis over the more common parametric procedures such as Analysis of Variance, because of its additional power.



### Violated Test Assumption

The additional power of parametric techniques may lead to increased statistical conclusion validity. However, Cook and Campbell (1979) warn that it may be diminished if the rather more rigorous assumptions underlying these statistical tests are violated. Parametric correlational methods are underpinned by assumptions of normally distributed scores, normal independent and homogenous errors, and arguably, interval scale properties. Parametric techniques, such as Multiple Regression Analysis and Pearson's correlation, appear to be robust to deviations from normal distributions and pure interval scales. Labovitz (1967; 1970), Burke (1953) and Borgatta (1968, 1970) demonstrated that given the weak assumption of a monotonic relationship between the measurement scale and the underlying psychological scale, the application of parametric significance test to ordinal data yielded few aberrations.

With regard to parametric measures of association, Havlicek and Patterson (1977) on reviewing the relevant literature, concluded that there is substantial evidence to support the use of Pearson's  $r$  correlation coefficients with non-normal samples. In addition, they applied Monte Carlo techniques to demonstrate that Pearson's  $r$  is robust to violations of the scaling assumption, the distribution assumption and combinations of these two violations. They concluded that, '...it appears that Pearson's  $r$  can be used in nearly all situations in which there is a need for a measure of relationship between two variables, regardless of the shape of the distribution of scores or the type of scales being used' (p 376).

Similar arguments have been applied to Multiple Regression Analysis, with Bohrenstedt and Carter (1971) arguing that '...when one has a variable which is measured at least at ordinal level, parametric statistics not only can but should be applied' (p 132).

With regard to the assumption of interval scales, the empirical work of Havlicek and Patterson (1977) suggested that it is not important with respect to Pearson's  $r$ . Gaito (1980) argued that mathematical statisticians do not consider the scaling assumption to be necessary for techniques such as Anova and he stated that, '...this requirement was merely a figment of the imagination of a number of psychologists because of a confusion of measurement theory and statistical theory. Statistical procedures do not require specific scale properties' (p 566). Perhaps this is just as well. As Bohrenstedt and Carter (1971) indicated true interval or ratio scales are rarely if ever seen in the social sciences.

Anova and Multiple Regression Analysis are underpinned by the assumption of normal, independent and homogeneous errors. Cook and Campbell (1979) argued that this is an important set of assumptions. Fortunately, however, conformity with these assumptions may be assessed by careful use of residual plots (Cook and Campbell, 1979; Chatterjee and Price, 1977).

In conclusion it may be argued that parametric techniques are robust to violations of their principal assumptions and conformity to other assumptions can be assessed. Statistical conclusion validity, therefore, is not likely to be diminished by their use, even in sub-

optimal conditions. Rather the inherent power of parametric techniques will generally lead to enhanced statistical conclusion validity.

#### Measurement Reliability

Poor measurement reliability can attenuate statistical conclusion validity because of the measurement instrument's inability to register true change. Unreliability acts to inflate the standard errors of estimates and these standard errors are crucial in determining differences amongst subjects (Cook and Campbell, 1979). Increasing the reliability of tests generally involves reduction of random error and various procedures to this end are relevant to the life event literature.

Brown (1974) analysed the problems involved in developing reliable life event ratings. Using the Schedule of Recent Experiences (S.R.E.) developed by Holmes and Rahe (1967) as an exemplar, he argued that vagueness in defining life events contributed to substantial random error variance. Brown (1974) indicated that random error occurs due to variability in response to an item such as 'change in health in family member'. Variability in a subject's interpretation of what constitutes a 'change in health', and who should be regarded as a 'family member', can lead to increased error variance and a concomitant lowering of reliability. Brown (1974) and Tennant et al (1981a) indicated that the reliability of life event measures can be enhanced by the use of more specific definitions. Paykel et al (1976) developing a life event inventory

incorporated a similar item but defined a 'major physical illness' as requiring hospitalisation, surgery or one month's absence from work and 'close family member' as a parent, sibling or other close person e.g. fiancé. Clearer specification tends to enhance reliability, although rigid application of this principle would tend to make life events schedules unwieldy and reliability may be reduced due to subject's fatigue.

Decisions regarding acceptable levels of reliability depend on the purpose of the research. Nunnally (1967) indicated that for basic research it is often wasteful to attempt to increase the reliability of scales beyond 0.80. Very little attenuation of correlations will occur with reliabilities of this magnitude and thus statistical conclusion validity can be maintained. Nunnally (1967) suggested that lower reliability figures may be acceptable in the earlier phases of research with the correction for attenuation being used to estimate the level of covariation. Cook and Campbell (1979) indicated, however, that this correction should be used with great care when reliability estimates were low.

Within the life event literature two types of reliability estimates on two types of procedure can be discerned (Steele et al, 1980; Tennant et al, 1981a). Reliability estimates have been obtained by test-retest procedures and by inter-informant agreement. They have been obtained for life event schedules such as the S.R.E. (Holmes and Rahe, 1967) and its derivatives and also for semi-structured interviewing procedures.

Steele et al (1980) indicated that test-retest results with the S.R.E. ranged dramatically from 0.26 (Thurlow, 1971) to 0.90 for a new version published by Rahe (1975). Johnson and Sarason (1978a) using an approach derived from the S.R.E. reported test-retest reliabilities in the 0.19 to 0.53 range for their measure of positive change and reliabilities in the 0.56 to 0.88 range for negative change.

Horowitz et al (1977) examined inter-informant reliability with non-patient married couples. The level of agreement was low for the less concrete items such as frequency of arguments, but in addition, the level of agreement was only 69 per cent for more concrete items such as the birth or adoption of a child.

Yagar et al (1981) assessed the level of inter-informant reliability using the S.R.E. with couples. They obtained low levels of agreement with Kappa taking the value of 0.35 for patients and 0.39 for non-patients. They contended that this low level of agreement called into question the value of self-report checklist approaches and they advocated the application of semi-structured interviews.

Hudgens et al (1970), using a semi-structured interview, found that the level of agreement between newly admitted psychiatric patients and their relatives was 75 per cent.

The semi-structured interviewing procedures developed by Brown and his colleagues in London, have probably received most attention with

regard to inter-informant reliability. Brown et al (1973a) reported an 81 per cent level of agreement on occurrence of events between recovered schizophrenics and their relatives. When the more severely threatening events were considered alone the level of agreement rose to 92 per cent.

Tennant et al (1979) demonstrated that the degree of long-term contextual threat entailed by an event could be rated reliably by workers functioning independently from Brown's research team. Tennant et al (1979), correcting for chance agreement, demonstrated inter-rater Kappa values in the range 0.7 to 0.9 for short and long-term contextual threat. Parry et al (1981) found concordance values of 0.84 and 0.81 for short and long-term threat respectively.

While the evidence for reliability is by no means complete, it would appear that satisfactory levels of reliability for life event measures can be achieved with interview techniques rather than self-report techniques. As Yager et al (1981) indicated '...interviews are likely to increase the subject's motivation and involvement, clarify ambiguous meaning of items and result in more accurate time framing of event occurrences' (p 347).

Reliability of the dependent or effect variable is also important. Most workers attempt to maximise the reliability of their dependent variable by capitalising on the established reliability of published instruments. Workers adopting this strategy include Brown et al (1975 et seq.) using the Present State Examination (P.S.E.), Henderson et al (1980) the P.S.E. and the Zung Depression Scale,

Andrews et al (1978a, 1978b) using the General Health Questionnaire and Surtees and Ingham (1980) the Hamilton Rating Scale of Depression. The reliability of these published instruments in the sense of internal reliability or alpha reliability, can be maximised by the use of Principal Component Analysis (Armor, 1969). This approach will be considered in relation to construct validity. In conclusion, it may be argued that reliability levels of the order 0.80 are perfectly satisfactory within the context of basic life event research. Statistical conclusion validity will be maintained at this level of reliability.

#### Random Heterogeneity of Respondents

Statistical conclusion validity will be reduced if sub-groups of respondents vary in the intensity of their response to life stress. Substantial variability in response will tend to inflate error variance and attenuate any substantive relationships. This threat can be countered, to some extent, by the use of homogeneous respondent populations. Such an approach involves a trade off of reduced external validity in favour of increased statistical conclusion validity. Brown (1975 et seq.) adopted this strategy and deliberately attempted to increase the chances of obtaining a significant result by concentrating on women, rather than men and women. Surtees and Ingham (1980) and Surtees (1980), achieved the same aim by restricting their sample to patients with a moderate to severe degree of depression.

Cook and Campbell (1979) argued that all research designs involve a balance between the four forms of validity. If external validity is

of some importance, however, the threat of respondent heterogeneity can be countered if reliable measurement of the critical respondent features is possible. If a reliable measurement is available then it may be used as a covariate to statistically equate the relevant groups. If the overall sample size is sufficient, analysis within sub-samples may increase statistical conclusion validity by reducing heterogeneity.

#### INTERNAL VALIDITY AND LIFE EVENT RESEARCH

Once the researcher has minimised the threats to statistical conclusion validity and has demonstrated that two variables covary to a significant degree, the next step in the evaluation of a causal statement entails the assessment of the internal validity of the relationship. Cook and Campbell (1979) defined internal validity as '...the validity with which statements can be made about whether there is a causal relationship from one variable to another in the form in which the variables were manipulated or measured' (p 38). Internal validity is concerned, therefore, with the extent to which relationships between the measured variables can be said to be causal and with the direction of causality between them.

#### Temporal Relationships Among Variables

The specification of causality in general, and the direction of causality in particular, usually depends heavily on information pertaining to the time sequence of the measured variables. Mahoney (1978) and Susser (1973), enumerating the principal meta-theoretical characteristics of cause-effect relationships, argued that cause and effect should occur with an appropriate degree of temporal



contiguity with the cause preceding the effects.

#### (1) Temporal Contiguity

The notion of temporal contiguity, or the interval between the cause and its effect, is not immediately useful within the context of life event research. Theories or hypotheses regarding life events are generally not articulated in sufficient detail to allow specification of the probable delay between the occurrence of events and the development or exacerbation of symptomatology. This problem may not be significant in other fields with, for example, the theories regarding the mechanisms of particular drugs determining whether a rapid or gradual change in symptomatology should be expected.

#### (2) Temporal Priority of Cause Over Effect

Superficially, delineating the temporal priority of cause and effect might appear less complex than specifying appropriate contiguity of cause and effect. Consideration of this problem, however, reveals it to be less than straightforward.

Most life event studies have considered the relationship between the events that occurred prior to the assessment interview and the presence or intensity of symptomatology measured at the interview (Ulenhuth and Paykel, 1973; Andrews et al, 1978a, 1978b; Miller and Ingham, 1979). Using this approach it may be argued that the putative effect follows the putative cause. Brown (1972), however, argued on logical grounds that only events that occurred before onset of the symptoms should be considered to have any potential

causal impact. The concept of onset, while superficially attractive, does suffer from limitation. These limitations will now be examined.

Brown and Birley (1968) introduced the concept of onset to the life event field in their work on acute schizophrenic breakdown. They distinguished between two forms of onset "(1) from 'normality' or non-schizophrenic to schizophrenic symptoms, (2) from mild to severe schizophrenic symptoms" (p 205). These definitions indicate that onset entails either a quantitative change or a marked qualitative change in a person's symptomatology. Brown and Birley (1968) studied patients who had clear and acute onset and it is likely that the notion of onset is useful for conditions that are marked by distinct qualitative changes. Unfortunately, however, marked qualitative changes are less frequent in depressive disorders. Indeed, some authors (e.g. Eysenck, 1970; Weckowicz 1973; Ingham and Miller, 1976; Williams et al, 1980) have contended that depressive disorders entail quantitative rather than qualitative changes from normal states. Brown and Harris (1978a) concurred with this view in that they defined 'caseness' as '...something we have hypothesised as not a categorical distinction but a continuum representing a dimension of some sort of severity of symptomatology' (p 583). Onset, generally transition into caseness, occurred at a defined but nonetheless arbitrary point on this dimension of severity '..there is evidently an arbitrary element in choosing a cut-off point between a case and a borderline (case)' (Brown and Harris; 1978a, p 229). The designation of particular events as either pre-onset or post-onset events depends on whether symptom severity reaches the

prescribed level and thus an arbitrary element enters into the designation.

While qualitative changes may be infrequent, it would also appear that distinct quantitative changes may also be infrequent. Susser (1973), Tennant et al (1981a) and Cranach et al (1981) argued that the retrospective assessment of the onset of mild affective disorders is very difficult because of the absence of clear change points.

Cranach et al (1981), in a detailed critique of the concept of an onset in psychiatry, argued that the critical problem in its definition is a failure to specify to which set or sub-set of symptoms onset refers. The symptoms of affective disorders are heterogeneous in quality (Blumenthal, 1971) and sub-sets of symptoms may occur at different times in the course of the disorder. Brenner (1979) argued, from data collected in a community setting, that depressed affect preceded the major somatic symptoms of depression by several months. Brown and Harris (1978a) did not distinguish between different types of depressive symptoms, but rather in the rating of onset took account of increases in all psychiatric symptomatology, not just those of depression (Camberwell Studies Life-Event Manual). To be consistent with the notion of temporal priority it would be necessary to consider the relationship among events and different symptom classes, a task which would be unreliable in retrospective interviewing.

From the empirical viewpoint there is evidence that post-onset

events influence intensity of symptomatology experienced by an individual. Surtees (1978) and Surtees and Ingham (1980) demonstrated, in a prospective study, that the effects of post-onset events summate with the effects of pre-onset events to increase the level of 'adversity' and level of symptomatology experienced by an individual. Thus post-onset events appear to have a causal effect in the sense that they influence intensity of symptomatology experienced by an individual.

In conclusion, it may be argued that, given the difficulties inherent in defining and retrospectively identifying the onsets of symptom constellations, it is practically and theoretically more sound to impose temporal priority by relating events to symptoms extant at interview. This approach has the disadvantage that the impact of events is likely to decay in proportion to the time between its occurrence and the interview (Surtees, 1978). However, this procedure ensures temporal priority.

#### Direction Of Influence

The internal validity of the causal statement could be diminished because of a failure to accurately specify the direction of causality between the measured variables. In simple terms psychological disturbance might cause the event rather than vice-versa. It is necessary, as Mahoney (1978) and Susser (1973) indicated, to demonstrate asymmetry in the relationship between putative cause and effect. That is, a change in the cause variable should 'produce' a change in the effect and not vice-versa. Most life event studies fail to attempt to counter this threat to

internal validity and it may be that insidious onset of psychological disturbance leads to an increase in event frequency.

Brown and Birley (1968), acknowledging that difficulties in identifying insidious onset might lead to this threat to internal validity, introduced the notion of 'illness-independent' events. 'Illness-independent' events are regarded as events that on logical grounds, are unlikely to have been caused by the insidious onset of psychological disturbance. The death of the subject's parent is unlikely to be a consequence of the subject's psychological disturbance and thus this would be regarded as an independent event. 'Loss of job' or 'marital separation', however, may come about because of the subject's disturbance and those are thus regarded as 'illness-dependent' events. The inclusion of 'illness-dependent' events in any analysis results in a breakdown of the necessary asymmetry in the relationship between cause and effect and thus this reduces the internal validity of the causal link.

Brown and Harris (1978a) noted that this approach, despite its advantages, has the disadvantage that events that are probably causal are discarded, with a concomitant lowering of the estimate of any causal effect.

The introduction of the notion of 'illness-independent' events is a useful way of imposing the necessary asymmetry on the event-syndrome link. As an approach, however, it is not without difficulties. Surtees (1978), in a prospective study of hospitalised depressives, demonstrated that he could predict the frequency of 'illness-

independent events recorded at follow up by using the level of prior depressive symptoms. This result might imply, as Miller and Ingham (1979) have indicated, that personality and social/life circumstances may influence the occurrence of both events and symptoms, and that the apparent causal link between events and depression represents a spurious correlation.

#### The Presence of 'Dose-Response' Characteristics

Susser (1973) in his exposition of causation in epidemiology, noted that '...a response proportional to dose is persuasive of a causal relationship' (p 155). A dose-response relationship, that is, a linear relationship, is one in which the rate of change in the response produced by the dose does not vary with the value of the dose or the response (Stolzenberg, 1980).

Weiss (1981) reiterated Susser's contention regarding the importance of dose-response relationships but he indicated that exceptions may occur. For example, the range of exposure to stress normally experienced by a group may be beyond the threshold needed to trigger symptoms, the likelihood of the symptoms being dependent on other factors and no longer on the intensity of the exposure. Thus, to facilitate the detection of a dose-response relationship it is necessary to study subjects who have experienced a range of stress levels. Studies based on subjects who are homogenous in terms of life event experience may fail to detect dose-response relationships.

A further situation in which a 'dose-response' relationship is not

apparent is when the risk of disease is greater with smaller quantities of a dose. Weiss (1981) indicated that small amounts of thyroid irradiation may produce an increased risk of nodular goitre and cancer, while higher amounts do not. This type of relationship is unlikely to hold in the life event field, where it is generally assumed that severe threat is necessary to increase the risk of disorder (e.g. Brown and Harris, 1978a; Tennant et al, 1981a). It is perhaps possible that under-stimulation may lead to depression.

The presence of a 'dose-response' relationship is important in epidemiology in general. Within the life event field Paykel (1978) and Tennant et al (1981a) have argued that a linear event syndrome association strengthens the case for a causal link. Brown and Harris (1978a), however, argued that their data did not support a dose-response relationship in that the experience of more than one markedly threatening event did not significantly increase the risk of depression. Paykel (1978), re-analysing the data of Brown et al (1975), qualified this contention. He demonstrated that the relative risk of depression increased with the degree of threat entailed by the events experienced. While most investigators do not examine their data for linear trends, two general population studies using continuous dependent variables rather than trichotomous caseness measures (Brown and Harris, 1978a) have demonstrated the presence of linear relationships between events and severity of depression (Miller and Ingham, 1979; Cooke, 1981a). The use of continuous scales tend to facilitate the demonstration of dose-response relationships, and the presence of such a relationship tends to enhance the internal validity of the causal statement.

### Excluding Third Variable Explanations

Cook and Campbell (1979) indicated that the evaluation of internal validity principally entails determination of the extent to which explanation of the covariation in terms of spurious correlations can be ruled out. That is, it involves deciding whether the cause and effect variables appear to covary merely because they are both related to some third variable.

Brown (1974) published a seminal paper on the problems of ruling out 'third variable' explanations or threats. This discussion of the strategies of data collection and analysis that are useful in ruling out these threats leans heavily on his principles.

Brown (1974) identified three general ways in which an additional variable might reduce the internal validity of any causal statement. He described these as 'direct contamination', 'indirect contamination' and 'spuriousness'. Each general form of threat will be considered in turn and possible counter strategies will be outlined.

#### (1) Direct Contamination

Direct contamination can occur in retrospective studies when the subject's depressive symptomatology leads them to report an excess of events or stimulates the interviewer to probe more thoroughly for undisclosed events. Measurement of the causal variable can be contaminated in this manner by knowledge or influence of the effect variable and thus internal validity is reduced. The depressed person,



in his attempt to explain his symptomatology causally, may report more distressing life experiences than the non-depressed person. Brown (1974) argued that this tendency is a form of 'effort after meaning' a process identified by Bartlett in his work on the active reorganisation of memory.

Brown (1974) argued that the 'check-list' approaches to the measurement of life-events as exemplified by the Schedule for Recent Life Experiences (S.R.E.) are particularly prone to this form of contamination. The respondent is not, however, the only culpable person in respect to direct contamination effects. Brown (1974) suggested that studies which use semi-structured interview techniques are also open to this threat. He illustrated this argument by examining results on the cause of Downs Syndrome in which 'shocks' during pregnancy rather than chromosomal abnormalities were implicated. Stott (1958, quoted Brown 1974) reported that mothers of mongoloid children retrospectively reported experiencing more 'shocks' during pregnancy than mothers of normal controls. More recent chromosomal evidence would suggest that interviewer expectancies together with the mothers' 'effort after meaning' were responsible for this effect.

How can this threat be countered? Brown (1974), echoed by Paykel (1978) and Cochrane and Sobol (1980), suggested that one requirement is that careful pre-interview description of the event should be carried out. They argued that the subjects should not decide which events occurring to which elements of their social circle should be reported, but rather this should be prescribed in detail before the

interview is initiated. In addition, during the interview the interviewer, should be required to concentrate on the fact of the occurrence of all the events before pursuing issue concerning the meaning of the events for the individual.

Brown (1974) argued that a further necessary precaution is that the 'threat' or severity of the event should be rated in the absence of information relating to the respondent's psychological state. Brown achieved this in his studies by having a committee rate each event while being blind to the subject's psychological state. This separation could also be achieved by applying consensually derived weights obtained from suitable respondent samples (e.g. Holmes and Rahe, 1967; Paykel et al, 1976).

The influence of direct contamination on the internal validity of the causal relationship is minimised by careful pre-interview definition of relevant events, by establishing the occurrence of events in the absence of information about their impact, and finally by keeping the process of rating symptomatology and event characteristics separate.

## (2) Indirect Contamination

Brown (1974) argued that 'indirect contamination' could also critically affect the internal validity of causal statements in this field. Indirect contamination may occur in prospective studies with the measurement of life events at the initial interview being influenced by the investigator's awareness of the subject's anxiety. As was noted above the interviewer may elicit more events from an

anxious patient or the anxious subject may endorse more events on a checklist than a non-anxious subject. At a later date, the anxious subject may show more intense psychological disturbance and a presumption of causation between events and symptoms made. This interpretation would be invalid because of indirect contamination with the subject's anxiety being responsible for both distorting the measurements process and for 'causing' the symptoms. Brown (1974) indicates that the threat of indirect contamination can be countered using essentially the same procedure as outlined above for direct contamination.

### (3) Spurious Correlations

The problem of spuriousness, or the possibility that the correlation between two measures is a result of their mutual correlation with a third variable, is a more serious threat to internal validity and it is not a result of faulty measurement process. Brown (1974) indicated the spuriousness can occur even with perfect measurement of the putative cause and effect variables. Thus, a third factor, such as high general anxiety or neuroticism, may increase both the frequency of events a subject experiences and the probability of psychological disturbance. Thus, as Miller and Ingham (1979) and Tennant et al (1981a) suggested, the correlation between events and psychological disturbance might not be causal but rather might merely reflect mutual correlations with the subjects' personality characteristics and life stress variables.

Brown (1974) and Brown and Harris (1978a) were unable to provide a totally satisfactory solution to the problem of spuriousness. Brown

(1974), rather optimistically argued that '...since actual experience of the event is ignored, we can hope to rule out the possibility that some unknown factor is influencing both onset and reporting or experience of events (i.e. to rule out indirect contamination and spuriousness.)' Brown (1974, p 228). While these precautions might counter the threat of spuriousness to some extent, a more powerful, yet not foolproof procedure would be to measure intervening variables that potentially have a spurious impact. These measures could be used as control variables. Blalock (1964) suggested that spurious relationships can be detected by controlling for intervening variables in statistical analysis and determining whether this results in the original correlation between the putative cause and effect measures disappearing. This again is only a partial solution as there is a very large number of variables that could be considered, at any particular level of reduction, to intervene between cause and effect variables. The experimenter must be guided by theory to allow the determination of which control variables should be measured.

The application of these design and analysis strategies helps to increase the internal validity and thus to increase the plausibility of any particular cause and effect relationship between measured variables.

## CONSTRUCT VALIDITY AND LIFE EVENT RESEARCH

If the investigator has demonstrated that the link between two variables is plausibly causal then he must attempt to determine which higher order constructs are involved in the relationship. It is of limited value to describe variables as causally related if they must be described in exhaustive, operational detail, thus higher order constructs should be specified. Cook and Campbell (1979) defined construct validity as '...the approximate validity with which we can make generalisations about higher order constructs from research operations'. (p 38).

High construct validity has practical as well as theoretical importance. Replication of results across studies is easier if the putative cause and effect constructs have been clearly defined. In addition, if particular types of causal agents (e.g. undesirable events) are thought to be particularly potent then it may be more efficient to focus new studies on these.

Cook and Campbell (1979) suggested that there are two distinct processes involved in the assessment of construct validity. These processes entailing tests for convergence across different measures of the same construct and tests for divergence between measures of related but conceptually distinct constructs. These tests of construct validity require a two-pronged attack involving both careful pre-experimental verbal specification of the constructs and detailed data analysis. These two avenues of attack will now be considered.

### Pre-Experimental Definition of Constructs

Cook and Campbell (1979) noted that the first step in evaluating construct validity entails, '...careful pre-experimental explication of constructs so that definitions are clear and in conformity with public understanding of the words being used'. (p 60). Three general pitfalls should be avoided at this stage. The construct should neither be under-represented nor over-represented, that is, all core features of the construct should be measured while irrelevancies or epiphenomena should be excluded. In addition, it is important that constructs from different domains of discourse should be maintained as separate. Thus, for example, symptoms, social support and life events should be maintained as separate constructs. These three pitfalls are very apparent in the literature on depression in general and life events and depression in particular. Procedures for minimising them will now be considered.

Weckowitz (1973) indicated that the term depression has four distinct usages that conform with public understanding. The term may be used to describe a normal affective state, a symptom, a syndrome, or a diagnostic entity. These different usages are apparent in the life event literature. For example, Miller and Ingham (1976) used the term depression to indicate depressed mood, that is, they were using the term to refer to one symptom. Brown and Harris (1978a), however, used the term to denote a diagnostic entity. Thus, their description of depression not only included depressed mood but it also included a wide range of other symptoms such as fatigue, irritability, retardation, confusion and weight loss. It is very clear that these different investigators are talking about very different constructs

and very different experiences. Miller and Ingham's (1976) use of the term depression may suffer from construct under-representation in that the publicly accepted definition of depression generally entails other core defining features. The use of the single symptom to represent the construct depression will result in lower construct validity than when multiple symptoms are used to triangulate on the construct.

Brown and Harris' (1978a) 'caseness' measure may suffer from construct over-representation in the sense that their definition includes an extremely heterogeneous set of symptoms. In order to reduce construct over-representation, they attempted to divide their measure into separate constructs i.e. 'psychotic' and 'neurotic' depression (Brown et al, 1979). This attempt will be considered below. Construct over-representation may result in reduced statistical conclusion validity in that the operationalisation of the construct is dominated by irrelevant factors. The inclusion of epiphenomena will tend to attenuate the relationship between the proposed causal principle and the putative effect.

Tennant et al (1981a) indicated that life event measures should have an appropriate level of representation in order that specific potent properties of the events can be discovered. They argued that '...a wide range of events of differing qualities, severities and types is required in order to contrast those events which possess the hypothesised pathogenic qualities with those that do not'. (p 380). Multiple rating of each individual event can be considered to identify the pathogenic features (e.g. Brown and Harris, 1978a; Miller and

Ingham 1983) or the influence of specific types of events e.g. desirable versus undesirable or exits versus non-exits (Paykel et al 1969; Cooke and Greene, 1981) can be contrasted.

As with the outcome variable, over-representation in the life event measure can lead to reduced statistical conclusion validity due to the presence of irrelevancies.

The application of data analysis techniques that attack the problem of over-representation will be considered below. The third problem in the pre-experimental phase, i.e. separating constructs from related but distinct domains of discourse will now be considered.

#### Separating The Domains Of Discourse

One of the principle considerations in the assessment of construct validity is the degree of divergence between measures of related but conceptually distinct constructs. Therefore, in the pre-experimental explication of key constructs, it is necessary to separate the characteristics, behaviours or experiences that characterise the essence of each of the theoretical domains of interest. If this is not achieved then there is a danger that the putative cause-effect relationship is spuriously based on two measures of the same construct. To illustrate this point the study of Weissman & Paykel (1974) should be considered. Weissman & Paykel (1974) reported a relationship between poor social adjustment and depression in a group of women. Unfortunately many items in the Social Adjustment Scale (Paykel et al, 1971) appeared to be related to primary symptoms of depression e.g. guilt, worry, resentment, disinterest in sex, friction

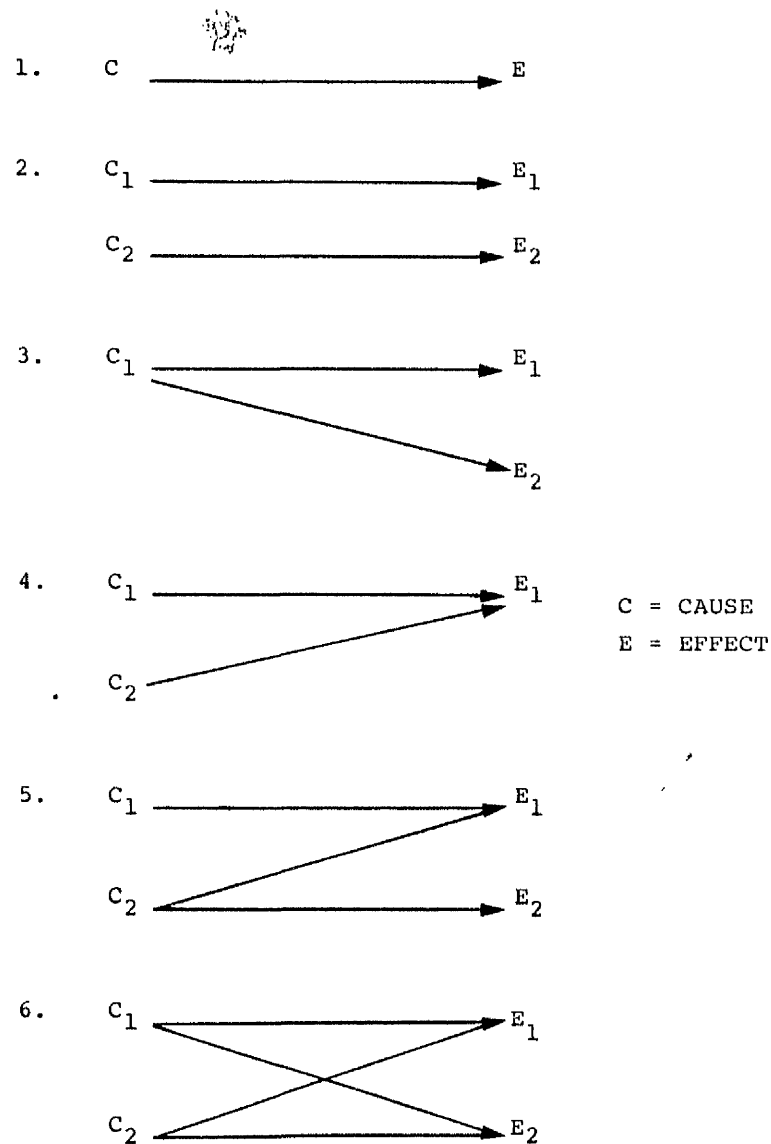


and distress. In using the full Social Adjustment Scale there is a danger that the social adjustment construct is contaminated and submerged by measures of the depressive symptom domain and that part of the observed correlation is due to this contamination.

Unfortunately, no a priori rules exist for distinguishing between the core features and correlates of depression. However, some guide-lines are available. Derogatis et al (1972) and Ni Bhrolchain (1979) have cogently argued that the understanding of the causal processes in depression is increased when the construct depression is defined in the restrictive terms of clinical signs and symptoms. These authors contend that greater construct validity is achieved if depression is defined in terms of signs and symptoms and distinguished from correlated, but conceptually distinct, features such as demographic, personality, social adjustment and life event variables.

Ni Bhrolchain elegantly demonstrated the utility of this approach in the context of the unitary-binary debate in the classification of depression. Making the restrictive assumption that at the most there are two putative causes of depression and only two symptom patterns, she demonstrated that there are at least six logically distinct relationships between the cause and effect variables (See figure 1).

If the symptoms and potential causes are not maintained separate, then when two distinct forms of depression are detected, it is not possible to argue whether one symptom pattern results from two different causes or two causes result in one symptom pattern or whether a more complex situation exists.




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FIGURE 1. SIX HYPOTHETICAL MODELS DEPICTING POSSIBLE  
RELATIONSHIPS BETWEEN AETIOLOGY AND SYMPTOMATOLOGY  
(FROM NI BHROLCHAIN, 1979)

Restricting the construct of depression to clinical features, therefore, allows clear demarcation between the putative etiological agent and its putative effect.

Lack of clear demarcation has been a problem in life event research (Thoits, 1981). Dohrenwend and Dohrenwend (1978) argued that 29 of 43 'life events' in the ubiquitous Social Readjustment Rating Scale (Holmes and Rahe, 1967) could be regarded as symptoms of illness or related to illness. Schless et al (1977) indicated that there are certain phenomena that, on a priori grounds, could be equally well regarded as either symptoms or antecedents of psychological distress. Even if detailed contextual information is available it is frequently impossible to determine whether, for example, 'marital difficulties' should be regarded as a cause or a concomitant of depressive disorders.

Tennant et al (1981a) suggested that more subtle distortions may be present in the life event literature. These distortions may occur when the influence of life events and other antecedent variables on psychological well-being is considered. Tennant et al (1981a) suggested that the 'contextual threat' measure developed by Brown and Harris (1978a), while being uncontaminated by symptoms, may be contaminated by the antecedent variables that they term vulnerability factors. Tennant et al (1981a) argued that the vulnerability factors are not independent and distinct from the life event measure because they are part of the context within which 'contextual threat' is assessed.

Construct validity is increased to the extent that the separate constructs are independently measured and defined.

When the characteristic behaviours and experiences of each of the individual domains of interest have been distilled to their essential features an unacceptable level of heterogeneity will generally remain. It is necessary, therefore, to further condense and clarify the constructs. This is one of the purposes of data analysis aimed at maximising construct validity. The importance of this second avenue of attack on construct validity will now be considered.

#### Data Analysis and Construct Validity

While careful pre-experimental specification of constructs can be regarded as being the first approach towards improved construct validity, the second approach is careful data analysis. Such analysis may examine the relationships among pre-experimentally defined constructs or the relationships among constructs defined a posteriori on the basis of the result patterns obtained. The principal purpose of these analyses is the demonstration of specificity of effect. Following Susser (1973) specificity may be regarded as the extent to which one construct can be used to predict another. Susser (1973) echoed by Tennant et al (1981a) argued that while the absence of high specificity does not negate a causal hypothesis its presence increases its plausibility. Paykel (1974) noted that specificity can be improved by examining both sides of the cause-effect equation, that is, by refining both the independent and dependent variables. Refining the independent and dependent variables entails tests for convergences across cognates and divergences between conceptually distinct concepts

as well as tests of strength of relationships amongst specific cause and effect constructs. While simple analysis may be applied, Principal Component Analysis is a valuable tool for examining divergences and convergences, while bivariate and multivariate correlational analysis are used for examining relationships among cause and effect constructs. Simpler approaches and evidence in support of specificity will be considered initially with the application of Principal Component Analysis and Multiple Correlational Analysis being considered later.

The simpler approaches to specificity are well illustrated by Susser (1973) in the analysis of the link between smoking and illness. Initial observations on the link between smoking and illness suggested little specificity because smokers were shown to have increased risk of death from all causes. Refining the independent variable, that is, the cause of death, revealed a significantly greater risk of cancer in general and cancer of specific sites in particular. The refinement of the independent variable, that is, the type of smoking, indicated that cigarette smoking carried an increased risk for particular types of cancer as compared with pipe smoking. Susser (1973) indicated that refining the dependent and independent variables revealed a more specific relationship and thus increased the plausibility of the causal influence.

### Specificity in the Life Event Literature

Initial examination of the life event literature might suggest little evidence of specificity. Indeed over the last two decades life events have virtually acquired the status of universal causes. They have been implicated in the processes of a wide variety of pathological conditions: from schizophrenia to depression, from diabetes mellitus through myocardial infarction to the common cold (Brown and Birley, 1968; Brown and Harris, 1978a; Bradley, 1979; Connolly, 1976; Totman, 1979, 1981).

Within the field of psychological dysfunction the process of refining the dependent and independent variables has gradually taken place. Initial studies examined the link between events and illness in general (Rahe et al, 1967) while later studies examined the influence of specific events on specific diagnostic groups (Ulenhuth and Paykel, 1973b). Despite these attempts to identify specificity of effects, recent commentators have argued that there is little evidence of specificity (Andrews and Tennant, 1978; Tennant et al, 1981a), this view will be examined.

### Refining The Causal Construct

Initial attempts to demonstrate some degree of specificity emphasised the refining of the independent variable, that is, the life event measure. Early studies (e.g. Holmes and Rahe, 1967) attempted to measure change or disruption in a person's life, whether this change could be regarded as positive and desirable or negative and undesirable. Paykel et al (1971a) refined the measure from change per se to change that entailed some degree of upset or distress. Tennant

and Andrews (1978) empirically demonstrated that distressing change rather than change per se was a better predictor of neurotic impairment. Paykel et al (1969) were perhaps the first group of researchers to move from the study of heterogeneous events, as exemplified by Holmes and Rahe (1967) list, towards the study of specific categories of events. Paykel et al (1969) attempted to improve the construct validity of the empirical relationships by contrasting the impact of certain event types. They demonstrated that undesirable events and 'exit' events, the latter being events involving departures of others from the social field of the subject, were more closely related to depression than were desirable events or 'entrances' into the social field.

The comparative importance of undesirable events and exits has been confirmed by independent studies (Ross and Mirowsky, 1979; Barratt, 1979; Cooke and Greene, 1981; Grant et al, 1981).

Specifying which aspect of the construct 'life events' are predictive of psychological distress has not always been straightforward. Brown et al (1973) considered approximately 30 ratings of each event and found that only one was predictive of depression. Long-term contextual threat, or the degree of threat or unpleasantness entailed by an event one or more weeks after its occurrence, was shown to be predictive (Brown and Harris, 1978a).

Other researchers have confirmed that the life event construct is too general and requires to be refined in order that the irrelevant

features may be excluded. Mehrabian and Ross (1977) demonstrated an association between symptoms and unpleasant and arousing events but not between the pleasant, unarousing or dominance-submissiveness inducing events. Findlay-Jones and Brown (1981) distinguished between events that entailed loss and those that entailed danger. Their rating of loss related to loss of a valued person, physical health, employment or career opportunities or a cherished idea. The rating of danger related to events that were likely to produce a specific unpleasant crisis in the future. Findlay-Jones and Brown (1981) demonstrated that 'danger' events were good predictors of anxiety while 'loss' events were good predictors of depression.

Tennant et al (1981b) demonstrated that another type of event, termed 'neutralising life events', i.e. events of minimal threat which substantially negated or counteracted the impact of an earlier threatening event, acted in a positive fashion to facilitate the remission of neurotic disorders. Grant et al (1981) also indicated that the positive features of events may be important by demonstrating a negative correlation between the occurrence of desirable events and feelings of dysphoria and somatic complaints. The aggregation of evidence seems to imply that specific characteristics of events are better predictors of psychological distress than others. In addition, however, there is some evidence, albeit limited, that event characteristics may interact together to enhance their predictive ability (Miller and Ingham 1983; Cooke and Greene, 1981). Cooke and Greene (1981) demonstrated that somatic symptoms were elevated only when stress arising from miscellaneous sources was accompanied by stress arising from exit events. Refining



the independent variable from measures of overall disruption and change to more specific concepts of threat, loss and danger appear to have paid dividends and increased the specificity and construct validity of the presumed cause.

#### Refining the Effect Construct

While refining the independent variable has proved useful, Tennant et al (1981a) suggested that attempts to demonstrate specificity by refining the other side of the equation have been less successful. Andrews and Tennant (1978) argued that life events merely acted to demoralise the individual with their symptom pattern being determined by personality or socio-demographic characteristics.

In general, the earlier studies failed to detect any link between specific psychological symptom patterns or psychiatric diagnostic categories (Garmany, 1958; Leff, et al, 1970; Thomson and Hendrie, 1972; Ulenhuth and Paykel, 1973a; Paykel, 1974; Brown and Harris, 1978a; Brown et al, 1979; Benjaminsen, 1981). Indeed, Brown and Harris (1978a) strenuously argued against the presence of specificity with regard to sub-types of depression and stated '...the psychiatric tradition has been misleading in its claim that there are, in any ~~sense, any clearly distinct~~ forms of depression and that the psychotic type is in general not reactive'. (p 217). More recently, however, Bebbington et al (1981) and Findlay-Jones and Brown (1981) have suggested that events may predict specific symptom clusters or particular diagnostic categories. Bebbington et al (1981) demonstrated that events predicted caseness levels of neurotic

depression and anxiety but did not predict psychotic depression or retarded depression with associated pathological guilt. Further, as was noted above, Findlay-Jones and Brown (1981) found that loss events predict depression better than mixed symptomatology and danger events predicted anxiety better than mixed symptomatology. These results suggest that there is a degree of divergence between measures of related concepts (i.e. symptoms) and thus tend to increase specificity and construct validity. It is argued in detail below that the apparent lack of specificity noted above may be more illusory than real.

#### EXTERNAL VALIDITY AND LIFE EVENT RESEARCH

When the investigator has established a case for a causal link between his cause and effect constructs for a given sample, the next step is to determine the level of generality of the relationship. Cook & Campbell (1979) defined external validity as '...the approximate validity with which conclusions are drawn about the generalisability of a causal relationship to and across populations of persons, settings and times'. (p 39). By generalisation to a particular population of persons, settings and times they mean the extent to which the observed relationship can be thought to apply to the average person in the population under average conditions. Thus, for example, the extent to which Brown and Harris (1978a) could argue that their results apply to the average female in Camberwell. In considering generalisation across types of persons, settings and times they are concerned with the extent to which an observed relationship can be considered to apply to sub-groups within the obtained sample or to new samples. This is well illustrated by the work of Brown and his

colleagues (Vide Infra) in that they have attempted to determine whether their results, obtained in the inner city area of Camberwell, generalise to a sample obtained in the Outer Hebrides.

Cook and Campbell (1979) argued that generalisation across samples is the most important aspect of external validity in field settings. This is principally because of the difficulties inherent in obtaining a sample that adequately represents the original population of people or assessment methods.

Susser (1973) similarly contended that the plausibility of a causal link is increased when similar results are obtained with diverse measures and procedures in different samples. He indicated that concern with generalisation across situations, methods and samples is related to the principles inherent in Mills 'method of agreement'. Essentially, the investigator is concerned to demonstrate the presence of a constant association under varying circumstances.

These general views would seem to imply that the evaluation of external validity is based on replication. Replication within one study where the variability of association across sub-groups and across measures is evaluated and replication across studies where the same techniques are applied to different populations or different techniques and measures are applied.

Cook and Campbell (1979) argued that the assessment of the generalisability of a relationship within a study can be enhanced by

deliberately increasing the heterogeneity of the sample studied. They noted that random sampling is ideal as it ensures heterogeneity. In many field settings random sampling is not always feasible, however, heterogeneity can be enhanced by sampling from a wide range of people, settings and procedures. Given a heterogeneous sample the limits of generality can be assessed by examining the results for the presence of statistical interactions. Statistical interactions demonstrate the extent to which the magnitude of an association varies with the level of a third variable. From the point of view of life event research, the investigator would be concerned to demonstrate whether or not the association between the experience of life events and psychological distress varied with other variables such as age, sex and class. To the extent that interactions are present within a particular sample the degree of generality and thus external validity of any putative causal link is diminished.

#### The Generality of Relationships Within Life Event Research

Tennant et al (1981a) argued that there is a reasonably high level of generality of findings with regard to the simple link between life events and schizophrenia, depression and neurotic difficulties. This level of generality has been achieved despite the variability of populations studied and procedures used.

The degree of generality and replication of findings diminishes substantially when the more complex models linking life events, symptoms and social variables are considered. For example, some workers have argued that social support variables act merely to enhance the influence of life stress (e.g. Lowenthal & Havens, 1968;

Brown et al, 1975; Cobb, 1976; Kessler, 1979; Dimsdale et al, 1979) while others have argued that they directly influence the level of symptomatology experienced (e.g. Miller and Ingham, 1977; Tennant and Bebbington, 1978; Lin et al, 1979; Henderson et al, 1980). Replication across studies has not supported the generality or external validity of the more complex models.

Myers et al (1972) attempted to replicate their findings on a sample using the same measures on two different occasions some two years apart. They found a similar distribution of life events and psychiatric symptoms on both occasions and they found that the association between the two was replicated across occasions.

Brown and his colleagues, virtually alone amongst life event researchers, have attempted to demonstrate the generality of a more complex model, their 'vulnerability' model (Brown et al, 1975), by examining a variety of samples. They have examined the effects of life events and vulnerability factors on depression in two general population samples in Camberwell and two in the Outer Hebrides as well as a sample of depressed in- and out-patients (Brown et al, 1975; Brown and Harris, 1978a; Brown et al, 1979; Brown and Prudo, 1981). By contrasting the Outer Hebrides and Camberwell, Brown and his co-workers deliberately increased the heterogeneity of their samples in order to provide an extremely rigorous test of the external validity of their model. Severely threatening life events and major long-term difficulties appeared to play the same role in all of these five samples; an elevation in the rate of events occurred prior to onset.

While this simple link between provoking agents and depression was successfully replicated, Brown and his colleagues were rather less successful in their attempts to replicate the more subtle features of their aetiological model of depression. These attempts are considered in considerable detail below.

#### SUMMARY

Ethical and practical difficulties preclude the use of conventional experimental designs in the field of life event research. Other criteria for inferring causal associations are required. The validity of putative causal associations can be assessed with regard to four inter-related but distinct forms of validity, namely, statistical conclusion validity, internal validity, construct validity and external validity. These forms of validity relate to four important issues faced by the investigator. Firstly, do the life event measure and the depression measure correlate? Secondly, if they correlate, is there a plausible causal relationship between the two measures? Thirdly, given a plausible causal relationship between the two measures, can the theoretical constructs that underpin the correlation be identified? Fourthly, does the observed association apply generally across different people, settings and times?

These types of validity can be improved by particular strategies of design and analysis. The design and analysis of any life event study should try to maximise these types of validity.

## CHAPTER 2

### THE INFLUENCE OF ADDITIONAL VARIABLES ON THE SIMPLE EVENT-SYNDROME LINK

#### INTRODUCTION

The criteria that may be used in the evaluation of the causal hypothesis that life events cause depression have been considered in detail above. Rabkin and Struening (1976), in their incisive review of the life event literature, argued that the preoccupation with the simple event-syndrome link should give way to the consideration of more complex models. Rabkin and Struening (1976) were not alone in their contention and indeed, various authors have argued that a third variable should be added to the simple event-syndrome link. These variables have been variously called mediators (Brown et al, 1975; Rabkin and Struening, 1976; Dohrenwend and Dohrenwend, 1978; Andrews et al, 1978a; Lin et al, 1979) moderators (Cobb, 1976; Miller and Ingham, 1976; Johnston and Sarason, 1978b; Andrews et al, 1978a; Jenkins et al, 1979; Bebbington, 1980) modifiers (Paykel, 1978; Kessler, 1979; Jenkins et al, 1979), vulnerability factors, (Brown et al, 1975; Miller and Ingham, 1976; Roy, 1978; Kessler, 1979; Zubin, 1979; Bebbington, 1980), buffers, (Lowenthal and Haven, 1968; Dean and Lin, 1977), protective factors, (Nuckolls et al, 1972; Brown and Harris, 1978a) intervening variables, (Rabkin and Struening, 1978; Garrity, 1977a) resistive factors, (Garrity et al, 1977b) and conditional or predisposing factors (Cassel, 1976).

Brief consideration of the names given to these additional variables indicates that they are generally construed as variables that qualify or modify the impact of stressful life events.

This chapter is concerned, therefore, with going beyond the simple event-syndrome link to identify and analyse issues pertaining to the influence of these additional variables. Six specific issues will be considered.

Firstly, a selection of the additional variables will be described in order to illustrate the quality and range of the variables which have been considered. Secondly, a brief discussion of the theoretical and empirical advantages that accrue from the consideration of additional variables will be provided. Thirdly, four hypotheses concerned with the mode of influence of the additional variables will be described and the implications of these hypotheses will be outlined. Fourthly, the empirical evidence in support of each of these four current hypotheses will be evaluated in detail. In particular, the work of George Brown and his colleagues, in defence of the vulnerability hypothesis, will be discussed in detail. Fifthly, the methodological and statistical difficulties inherent in describing the mode of action of the additional variables will be considered. Sixthly, and finally, the rationale behind the development of a mathematical model designed to aid the evaluation of each of the four hypotheses will be described in detail.



## VARIABLES THAT INFLUENCE THE IMPACT OF LIFE EVENTS

A considerable number and variety of variables have been posited as having an influence on the simple event-syndrome link. In this brief discussion no attempt will be made to provide an exhaustive or detailed account of all the variables that have been put forward as being influential. Rather, the purpose of this section is to provide an indication of the range of variables which have been examined.

Several reviewers (e.g. Rabkin and Struening, 1976; Paykel, 1979; Brown and Harris, 1978a; Rahe and Arthur, 1978), on discussing the additional variable problem, have argued that important additional variables may be drawn from the sociological, psychological and physiological domains of discourse. Examples from within each of these domains will now be considered.

### Additional Variables Within The Sociological Domain

Lowenthal and Haven (1968), Brown et al (1975, et seq.), Cobb (1976), Miller and Ingham (1976) and Henderson et al (1980) have been instrumental in emphasising the role that both the extent and the quality of social relationships have in mediating the impact of stressful life events. Lowenthal and Haven (1968) were perhaps the first workers to describe the important role that an intimate or confiding relationship may play in relation to the simple event-syndrome link. Brown et al (1975) have intensively promulgated the notion that intimacy can be a powerful mediator of life event stress. In the Camberwell studies of Brown et al (1975) and Brown and Harris (1978a), the presence of a confiding relationship appeared to be the only aspect of social relationships that provided any protection

against the effects of life events. Other workers, most notably Miller and Ingham (1976), Eaton (1978), Henderson et al (1980) and Surtees (1980) have argued that other aspects of social support networks may play a lesser but, nonetheless, important role. Miller and Ingham (1976), for example, supported the view that the presence of a confidant afforded protection against life events, but further, they indicated that the presence of a few acquaintances also provided protection, albeit to a lesser degree. Brown et al (1975 et seq.), Paykel (1979), Andrews et al (1978a) and Kessler (1979) have suggested that coarser sociological variables such as socio-economic status may also effect the impact of life events. Brown et al (1975 et seq.), Roy (1978) and Finlay-Jones and Burville (1979) have contended that lack of employment might also have an influence.

#### Additional Variables Within the Psychological Domain

Many authors (e.g. Hendrie et al, 1975; Rabkin and Struening, 1976; Garrity, 1977a, 1977b; Paykel, 1979; Johnston and Sarason, 1978b; Jenkins, 1979) have contended that personality characteristics probably play a significant role in relation to the event-syndrome link. Paykel (1979) surmised, for example, that people with obsessional personalities might be particularly influenced by those events that involve major changes in their life patterns.

Johnston and Sarason (1978b) argued that the extent to which an individual perceives that he has control over his environment, as measured by a 'Locus of Control' scale, could moderate the impact of life events. Smith et al (1978) contended that the link between life

events and psychological adjustment varied as a function of a subject's level of 'sensation seeking'; that is, with the level of environmental stimulation that the subject believes he requires. Hendrie et al (1975), using the Minnesota Multiphasic Personality Inventory (MMPI), found no evidence to support the view that personality characteristics influenced the impact of life events. Garrity et al (1977b) argued that the personality dimensions of 'Conformity', 'Intellectualism' and 'Sensitivity' may have a role in mediating the event-syndrome link. Jenkins (1979) theorised that ego strength and flexibility may provide resistance to the impact of life events.

Other psychological characteristics, beyond personality traits, have also been implicated. Jenkins (1979) has suggested that problem-solving ability and social skills may be of some importance. Pearlin and Schooler (1978) and Andrews et al (1978a) have contended that a wide range of general coping skills may also be implicated. Finally, Cobb (1976) and Paykel (1979a) argued that habitual psychological defence mechanisms and reaction patterns should be identified and their influence on the simple event-syndrome link analysed.

#### Additional Variables from the Physiological Domain

There has been little speculation, and as far as the author is aware, no empirical work, that defines the role of physiological variables in this field. Paykel (1979), however, in his descriptive development of a multi-causal model, speculated that cyclical changes in function, enzyme defects in mono-amine pathways and variation in metabolic pools of transmitter substances may influence the impact of life events.

The evaluation of additional variables from the physiological domain, however, has not gone beyond the level of speculation.

#### THE IMPORTANCE OF ADDITIONAL VARIABLES

At least three advantages may accrue from the consideration of additional variables. The consideration of additional variables may:

- (1) increase the variation explained in the dependent variable and in addition explain some of the variability in response to events,
- (2) increase the level of understanding of the causal processes involved,
- (3) suggest possible treatment strategies.

Each of these three advantages will be considered in turn.

#### Explanation of Variability in Response to Events

It was argued above that a pre-requisite characteristic of a causal link was evidence for mutual covariation between the measures of interest. In the section concerned with statistical conclusion validity and the simple event-syndrome link, the influence that random heterogeneity of respondents had on attenuating the variance explained in the dependent variable was discussed. Restricting the sample to a homogeneous group reduces the threat to statistical conclusion validity but there is a concomitant reduction in external validity. Cook and Campbell (1979) indicated, however, that when the appropriate respondent variables are used as covariates the variance explained in the dependent variable could be enhanced without any detrimental effect on external validity. Brief examination of the life event literature suggests that an increase in the variance explained by the

simple event-syndrome link is desirable. Although the simple event-syndrome link has frequently been shown to be statistically significant, the amount of variance explained has tended to be low. Two sets of reviewers have assessed the variance explained in a range of studies; Cochrane and Sobol (1980) indicated that on average 10 per cent of the variance is shared, whereas Lin et al (1979) presented a rather more pessimistic estimate of between 4 and 8 per cent. The identification of relevant additional variables could improve the statistical conclusion validity of the relationship by improving the estimate of the magnitude of simple event-syndrome links that might otherwise be obscured (Tennant et al, 1981). It has been argued in detail elsewhere (Cooke and Hole, 1983), that variance explained is not necessarily the most appropriate index of causal impact. Nonetheless, because of the common application of this index in the life event literature (e.g. Cochrane and Sobol, 1980; Miller and Ingham, 1979; Lin et al, 1979) and its direct relevance to statistical conclusion validity, it may be argued that procedures that enhance it may be desirable.

#### Enhanced Understanding of Causal Processes

From the theoretical rather than the statistical viewpoint, the evaluation of the additional variables may explain some of the variability of response to life events; not everyone who has experienced life events necessarily develops significant psychological symptoms. Brown and Harris (1978a), for example, demonstrated that the link between events and symptoms was stronger for working class women than for middle class women. Thus the additional variable,

'social class' appeared to contribute to the variability in responsiveness to life events. This type of knowledge about variations in the strength of the simple event-syndrome link, variations in relation to the characteristic of the 'host', can be useful in enhancing the construct validity of the relationship (Weiss, 1981). To the extent that the variability in the event-syndrome link can be theoretically explained in terms of the 'host' characteristics, our belief in the causal significance of life events is enhanced. This point may be illustrated by returning to the example of social class as an additional variable. Brown et al (1975) originally espoused the 'stress-distribution' hypothesis. In simplistic terms, they held that life events cause depression. More working class women experience depression than middle class women and that this is due to the inequitable distribution of stress. Brown et al (1975) had to modify this view. Although working class women reported more stressful events and long term difficulties than middle class women, the comparative excess of stress was not sufficient to account for the greater prevalence of disorder amongst working class women. Working class women appeared to be particularly susceptible to the events that they had experienced. Brown et al (1975), by refining the 'social class' construct and examining the consequences of being working class, attempted to explain the apparent susceptibility to life events. To the extent that they succeed in these explanation (vide infra) they increased the theoretical understanding of the mode of influence of life events.

### Possible Treatment Strategies

The examination of additional variables may suggest possible treatment strategies. The study of life events has been characterised as a 'doomsday' exercise (Andrews and Tennant, 1978) because it is often difficult to devise strategies to reduce the frequency of many events and thereby prevent people suffering their adverse consequences. The study of additional variables may suggest approaches to the development of prophylactic strategies. Cobb (1976) for example, has argued that the availability of social support acts to buffer the impact, or immunises individuals against the effects of severe life events. While it may be difficult to reduce the frequency of events, therapeutic strategies that influence the quality and extent of social and marital relationships are available (e.g. Trower et al 1977; Jacobson and Martin, 1976). If Cobb's contention proves valid then these strategies might be applied prophylactically to improve resistance to the stresses and strains of everyday life.

### HYPOTHESIS PERTAINING TO THE MODE OF ACTION OF ADDITIONAL VARIABLES

Examination of the life event literature suggests that four hypotheses regarding the effect of additional variables on the simple event-syndrome link have been mooted. These may be stated in the following manner.

- (1) The Vulnerability Hypothesis: The additional variable increases the level of psychological distress in the presence of life-stress, but has no effect in the absence of life stress.
- (2) The Independent Causes Hypothesis: The additional variable

increases the level of psychological distress, irrespective of the level of life stress.

- (3) Mutual Potentiation Hypothesis: The additional variable increases the level of psychological distress only in conjunction with high life stress. Psychological distress will be low if either the additional variable or life stress is low.
- (4) Synergism Hypothesis: The additional variable and life stress have independent influences on psychological distress, but, in addition, their concatenation produces a disproportionate increase in the level of psychological distress.

The first hypothesis, the vulnerability hypothesis, has probably received most attention. Lowenthal and Havens (1968), Brown et al (1975) and Cobb (1976) have been strong proponents of this view. In essence, they argue that a 'vulnerability' factor does not influence a person's level of psychological distress until they experience stressful life events; under stress the vulnerability factor amplifies or exacerbates the effects of the life event or inhibits effective coping with the life event. For example, if a person lacks a confiding relationship and experiences a highly threatening life event then he has a high risk of becoming depressed. In the absence of a threatening life event, the absence of a close confiding relationship does not result in an increased risk of becoming depressed. This hypothesis has strong intuitive appeal, to clinicians in particular, as it implies that the improvement of a person's inter-personal relationships may help them resist the effects of life events.



The second hypothesis, the independent causes hypothesis, has been supported by Andrews et al (1978a), Miller and Ingham (1979) and Henderson et al (1980). Under this hypothesis, poor social relationships are thought to produce psychological distress irrespective of whether an individual has experienced life events or not. The quality of a person's social and marital relationships would always be important with respect to their psychological wellbeing and would not merely become influential following the experience of stressful life events. Under the independent causes hypothesis the additional factor retains its theoretical and clinical interest. It possesses less intuitive appeal, however, because unlike the vulnerability hypothesis, it implies that intervention that improves a person's interpersonal relationships will not aid them resist the impact of ubiquitous and inevitable life stress.

The third hypothesis, termed the mutual potentiation hypothesis, suggests that life events and social support mutually potentiate the effect of the other without having any independent direct effect. High levels of psychological distress will occur only when both poor social support and life stress are high, psychological distress being low if either poor social support or life stress is low. Therefore, neither poor social relationships nor the experience of life events on their own would increase the intensity of psychological distress, however, concatenation of these factors would lead to increased psychological distress. Nucholls et al (1972) reported that a concatenation of high life events and poor social support was required for an increased level of birth complications.

Blalock (1968) indicated that the above description is consistent with a multiplicative log linear analysis model such as that used by Tennant and Bebbington (1978) in the re-analysis of Brown and Harris' (1978a) results. Their re-analysis could be interpreted as being consistent with the mutual potentiation model.

The fourth hypothesis, termed the synergism hypothesis suggests that both life events and poor social support contribute to the level of psychological distress, but the concatenation of poor social support and high life events will produce a disproportionate increase in the level of psychological distress. Miller and Ingham (1979) reported that the availability of a confidant and the experience of life events contributed to the intensity of depression, in addition, concatenation of the two variables produced a disproportionate increase in depression.

Four competing hypotheses appear to be present in the life event literature. The empirical evidence in support of each will be considered in detail below.

#### EMPIRICAL EVIDENCE PERTAINING TO THE MODE OF ACTION OF ADDITIONAL VARIABLES

In this section no attempt will be made to provide an exhaustive review of the evidence in support of each model. An attempt will be made, however, to consider the principal epidemiological accounts of the influence of additional variables on the simple event-depression link. Where it is thought to be advisable, non-epidemiological studies and studies of conditions other than depression, will also be

considered.

In preface it should be noted that any comparison across studies is made difficult by, amongst other things, the absence of cross-study comparability in terms of variables, sample or procedures used. This absence of comparability will clearly test the external validity of any putative causal links, however, it makes the specification of the relevant constructs difficult. This may be illustrated by the simple example provided above. Depression has been a commonly considered construct, however, the term depression may be used to denote a wide range of constructs. Miller and Ingham (1976) used the term depression to denote depressed mood, while Brown and Harris (1978a) used it to denote a diagnostic entity that not only included depressed mood but also symptoms such as fatigue, irritability, retardation, confusion and weight loss. These different usages denote very different constructs. Further differences may emerge even amongst those studies that have enumerated 'cases' of depression. Henderson et al (1980) noted, for example, that differences may emerge in the hypothesis supported because of the time at which 'caseness' is measured. In contrasting their results with those of Brown and Harris (1978a) they argued that the differences may be attributed to the fact that they measured point prevalence while Brown and Harris (1978a) measured prevalence at onset of depression.

Not only is there a considerable variability in the dependent variables that have been considered, but there is also considerable variability in the independent variables that have been used. Independent variables have varied in terms of the constructs used and

also in terms of their operational form. In terms of operations form, life event characteristics have been assessed both by detailed interviewing procedures (e.g. Brown et al, 1975; Miller and Ingham, 1976) and by the completion of self-report questionnaires (e.g. Andrews et al, 1978; Henderson et al, 1980).

Brown (1974) and Dohrenwend and Dohrenwend (1978) have discussed the relative merits of these two approaches; whatever their relative merits it may be that the very different procedures may have provided quite different results.

Given these not inconsiderable difficulties it might appear futile to hope for adequate corroboration or refutation of any of these models. Despite this, several authors including Dohrenwend and Dohrenwend (1974), Cobb (1976), Rahe and Arthur (1978), Kessler (1979) and Dimsdale et al (1979) have argued for the generality of the vulnerability model. They have argued that the vulnerability hypothesis may be an appropriate way to conceptualise the influence of the social environment in a wide range of health problems. Cobb (1976) emphasised the generality of the model by stating '..it appears that social support can protect people in crisis from a wide variety of pathological states: from low birth weight to death, from arthritis through tuberculosis to depression, alcohol and the social breakdown syndrome'. (p 300, own emphasis).

#### Empirical Evidence in Support of the Vulnerability Hypothesis

Of all the four hypotheses, the vulnerability hypothesis has probably generated the greatest interest and the most heated debate (e.g.

Tennant and Bebbington, 1978; Brown and Harris, 1978b; Shapiro, 1979; Bebbington, 1980; Everitt and Smith, 1979). Professor George Brown and his colleagues have been the most vociferous proponents of this hypothesis. Evidence from their work will, therefore, be given prominence. Other evidence will be considered later.

#### The Vulnerability Hypothesis and the Work of George Brown and his Colleagues

It was noted above that the vulnerability hypothesis arose from the failure of the 'stress-distribution' hypothesis. Brown et al (1975) noted that the disproportionate rate of stressful life events and long term difficulties in working class women was not sufficient to explain the disproportionate rate of cases of depression. They postulated, therefore, that working class women were particularly vulnerable or susceptible to the stress that they had experienced.

Socio-economic status is a crude index. Brown and his colleagues, on considering more subtle cross-class differences claimed to identify four factors that explained the observed vulnerability or heightened susceptibility to life stress.

These four factors were,

- (a) Absence of a confiding relationship;
- (b) Loss of mother before the age of 11;
- (c) Lack of employment outwith the home;
- (d) The presence of 3 or more children under the age of 14 at home.

It can be argued that these results should be regarded as preliminary and of perhaps limited external validity because they acted only for married women with children at home. That is, the results were based on a sub-sample of 126 respondents, of whom only 16 were cases. Brown and his colleagues, appreciating that the modest sample size might adversely influence the stability of their results collected a second sample using procedures that were essentially identical to those used by Brown et al (1975). The information obtained from both waves of sampling was combined to give a total sample of 418 respondents. The analysis of this information was published by Brown and Harris (1978a).

The importance of a confiding relationship was still apparent in the larger combined sample. Brown and Harris (1978a) indicated that the effect of a confiding relationship had greater external validity than in the previous analysis because it applied to all women rather than just married women with children at home. Although there was greater generality, the effect was stronger for women with children at home, women without children at home appeared to be less affected by the absence of a confiding relationship.

When the other, 'harder', vulnerability factors were considered in the combined sample both the loss of mother before the age of 11 and the presence of 3 young children at home acted as vulnerability factors. Lack of employment outwith the home, however, failed to act as a primary vulnerability factor in the combined sample. Brown and Harris pleaded special status for this particular factor, arguing that it still acted as a vulnerability factor when present in combination with

the other three factors. It may be argued on the basis of this evidence that it cannot be given the status of a primary vulnerability factor.

This theoretical model, sometimes termed the vulnerability model, developed by Brown and Harris (1978a), was based, therefore, on some 419 respondents, of whom 37 were onset cases of depression i.e. 9 per cent of their combined samples. While the painstaking procedures used by Brown and his colleagues involved a massive research effort to achieve the careful enumeration of these cases, they still represent a numerically small, and thus potentially unstable, set of data. Given the highly skewed nature of the results they are liable to be unreliable.

One method for assessing the stability of results and their generality beyond the original sample is replication across samples. Brown and his colleagues have attempted to demonstrate the generality of the vulnerability model by examining its functioning in a variety of samples. They examined the effects of life events and vulnerability factors on depression in two general population samples in Camberwell and two in the outer Hebrides as well as a sample of depressed patients (Brown et al, 1975; Brown and Harris, 1978a; Brown et al, 1979; Brown and Prudhoe, 1981). By contrasting samples drawn from the Outer Hebrides and Camberwell, Brown and his co-workers deliberately increased the heterogeneity of the samples to provide an extremely rigorous test of the external validity of the vulnerability model. Corroborative support for the vulnerability model, from such diverse sources, would be of great significance.

When cross-sample comparisons are made it appears that severely threatening life event and major long term difficulties play the same role in each of these five samples, i.e. an elevation in the rate of these provoking agents occurs prior to onset. While this simple link between provoking agents and depression was successfully replicated, Brown and his colleagues were rather less successful in their attempts to demonstrate the stability of the vulnerability model. The evidence in support of this contention will be considered.

The evidence from the two Camberwell general population studies will be considered first. Results from the first sample were published by Brown et al (1975), while those for the second sample were only published and analysed in combination with the first sample (Brown and Harris, 1978a). Brown (1980a, Personal Communication) indicated that certain aspects of the original vulnerability model were not replicated in this second sample. Although the sample size of the second sample was of a similar magnitude to that of the first, the results pertaining to early loss of mother and the lack of a confiding relationship failed to reach statistical significance at the 5 per cent level. The results regarding the presence of 3 or more children at home were significant but in the reverse direction.\*

\* It was noted above that Brown and Harris (1978a) attributed special status to lack of employment. Because of this Professor Brown did not provide data pertaining to it.



Replication across these two samples would therefore tend to cast doubts on the stability and the generality of the vulnerability model. Corroboration of the vulnerability model will, therefore, be sought from other parts of Brown's data base. Before considering the patient sample and the two Outer Hebrides samples, further consideration of other parts of the Camberwell data might be fruitful.

Brown and Harris (1978a) distinguished between 'onset cases' and 'borderline onset cases' in their sample. They contended that these different 'cases' were not qualitatively different but rather represented different points on a dimension of severity. Brown and Harris (1978b) described caseness as '...something we have hypothesised as not a categorical distinction but a continuum representing a dimension of some sort of symptomatology'. (p 583).

Brown and Harris (1978a) noted further that 'There is evidently an arbitrary element in choosing a cut-off point between a case and a borderline'. (p 229).

It is clear, therefore, that Brown and Harris viewed 'onset cases' and 'borderline cases' as different in terms of the severity of their disorder, not in the quality of their disorder. It is further clear that there was a degree of arbitrariness in the distinction.

Given that the distinction was one of quantity rather than quality it would be parsimonious to assume that similar etiological processes underpin both levels of 'caseness'. Examination of the information on

the 31 borderline cases might provide another source of replicative support for the vulnerability model. Indeed, Brown and Harris (1978a) used this information for this purpose and concluded, '...the causal model established for onset of clinical depression, therefore, can be enlarged to take account of borderline conditions'. (p 195). This would appear to lend replicative support to the vulnerability model, however, the validity of the arguments leading to this conclusion must be questioned.

Brown and Harris (1978a) did not approach the specification and description of the vulnerability factors in the 'borderline cases', in the same manner as they approached it in the 'cases'. Their conclusion, quoted above, was based on two other forms of argument. Firstly, they argued that women with 'lower' vulnerability (fewer vulnerability factors) were more likely to develop borderline conditions than case conditions. They found that the ratio of 'borderline cases' to 'cases' varied significantly, in the predicted manner, for different levels of vulnerability. Secondly, they argued that if vulnerability factors are operating then the most vulnerable women should be proportionally more frequently represented in the 'onset cases' followed by the 'borderline cases' and then the 'non-cases'. This proposition was found to be statistically significant.

Unfortunately, however, neither of these arguments are necessarily corroborative of the vulnerability model. The same patterns would emerge if vulnerability factors act as independent causal agents. If lack of an intimate relationship acted independently to induce depressive feelings then one would expect more 'cases' than

'borderline cases' in women with this factor. Similarly, one would also expect more 'cases' to be positive on this factor than 'non-cases'. A further difficulty with this approach is that similar findings would emerge if only one vulnerability factor has a large effect.

In order to determine whether the evidence pertaining to the borderline cases corroborated the vulnerability model it is necessary to carry out similar analysis on both sets of data. Brown and Harris (1978a) do not provide the necessary information, however, Professor Brown (Brown, 1980b Personal Communication) kindly provided the necessary data laid out in conventional three factor chi-squared tables (See table 1).

The table, which displays the relationship between 'caseness' and intimacy, strongly exhibits the characteristics that Brown and Harris (1978a) regard as being fundamental to vulnerability factors. That is, in the absence of provoking agents, intimacy does not appear to have any effect on the frequency of 'caseness', while in the presence of provoking agents intimacy appears to have a strong effect. The data for the 'borderline cases' exhibit a similar pattern. There is a significant effect for intimacy in the presence of provoking agents. These results would therefore seem to lend some corroboration to the results obtained for 'cases'. Brown (1979) noted, however, that 'intimacy' is a rather 'soft' measure in that the possibility of unreliability in its measurement cannot be ruled out, particularly in cross-sectional studies. Brown (1979) regarded the other three factors as being 'harder' indicators of vulnerability.

TABLE 1 COMPARISON OF VULNERABILITY DATA FOR ONSET 'CASES' AND ONSET 'BORDERLINE CASES' (COMBINED CAMBERWELL SAMPLES). SOURCES BROWN AND HARRIS (1978) AND BROWN (1980, PERSONAL COMMUNICATION)

	<u>CASES</u>		<u>BORDERLINE CASES</u>	
<u>Provoking Agent</u>	Intimacy		Intimacy	
	Yes	No	Yes	No
Present	10%	32%	8%	35%
Absent	1%	3%	5%	10%
	Loss of Mother		Loss of Mother	
	Yes	No	Yes	No
Present	47%	17%	0%	18%
Absent	0%	2%	0%	7%
	3+ Children at home		3+ Children at	
	Yes	No	Yes	No
Present	43%	17%	9%	18%
Absent	0%	2%	0%	7%
	Employment outwith Home		Employment outwith home	
	Yes	No	Yes	No
Present	17%	24%	20%	12%
Absent	1%	3%	7%	5%

When the three 'harder' indicators are considered for the 'borderline-cases' they fail to provide any replicative corroboration. The pattern for early loss of mother rather than acting as a vulnerability factor acts as a protective factor. The results pertaining to both the presence of children at home and unemployment outwith the home, show a tendency to act as vulnerability factors, however, the results in the 'borderline cases' are the reverse of those for the 'cases'.

At best, it must be argued that the results pertaining to borderline cases, despite Brown and Harris' (1978a) contention to the contrary, do not support the view that their etiological model can be 'enlarged to take account of the borderline cases'. (p 195)

At worst, it must be contended that the validity of their vulnerability factors, even for their Camberwell 'cases', is questionable. The patterns which have emerged may be critically linked to the essentially arbitrary decision regarding the division between 'cases' and 'non-cases' (vide infra).

Further evidence pertaining to the vulnerability model will now be considered. Brown et al (1977) and Brown and Harris (1978a) attempted to generalise the vulnerability model from their Camberwell general population sample to a sub-sample of 114 female psychiatric patients. Within this sample the absence of a confiding relationship acted as a vulnerability factor, but early loss and the presence of young children at home did not (Relevant information on lack of employment outwith the home was not provided). Brown and Harris (1978a) and Brown et al (1977) argued that these discrepancies may be due to

selective factors influencing the process of referral to psychiatric agencies. This contention may be valid, powerful selective factors are known to exist (e.g. Fahy, 1974a, 1974b), however, this further failure to replicate the vulnerability model reduces its external validity.

Brown and Prudo (1981) put the vulnerability model to an even more rigorous test by attempting to replicate the Camberwell findings in a general population sample in the Outer Hebrides. The generality of two of the vulnerability factors was confirmed in a very different socio-cultural milieu. The absence of a confiding relationship and the presence of young children at home acted as vulnerability factors, while lack of employment and early loss did not. In addition, they found that the lack of 'regular church-going' acted as a vulnerability factor on the Island of Lewis. This result did not generalise back to the earlier Camberwell sample.

In summary, while Brown and his colleagues have acted as strong proponents of the vulnerability hypothesis (Brown et al, 1975; Brown and Harris, 1978a; 1978b; 1980) their empirical evidence in support of this hypothesis is not as strong as it might initially appear.

Results from the first Camberwell sample were not replicated in the second. Results from analysis of the borderline cases suggested that the vulnerability factors did not operate in this group, and indeed, there is some evidence to support the view that they have a protective effect for this group. Only the 'softest' vulnerability factor, lack of a confiding relationship, acted as a clear vulnerability factor within the same patient sample. Within the very

different socio-cultural milieu of the Outer Hebrides, lack of a confiding relationship and the presence of young children at home acted as vulnerability factors.

#### Further Evidence in Support of the Vulnerability Hypothesis

While Brown and his colleagues have been the strongest proponents of the vulnerability model, other workers have provided some empirical support for the model.

Paykel et al (1980) examined the role of life events and additional factors in relation to puerperal depression. Their results indicated that the following variables, a poor marital relationship, poor communication with their husband and the absence of help from their husband, following the birth of their child, each acted as vulnerability factors. That is, these variables potentiated the effect of 'undesirable' life events, but had no effect in the absence of life events.

Paykel et al (1980) also examined constructs of a similar, although not identical operational form, to the vulnerability factors identified by Brown et al (1975). Their account of this study suggested that early loss, the lack of employment outwith the home and the presence of young children in the home all acted as vulnerability factors. Their results, however, tabulated in Table 3 (Paykel et al, 1980, p 343), did not appear to be consistent with the vulnerability hypothesis. Paykel (1980, Personal Correspondence) indicated that editing had made the script misleading and that these factors did not

in fact behave as vulnerability factors in this study.

It would appear that within this study the 'softer' aspects of social relationships, those that may be more open to contamination (Brown, 1979), were functioning as vulnerability factors, while the more concrete aspects were not.

Surtees (1980) reported a prospective study on the role of life events in depression. Using a life event interview that was based on the techniques of Brown et al (1975), he examined the association among social support, life events and symptom variables in a sample of depressed patients seven months after the onset of their disorder. Surtees (1980) measured the severity of depression using the Hamilton Rating Scale of Primary Depression (Hamilton, 1960). He examined the roles that the quality of a confidant and the presence of both close and diffuse social support played in relation to life event stress.

Surtees' measurement of the quality of confiding relationships was similar to that used by Brown and his colleagues except in that the most positive rating required reciprocity in disclosure between patient and confidant. The index of close social support was composed of variables relating to the existence of a confidant, presence of contact with close relations and the presence of contact with members of the living group. The index of diffuse support included variables relating to the presence of work contacts, contact with neighbours and contacts through attendance at clubs or church meetings.

Surtees (1980) espoused the vulnerability view as his theoretical



position and thus regarded the vulnerability hypothesis as the null hypothesis which he attempted to refute for each of the three measures of social support. He indicated that the results pertaining to both the confidant measure and the measure of close social support provided a satisfactory fit with the vulnerability hypothesis in statistical terms. He noted, however, that both a logistic and an additive model also provided a satisfactory fit to the results. Surtees (1980) pointed out that when statistical models all fit results equally efficiently then decisions regarding which to accept must be based on criteria other than statistical criteria. On this basis, he contended that his findings do not allow the rejection of the vulnerability hypothesis for two of the three social support variables and thus it must be regarded as providing '...a satisfactory statistical and socio-psychological fit to the data' (p 67).

Roy (1978, 1981) in a pair of papers that misleadingly refer to vulnerability factors claimed to corroborate the findings of Brown and his colleagues that unemployment, lack of confiding relationship and early parental loss acted as vulnerability factors in relation to depression. However, (Cooke 1981b) Roy, in fact, failed to collect any information pertaining to the experience of life events or long term difficulties, and therefore cannot legitimately claim on the vulnerability hypothesis.

Campbell's (1982 Personal Communication) study of working class women with at least one child at home, is of particular interest, as it attempts to identify vulnerability factors in a group selected for

their high risk of depression, using procedures virtually identical to those of Brown and his colleagues. In her preliminary account Campbell (1982, Personal Communication) provided clear support for the vulnerability model in respect to the lack of a confiding relationship. With regard to lack of employment outwith the home and the presence of three or more young children at home, Campbell's results did not conform to the vulnerability hypothesis. Loss of mother before the age of 11 was too infrequently represented in the sample to be amenable to analysis.

#### Empirical Support for the Independent Causes Hypothesis

The work of Miller and Ingham (1976, 1979) and Miller et al (1976) is of particular interest with regard to this discussion on additional variables because their life event interviews were developed from those of Brown and his colleagues and were applied in comparatively large samples.

Miller and Ingham (1976) reported the results obtained from a sample of 337 subjects. Half of the sample was composed of people who had consulted their general practitioner in the previous seven days and the other half was composed of patients, of the same age and sex, as the consulters, who had not visited their General Practitioner in the previous three months. Miller and Ingham (1976) found that the availability of a good confidant was associated with lower levels of depression, anxiety and tiredness. They also found, in contradiction to Brown et al's (1975) negative findings, that the availability of casual acquaintances was associated with lower levels of depression,

tiredness, palpitations and breathlessness.

Miller and Ingham (1976) carried out Brown's life-event interview with a sub-sample of sixty-eight people. They prefaced their remarks on this information with the caveat that this small size precludes firm conclusions. They noted that their social support variables appeared to provide partial protection against the elevation of symptomatology that is attributable to life events. They indicated, however, that the social support variables varied systematically with symptom levels even in the absence of life events. This finding suggested that the independent causes model might be the most appropriate model for these results. Miller and Ingham (1976) identified this point of conflict between their results and those of Brown and his colleagues. In attempting to reconcile their results with Brown's vulnerability model they suggest that differences in the results might be attributable to differences in the levels of symptom severity sampled. While their explanation is plausible, their results and conclusions merely serve to highlight the lack of generality of the vulnerability model.

Miller and Ingham (1979), extending their earlier work, reported on a larger sample of 1060 individuals half of whom were G.P. consultants the other half their controls. All subjects were interviewed using Brown's procedures for assessing life events and the authors examined their results for evidence of vulnerability effects. On examining the associations between a subject's sex and their level of symptomatology, they detected the oft reported higher levels of symptomatology in female subjects. They also found the experience of

stressful life events was associated with high levels of symptomatology. They failed to detect any significant interaction between the subject's sex and their experience of life events and, therefore, this tended to suggest that the results were consistent with the independent causes model.

Miller and Ingham (1979) quoted further results and these will be considered in respect to the synergy hypothesis.

Andrews et al (1978a, 1978b), reporting on a large epidemiological study, examined the role that life events and a variety of additional variables played in relation to psychiatric caseness as measured by the General Health Questionnaire (GHQ). They analysed their results using interaction chi-square procedures and showed that crisis support, psychological coping style, social support variables and also life events, had direct relationships with the dependent variable. They did not detect any interaction between life events and their additional variables and thus their results tended to corroborate the independent causes model.

Lin et al (1979) in a study of a Chinese-American community examined the relationship between social support, life events and the level of psychiatric symptomatology reported. Using multiple regression techniques they demonstrated that life event measures and social support measures were independently associated with psychiatric symptomatology and that there were no significant interaction effects.

Lin et al (1979) concluded that their analyses failed to support the view that the impact of life events were modified by the availability of social support. The results tended, therefore, to corroborate the independent causes hypothesis.

Scott Henderson and his colleagues have highlighted the importance of social relationships in relation to psychiatric symptomatology. They painstakingly developed the Interview Schedule for Social Interaction (ISSI) in order to measure 'the availability' to the respondent of a variety of social relationships and also the 'perceived adequacy' (i.e. respondent's satisfaction) of these relationships. Henderson et al (1980) used the Present State Examination (PSE) to estimate the point prevalence rate of psychiatric caseness in a large systematic sample (n = 756) of the general population. Using the ISSI they measured social relationships in terms of 'availability' and 'perceived adequacy' of attachment to significant others. They assessed the respondent's exposure to life events using a 71 item self-report inventory.

Upon analysing their results, Henderson et al (1980) detected clear sex differences in the association among life events, measures of social relationships and psychiatric 'caseness'. Within their male sub-sample, the level of 'caseness' was significantly associated with the experience of life events while the 'availability' of attachment was not. In contrast, both these variables were significantly associated with 'caseness' in the female sub-sample. It should be noted, however, that there was no evidence of a significant

interaction effect and that this result tended to corroborate the independent causes hypothesis. When they examined the role of 'adequacy' rather than 'availability' of attachment, they found that both life events and 'adequacy' were significantly and independently associated with 'caseness' in men. Another sex difference emerged, however, in that these variables were not only independently associated with 'caseness' but they also interacted. This result tended to corroborate the synergism hypothesis.

Henderson and his colleagues also examined the associations amongst social relationship measures, life events and scores on the Self-Rated Depression Scale (SDS) (Zung, 1965). In the male sub-sample, life events and each of the two support measures were independently associated with the Depression Scale with an interaction being absent. These two analyses tended to corroborate the independent causes hypothesis. In the female sub-sample, however, life events and each of the support measures were not only independently associated with the Depression Scale, but also, they interacted significantly. Within the female sub-sample, therefore, the results tended to corroborate the synergism hypothesis.

To summarise the analyses of Henderson et al (1980), it could be argued that only four were consistent with the independent causes hypothesis. Three of the analyses, all of which were based on the female sub-sample, were consistent with the synergism hypothesis. One analysis failed to demonstrate any effect of the additional variable on 'caseness' in men. Not one of these analyses corroborated the vulnerability hypothesis

Solomon and Bromett (1982) examined the role of Brown's vulnerability factors in a sample of 435 mothers who lived in semi-rural areas of Pennsylvania. The presence of affective disorder was determined using a modified version of the Schedule for Affective Disorders and Schizophrenia Lifetime Versions (SADS-L). Solomon and Bromet (1982) were concerned with episodes of depression and/or anxiety that occurred in the year prior to the interview. Life events were measured using brief self-report life event schedules.

Loss of mother before the age of 11 did not occur sufficiently frequently in this sample to allow analysis. Analysis of the associations between affective disorders, life events and each of the other three vulnerability factors using a log-linear analysis, failed to provide support for either the independent causes hypothesis or the vulnerability hypothesis. When log-linear analysis was performed with the number of vulnerability factors as the additional variable the results were consistent with the independent causes hypothesis.

Costello (1982), using a Canadian sample, attempted a '...procedural replication of the Camberwell retrospective community study.' (p 329). Four hundred and forty-nine women were interviewed using the P.S.E. and the Camberwell Life Event Interview. Life events were associated with the onset of depression, however, of the four vulnerability factors, only lack of a confiding relationship was associated with an increased risk of depression. When the associations among life events, intimacy and depression were analysed in the manner of Brown et al (1975), it appeared that they were not consistent with the

vulnerability model. Indeed, the lack of a confiding relationship only had a significant effect in the absence of life events. This result was directly in opposition to those of Brown and his colleagues. Intimacy was found to be associated with depression in its own right and its failure to interact with life events tended to corroborate the independent causes hypothesis.

#### Empirical Evidence in Support of the Mutual Potentiation Hypothesis

The mutual potentiation hypothesis implies that neither the presence of life events nor the presence of the additional variable is sufficient to elevate the level of psychological disturbance. It is necessary that both should be present for the elevation of psychological disturbance. There is little evidence available to support this view, however, it is considered here because an early, influential and oft quoted study supported this view. Nuckolls et al (1972) reported a study of the influence of life events and social support in relation to birth complications. Analysis of information, obtained from primigravida using self-administered questionnaires failed to provide support for any direct association between either psycho-social assets or life events and a range of birth complications. They claimed, however, that life events and psychosocial assets interact to increase the frequency of birth complications in a manner that would be consistent with the mutual potentiation hypothesis.

#### Empirical Evidence in Support of the Synergism Hypothesis

The synergism hypothesis can be distinguished from the mutual potentiation hypothesis in that the presence of either life events or



the additional variable is sufficient to lead to increased psychological disturbance. The synergism hypothesis can be distinguished from the independent causes hypothesis in that the concatenation of life events and the additional variable leads to a disproportionate increase in psychological distress.

In the discussion above on the independent causes hypothesis, it was noted that several workers had found evidence that supported the synergism hypothesis. Miller and Ingham (1979) reported that the availability of a confidant was associated with the level of depression reported by subjects at every level of life event stress. The availability of a confidant and life event stress interacted, however, to disproportionately increase the level of depression reported. Miller and Ingham (1979) were also able to corroborate their earlier findings that the availability of diffuse support was associated with the level of anxiety and depression reported by their respondents. In addition, life events and diffuse support interacted to produce disproportionately high levels of both anxiety and depression. These results tend to corroborate the synergism hypothesis.

Further empirical support for this position was noted above in the work of Henderson and his colleagues. They indicated that within their female sub-sample, life events and 'adequacy' of attachment showed first order effects and an interaction effect when the level of 'caseness' was the dependent variable. The synergism hypothesis was further corroborated when life events and both 'adequacy' and 'availability' of attachment were considered with the Self-Rated

Depression Scale (S.D.S.; Zung, 1965) as the dependent variable.

There would appear to be some evidence to support the view that while an additional variable may have some influence irrespective of the level of life events experienced by a respondent, its influence in the presence of high life stress is considerable. It may be important to note that these results, in support of this relatively complex hypothesis, come from large scale studies. A large sample size may be necessary to demonstrate these higher order effects.

#### VERBAL AND STATISTICAL DESCRIPTIONS OF HYPOTHESES

The preceding account of the empirical support for each of the four hypotheses was based on simple and primitive definitions of each of the four hypotheses. As was noted above, adequate evaluation of the empirical evidence was hampered by variability in the verbal and statistical descriptions adopted by individual workers. Disputes about the verbal and statistical definitions of two of the hypotheses, the vulnerability hypothesis and the independent causes hypothesis, have generated considerable debate (Duncan-Jones, 1976; Tennant and Bebbington, 1978; Bebbington, 1980; Shapiro, 1979; Brown and Harris, 1978a, 1978b, 1980; Duffy, 1978; Everitt and Smith, 1979; Harre, 1980; Tennant and Thompson, 1980; Cooke, 1980b). This debate will now be considered with a discussion of the verbal accounts being followed by a discussion of the statistical accounts.

### Verbal Descriptions of the Four Hypotheses

Brown et al (1975) in their initial account of vulnerability factors were content to specify vulnerability factors in comparatively simple terms as '...factors which increase the chances of developing a psychiatric disorder in the presence of the event or difficulty but have no effect in their absence.' (p 243). Brown and Harris (1978b) expanded and clarified this position by arguing that vulnerability factors do not produce depression but they act to increase the risk of developing depression in the presence of provoking agents. Brown (1979), in outlining a proposed causal model, contrasted the distinctive functions of the different types of variables by contending that provoking agents (i.e. life events and long term difficulties) tell us when someone will develop depression, while vulnerability factors tell us who will break down. Particular variables need not necessarily always function in one fashion or the other. Brown and Harris (1978a) argued that employment may act as a vulnerability factor because of its correlates with isolation and boredom, while it may also act as a provoking agent because of the experience of sudden loss.

This comparatively simple verbal description was perhaps first criticised in detail by Tennant and Bebbington (1978) and Bebbington (1980). Tennant and Bebbington (1978) described vulnerability factors in more detail and attempted to enumerate their defining characteristics. They argued that vulnerability factors should have the following relationships with other variables:

1. They should not be separately associated with the disorder variable.

2. They should be conceptually distinct from provoking agents and have no independent association with them.
3. They should show a positive relationship with the disorder variable only in the presence of a provoking agent.

Bebbington (1980) developed the above outline and added two additional necessary relationships to his list:

1. The association between provoking agents and the disorder variable ought to be stronger in the presence of the vulnerability factors.
2. Provoking agents and vulnerability factors should only be associated in the cases.

Tennant and Bebbington (1978) and Bebbington's (1980) concept of a vulnerability factor is thus more rigorous and restrictive than that developed by Brown et al (1975, et seq.). Brown and Harris (1980) have argued vigorously that the second additional relationship is unnecessary because of the overwhelming impact of provoking agents. Bebbington (1980) highlighted the specific characterising features of the vulnerability models by contrasting the features of two other models with which it could be confused. These other models, they termed the independence model (separate provoking agents with no joint effect) and the synergy model (separate provoking agents with a joint effect).

Bebbington (1980) contended that the conditional independence model should possess the following associations:

1. The associations between the first factor and the disorder variable should be similar in both the presence and absence of the second factor.
2. Similarly, the association between the second factor and the disorder variable should be similar in both the presence and absence of the first factor.
3. The first and second factors should not be associated in the cases or the non-cases.

In contrast, Bebbington (1980) argued that the synergy model should have the following characteristics.

1. The association between the disorder variable and the first factor should be greater in the presence of the second factor.
2. Similarly, the association between the disorder variable and the second factor should be greater in the presence of the first factor.
3. The first and second factors should be positively associated in the cases and the non-cases.

Tennant and Bebbington (1978) and Bebbington (1980) have therefore developed rather more detailed, specific and complex formulations of three, out of many, possible verbal formulations of the interrelationships among provoking agents, additional factors and disorders. These formulations contrast markedly with the comparatively simple verbal formulation of a vulnerability factor detailed by Brown et al (1975).

The core of debate regarding the appropriateness of these models has taken place within the statistical domain and it centres on appropriate procedures for parameterising the respective models. This statistical debate will now be briefly considered.

### Statistical Accounts

In the above evaluation of the empirical support for each of the four hypotheses, one difficulty was the already mentioned disparity in operational definitions, while a second difficulty was the different approaches used in statistical analysis. Lin et al (1979) contended that the majority of life event studies have relied on comparatively crude analytic techniques, generally in the form of tests of percentage differences or differences between means. They considered that the use of correlational methods, methods that they advocated, was comparatively rare. Their contention was probably accurate, nonetheless, it masked the diversity of tests that have been used.

Brown et al (1975) compared the significance of the chi-squared statistic for the association between caseness and the vulnerability factors in subjects who had not experienced an event and in those who had experienced one or more events. Andrews et al (1978a, 1978b) and Jenkins (1979) also relied on the chi-squared statistic, however, they used the more sophisticated interaction chi-squared techniques developed by Sutcliffe (1957).

Duncan-Jones (1976), Tennant and Bebbington (1978), Bebbington (1980) and Russell (1978), however, have questioned the adequacy of chi-squared techniques for the analysis of multi-dimensional contingency

tables. They have argued for the application of log-linear analysis in the evaluation of these tables. Surtees (1980) has applied both a logistic and a weighted least squares model to the analysis of data in this format.

Miller and Ingham (1976) used analysis of variance while Johnston and Sarason (1978b) used partial correlations. Garrity et al (1977a), Kessler (1979) and Henderson et al (1980) applied simple Multiple Regression techniques. Lin et al (1979), building upon Multiple Regression techniques, presented their data in the form of a simple linear additive three variable path model.

Thus an extensive range of techniques has been applied to the analysis of the simple three variable models that are favoured in the life event literature. The variety of techniques applied has inevitably led to confusion. Cross-study comparability has been diminished and has been the reason for some of the discrepancies in the interpretation of the empirical findings noted above. The deficiencies and merits of the principal techniques will now be evaluated.

#### (1) Analyses based on Contingency Tables.

Epidemiological data is frequently presented in the form of contingency tables. Within the life event literature these tables have been analysed using the conventional chi-squared statistic, more complex partitioned and interaction chi-squared statistics and ultimately log-linear analysis.

It was noted above, that Brown et al (1975) evaluated the validity of their vulnerability model by examining the difference in association between 'caseness' and the vulnerability factors for two different levels of provoking agents. Bebbington (1980), however, contended that this approach to the evaluation of the vulnerability model is inadequate. While Bebbington's paper has been heavily criticised regarding its outline of philosophical issues (Tennant and Thomson, 1980), Harre (1980), a noted authority on scientific method, concurs with this critique of Brown et als' analyses.

Bebbington's (1980) principal criticism of Brown et als' (1975) approach was that it involved evaluation of the data after the subjects had been partitioned solely in terms of one factor. Brown et al (1975) examined the associations between caseness and each of the four proposed vulnerability factors for the different levels of provoking agents. It was noted above, in the section on verbal formulations of models, that Tennant and Bebbington (1978) and Bebbington (1980) have demonstrated that the evaluation of the adequacy of both models requires the sequential evaluation of a set of associations among the variables. The thrust of Bebbington's argument was that Brown and his colleagues, by using only one particular partitioning of their data, were unable to evaluate all the relevant characterising associations for each of the models. Bebbington indicated that a full evaluation of the models required that the data should not only be partitioned in the original format but also in terms of the presence or absence of the disorder and the presence or absence of the various vulnerability factors. Bebbington (1980)



carrying out this strategy, re-analysed the data of Brown and Harris (1978a) after partitioning it in terms of the other two variables. The original partitioning clearly supported the vulnerability model, however, when the association between provoking agents and a vulnerability factor was compared for cases and non-cases the comparison was consistent with the independent causes model. In summarising his analysis and argument Bebbington (1980) stated, '...it is in fact possible simultaneously to refute and to confirm predictions supporting a single model in a single set of data using the method of Brown and his colleagues of partitioning contingency tables (p 324)'. On the basis of this observation he contended that all the predicted associations generated by each of the opposing models ought to be tested in turn. He considered, however, that the traditional procedures for partitioning  $2 \times 2 \times 2$  contingency tables were inadequate for two reasons. Firstly, because there are no adequate measures of differences between chi-squared values, and secondly, because the procedure of testing each of these different predictions can lead to irreconcilably ambiguous results. Tennant and Bebbington (1978), Bebbington (1980) and Russell (1978) following Duncan-Jones (1978) suggested that log-linear analysis would be the most appropriate form of technique to apply to data in this format. The nature of this technique will be briefly outlined, this being followed by an assessment of its advantages and disadvantages in this context.

## (2) Log-linear analysis and three factor models: its application

Duffy (1978) indicated that log-linear analysis, as applied to contingency tables, is analogous to multiple regression analysis or analysis of variance techniques. In contrast the traditional chi-squared statistic is used to determine whether there is an association between two factors. The information thereby obtained is analogous to that provided by a correlation coefficient. While the chi-squared statistic provides an estimate of the association between factors, Duffy (1978) indicated that log-linear analysis involves an explicit parametrisation of the problem, a parametrisation which relates true (expected) values to the 'effects' of the row and column factors and their mutual interaction.

The value of this approach is most clearly evident in the analysis of higher order contingency tables such as the  $2 \times 2 \times 2$  tables used by Brown and Harris (1978a). The analysis of these tables is made complex by the variety of possible forms of departure from the null hypothesis of independence. For example, it was noted above that Bebbington (1980) had demonstrated that analysis of one particular partitioning of the data may appear to corroborate one model while another partitioning may serve to refute it. The use of conventional chi-squared techniques does not allow the evaluation of the independent influences of each of the factors. Thus a pair of factors may be associated merely because of their mutual association with a third variable. The use of log-linear analysis can tease out situations of this type, situations which are generally termed

spurious correlations (Brown 1974, Susser 1973).

Duffy (1978), noting the difficulties usually encountered in the evaluation of three dimensional contingency tables, argued that the use of log-linear analysis may facilitate the interpretation of these tables. The procedure will now be outlined (Reynolds, 1977).

Applying the log-linear technique to a three dimensional contingency table, the first step is to test the independence of the three factors. If the null hypothesis of independence is rejected the next step is to determine whether the data can be adequately fitted by an association between one pair of factors. If this fit is unsatisfactory then the fit for an association between two pairs and ultimately three pairs of factors is assessed.

Duncan-Jones (1976) and Tennant and Bebbington (1978) used this technique to evaluate Brown et al's (1975) data, while Duffy (1978) and Bebbington (1980) used it to reanalyse data from the larger sample provided by Brown and Harris (1978a). Their analyses resulted in the following conclusions:

1. Rejection of the null hypothesis i.e. independence.
2. Rejection of the three models of association between only one pair of factors.
3. Rejection of the three models of association between exactly two pairs of factors.
4. Acceptance of the model of association between all three pairs of factors.

The use of this procedure circumvents the principal interpretative difficulties as outlined by Bebbington (1980). This particular set of conclusions was interpreted as indicating that each factor is significantly related to each of the other two factors and that the extent of the relationship is similar for each level of the third factor. In terms of the analysis of the data of Brown and his colleagues, this analysis may be interpreted as suggesting that provoking agents and vulnerability factors each have independent associations with depression. However, the strength of the association between depression and provoking agents is not influenced by the presence or absence of the vulnerability factor. This interpretation would therefore seem to be consistent with the independent causes model.

(3) Log-Linear analysis and three factor models: advantages and disadvantages

The use of log-linear analysis has several advantages over the use of traditional chi-squared procedures. Duncan-Jones (1976) and Tennant and Bebbington (1978) favour its use in the analysis of contingency tables because it allows the sequential testing of progressively more complex models. Duncan-Jones (1976) indicated that it not only allows the formulation and evaluation of complex hypotheses, but also, it forces one to decide whether complex hypotheses are in fact necessary or desirable.

Duffy (1978) favours the procedure on the practical grounds that it involves a direct method of fitting and a direct test of the adequacy of any fit.

Given those advantages, the method has certain significant disadvantages. Perhaps the principal disadvantage lies in the interpretation of the results.

Historically, explanations in the social sciences have been couched in terms of linear additive models (Stolzenberg, 1980; Everitt and Smith, 1979). The use of log-linear analysis, however, represents a trend, noted by Everitt and Smith (1979) towards the use of multiplicative models in the evaluation of multidimensional contingency tables.

Given a 2 x 2 contingency table of the form

a	b
c	d

Caltung (1967) indicated that an additive interaction is defined as  $(a + d) - (b + c)$  whereas the multiplicative interaction is given by the cross-product ratio  $\frac{ad}{bc}$ .

Everitt and Smith (1979), in a detailed account of the differences between the two forms of interaction, indicated that additive models are concerned with differences between proportions whereas a multiplicative model deals in ratios of proportions. This distinction is well illustrated by the dispute between Brown and Harris (1978b) and Tennant and Bebbington (1978). Brown and Harris' (1978b) analysis would appear to support one of the critical features of the vulnerability model because of the substantial difference in the differences of proportions (0.32 and 0.03) and (0.1 and 0.01) in those with events compared to those without events.

Tennant and Bebbingtons (1978) analysis, however, based on a multiplicative model would seem to be consistent with the independent causes model because of the similar ratios of proportion (0.32 and 0.03) and (0.1 and 0.01). Thus the interpretation put upon a particular analysis of this particular partitioning of the data depends, as Duffy (1978), Everitt and Smith (1979) and Duncan-Jones (1976) have indicated, on the model considered to underly the data. The multiplicative model, although it provides a parsimonious description of the data, has the disadvantage that it is unfamiliar to most social scientists. It is impossible to use statistical criteria to distinguish between the two approaches and two models and ultimately the choice between two models which appear to fit the data equally well must be decided on in terms of taste.

A further problem in the use of log-linear analysis is that there are difficulties in distinguishing between different types of second order interaction. Tennant and Bebbington (1978) argued that it is not possible to distinguish among different types of second order interaction, that is, between models of synergy, mutual potentiation or vulnerability. This is a fairly fundamental disadvantage, in that, if analysis indicates that a second order interaction is required then there are difficulties in determining whether the vulnerability model is corroborated.

Surtees (1980) extended this theme and argued that the conventional logistic model does not represent the variables in accord with a vulnerability model and thus it cannot be used to determine whether or

not the vulnerability model provides a satisfactory socio-psychological fit to any data.

Fienberg (1977) indicated that causal analysis based on logit and log-linear models is deficient because of the absence of a 'calculus' that allows the estimation and comparison of the magnitude of direct and indirect effects. Brown and Harris (1978b) and Tennant and Bebbington (1978) concur that such 'calculus' is necessary.

The case in favour of log-linear analysis does not appear to be overwhelming. It may be, as Everitt and Smith (1979) and Duffy (1978) contend, that the use of these techniques rather than conventional chi-squared techniques must be based on questions of taste.

This may be true for particular forms of data, however, the appropriateness of any form of categorical approach for causal analysis in this field of research, must now be considered.

#### PROBLEMS OF CAUSAL MODELLING WITH CONTINGENCY TABLES

The above discussion has outlined some of the problems involved in the analysis of multi-dimensional contingency tables. In the discussion, it was concluded that there are no adequate methods for distinguishing between the two forms of statistical interaction thought to underlie

the data and that any decision must be made on the grounds of taste.

Thus, in this section a more fundamental question is posed, whether causal modelling is possible with data which are tabulated in dichotomous contingency tables.

In attempting to make causal statements Bebbington (1980), amongst others, has attempted to apply the principles of formal logic to data. He argued that the 2 x 2 contingency tables, commonly used in psychiatric epidemiology represent the relationship between two variables in a manner analogous to the truth tables of formal logic. It was noted above that he argued that the use of log-linear analysis could lead to the corroboration of one causal model rather than another.

Nowack (1960, 1976), Blalock (1964), Reynolds (1977), Cook and Campbell (1979) and Cooke (1980b), however, have indicated that causal analysis of empirical data, based on the principles of truth tables, is fraught with difficulties. Both truth tables and the contingency tables with which we are concerned depend on dichotomous classifications. Pearson (1957) argued that dichotomisation is often merely a crude procedure for handling continuous variables. Blalock (1964) indicated that, while it is still technically possible to think always in terms of dichotomies, difficulties may arise because they are frequently the result of an essentially arbitrary cutpoint in some quantitative variable.



Blalock (1964) illustrated these difficulties. Given two continuous variables, one a hypothetical causal agent and the other its postulated effect then the best fitting curve, be it linear or non-linear, represents a continuous causal function linking the postulated cause and effect.

For the purpose of illustration the regression line is plotted in Fig. 2, sample fluctuation being ignored. Examination of fig. 2 illustrates that our decision regarding the arbitrary point at which to dichotomise the variables dramatically influences the entries in the truth table and the resulting causal inference made.

If fortuitously the intersection of our dichotomising lines falls precisely on the regression line then Fig. 2a results. When the cause is absent the effect is also absent, when the cause is present the effect is present. The resulting truth table implies that the cause is both necessary and sufficient.

If, however, the intersection of the dichotomising lines crosses above the regression line then different entries appear in the truth table (Fig. 2b). In this case when the cause is absent, the effect is absent, when the cause is present, however, the effect only appears spasmodically. The resulting truth table suggests that the cause is necessary but not sufficient. When the intersection of the dichotomising lines falls below the regression line a new set of

ILLUSTRATION OF EFFECT THAT ARBITRARY DICHOTOMISATION  
OF CONTINUOUS VARIABLES HAS ON CAUSAL INFERENCE

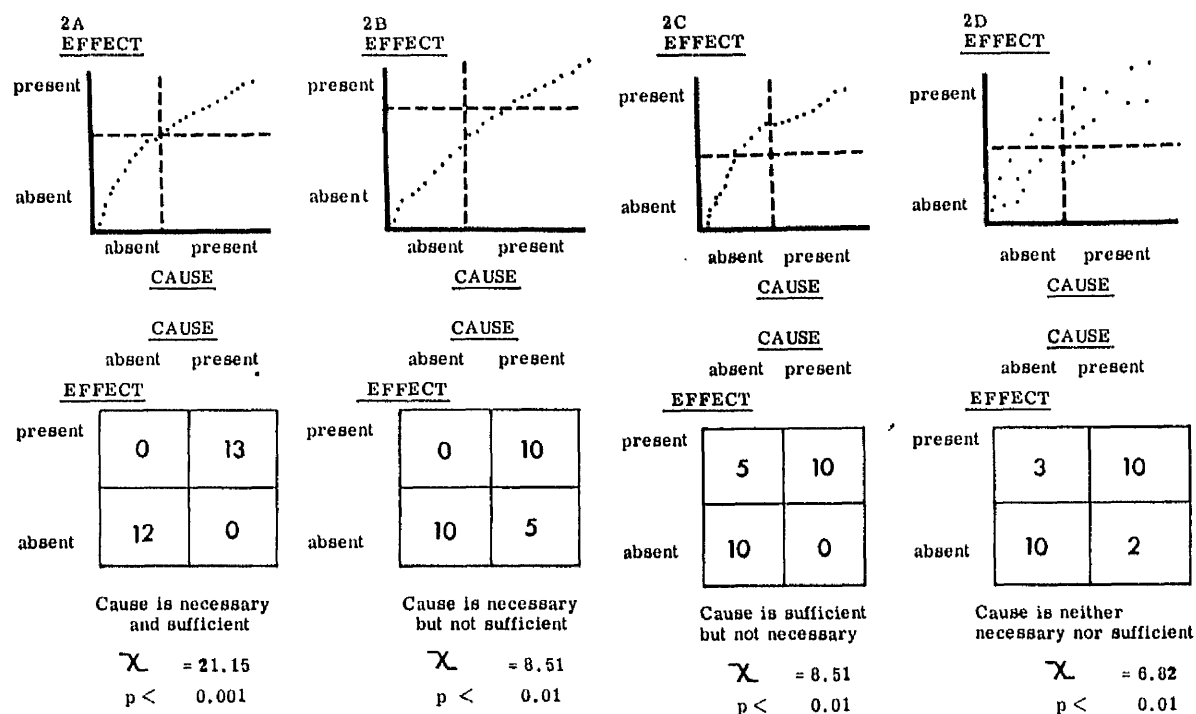


FIGURE 2. ILLUSTRATION OF THE EFFECT THAT ARBITRARY DICHOTOMISATION OF CONTINUOUS VARIABLES HAS ON CAUSAL INTERFERENCE.

entries is placed in the truth table. When the cause is absent the effect appears spasmodically, however, when the cause is present the effect is always present. This truth table implies that the cause is sufficient but not necessary (Fig.2c).

Sampling fluctuations and other factors entailed in any empirical distribution will tend to result in truth tables of the form fig. 2d. This table, the most common form in psychiatric epidemiology (Susser, 1973), suggests that causes are neither necessary nor sufficient.

It must be contended, therefore, that the entries in tables and the inferences made from them are critically influenced by essentially arbitrary decisions regarding the point of dichotomisation of continuous cause and effect variables. Even given that empirical data will inevitably result in truth tables without empty cells, the relative proportion of the entries in the cells critically affects inferences made about causal processes.

The above argument clearly entails the assumption that cause and effect are best conceptualised as continuous variates and not attributes. The validity of this assumption in the context of the example used by Bebbington will be considered.

Bebbington applied his principles of analysis to the data published by Brown et al (1975). Brown et al (1975) initially considered the link between the experience of life-events (postulated cause) and 'caseness' (postulated effect).

Brown and his colleagues dichotomised their sample into those subjects who had experienced no provoking agents and those subjects who had experienced one or more provoking agents. The critical question is whether the relationship between provoking agents and 'caseness' is continuous or all or none; that is, for example, is the risk of becoming a case greater for a subject experiencing three provoking agents than for a subject experiencing one provoking agent. Brown and Harris (1978a), referring to this property of the relationship as additivity suggested that their data supported the absence of an all or none effect even although '...our method of measuring threat probably rules out finding an 'additive effect' should one exist'. (p 109). Miller and Ingham (1979) and Surtees and Ingham (1980), using similar threat measures, on different populations, identified similar 'additive' effects. This would tend to imply that the hypothetical causal variable, i.e. number of provoking agents, does not act in an all or none fashion and it may, therefore, tend towards a continuum. Dichotomising the number of provoking agents experienced by subjects into zero and non-zero probably involves an essentially arbitrary division of a continuous variable. Brown and his colleagues regarded psychiatric 'caseness', derived from the Present State Examination, (Wing et al, 1977), as their 'effect' variable. The use of the concept of 'caseness' might superficially tend to suggest a qualitative rather than a quantitative distinction between cases and non-cases. Closer examination of Brown and his colleague's position, however, indicates that they view 'caseness' as a continuum and not as a qualitative distinction. Brown and Harris (1978b) described caseness as '...something we have hypothesised as not a categorical

distinction but as a continuum representing a dimension of some sort of severity of symptomatology' (p 583). This approach is consistent with that described and advocated by Williams et al (1980) in their review of case definition problems in psychiatric epidemiology. They contended that 'caseness' should be viewed as a continuum with psychiatric patients acting as a 'caseness' criterion and the case-finding instrument measuring the subjects closeness to this criterion. This view is consistent with the dimensional view of depressive disorders (Eysenck, 1970; Cooke, 1980a).

The effect variable, used by Brown and his colleagues, would appear to be continuous in quality. The relevant question in this context then is whether the 'case' v 'non-case' distinction is arbitrary. Brown et al (1975) confirmed that it is to some extent arbitrary, '...There is evidently an arbitrary element in choosing a cut off point between a case and a borderline (case)'. (p 229).

There thus appears to be clear support for the view that the 'cause' and 'effect' variables used by Bebbington in his examples are essentially arbitrarily dichotomised continuous variables. His approach to causal analysis is therefore subject to the deficiencies demonstrated above. While he has made a valuable contribution to the theoretical description of vulnerability factors, his use of contingency tables does not appear to provide a reliable way of parameterising his verbal formulations. Blalock (1964) commenting on their use in causal analysis stated, 'The simplicity and other obvious advantages of 2 x 2 tables should not blind us to its defect'. (p 124).

The use of chi-squared procedures or log-linear analysis would not appear to provide a useful technique for causal analysis, as distinct from a descriptive analysis, of this form of data.

A different approach to causal analysis will now be considered.

#### MODELLING THE EFFECT OF AN ADDITIONAL VARIABLE

Dunn (1981) recently emphasised the usefulness of mathematical models in the understanding of causal processes in psychiatric epidemiology. He argued that psychiatric disorders, despite their multi-causal nature, could be given a more explicit and precise description through the use of mathematical models. The understanding of causal processes is traditionally developed through the use of experiments (Campbell and Stanley, 1966). The application of traditional experimentation is precluded from psychiatric epidemiology by ethical and practical difficulties, however, mathematical models may provide a useful alternative approach to the problem (Heisse, 1968).

Mathematical models have been used extensively in the social sciences (e.g. Economics, Wold and Jurreen, 1953; Sociology, Blalock, 1964; Psychology, Wertz and Linn, 1970). They may be regarded as oversimplified analogues of reality in that they subsume only a limited number of variables and their inter-relationships out of the total infinite universe of social reality (Land, 1968).

#### Advantages of Mathematical Models

The use of mathematical models in the analysis of survey data has three general advantages.

Firstly, as Dunn (1981) indicated, their application may provide a more complete and thorough understanding of the data providing detailed explanation of the patterns found in the sample. For example, while conventional significance testing may indicate that the experience of stressful life events have an influence on depressive disorders, the construction of a model would allow estimation of the strength and magnitude of the effect of events on the disorder. Koopman (1977) indicated that epidemiology has moved from simply implicating a factor as a cause of disease to measuring its contribution. Mathematical models can be used in the assessment of effect magnitudes in complex multi-variable situations. Thus, models can be used to explore sub-group variability in the magnitude of effects. For example, the influence of life events on depressive symptoms with different levels of socio-economic status can be evaluated, determining whether life events have a uniform influence for all social strata.

Secondly, mathematical models, designed to be analogues of reality, can be applied to predict changes in the dependent variable following a change in an independent variable. For example, a model could be used to estimate the proportion of the population that would have a clinical level of depression if the overall frequency of life events was reduced by 15 per cent.

The third, and perhaps the most important advantage of a mathematical model is that its use constrains the investigator to make explicit non-ambiguous statements regarding theoretical constructs and presumed causal mechanisms. Their use can highlight inconsistencies of

argument and reveal unwarranted assumption (Blalock, 1964; Dunn, 1981).

#### Developing an Appropriate Model

It was argued above that four hypotheses, pertaining to the influence of additional variables on the simple event-syndrome link, are discernible in the life event literature. Duncan-Jones (1976) indicated that if a sufficiently precise hypothesis was available then a theoretically appropriate model could be specified precisely. Unfortunately, however, where the imprecision of theory leads to the possibility of four models, it is necessary to carry out comparisons amongst models. Given the necessity of comparison a suitable technique must be adopted.

It was noted above that the analytic techniques which have been applied to the comparison of models do not satisfactorily distinguish between the four hypotheses. Another approach to the comparison of these hypotheses is required and a possible causal approach will now be described.

Stolzenberg (1980) clearly established the principal problems inherent in the development of a causal model that will allow such a comparison, '...one must find a mathematical model that corresponds to the theoretical notions of how the variables in the model affect the dependent variables, and one must find an accurate, understandable measure of the way in which causal variables affect the dependent variable.' (p 471).



No clear definition of 'how the variables in the model affect the dependent variable' emerged from the discussion of the theoretical aspects of the hypotheses. One of the principal areas of disagreement was about the definition of an interaction, that is, whether an additive or a multiplicative definition should be adopted. The adoption of one or other definition is probably a matter of taste (Duffy, 1978). In developing this current model for a comparison a deliberate decision was made to adopt the additive definition of an interaction. This decision was based on two considerations. Firstly, Brown and Harris (1978b) espoused this definition and an attempt will be made to replicate their results below. Secondly, despite gradual changes, most social scientists are only familiar with the additive definition (Everitt and Smith, 1979).

That Brown and Harris (1978b) adopted an additive definition is clear from both their discussion on vulnerability factors and Everitt and Smith's (1979) discussion of different types of interaction. Brown and Harris (1978b) noted that '...a minimal requirement of a vulnerability factor is that it must show interaction when considered with provoking agents and onset of depression.' (p 585), and further '...we view interaction as what is left when the independent effects of a and b are subtracted from the effect that a and b (the independent variables) have on c (the dependent variable) when they occurred together.' (p 585). This description conforms to the common verbal description of an additive interaction but they further emphasised their position by quoting Galtung's (1967) formal mathematical definition of an additive interaction in contrast to his

definition of a multiplicative interaction.

The influence of additional variables could be modelled using the path analytic techniques which have been used extensively in the social sciences (e.g. Werts and Linn, 1970). Unfortunately the verbal descriptions of three of the four hypotheses entail non-additive relationships, that is, the effect of life events on depression is assumed to vary with the value or level of the additional variable. Stolzenberg (1980) indicated that while many verbal hypotheses in the social sciences are non-additive, there have been few attempts to develop non-additive causal models. He detailed a procedure for specifying non-additive models and for estimating the magnitude of the causal impact. Stolzenberg (1981, Personal Communication) confirmed that using his procedures the four hypotheses could be tested using the same general model. The general model may be expressed by the equation,

$$D = a + b_1 A + b_2 L + b_3 AL + e$$

Where D = Depression  
A = Additional Variable  
L = Life events  
e = Random Error

and the other symbols are parameters.

This general model can be estimated using hierarchical regression analysis. Hierarchical regression analysis is used because of the structural properties of the general model. In order that the interaction may be adequately represented, it is necessary to partial out the first order effects (A and L) by entering them prior to the product term (Cohen and Cohen, 1975). Cohen (1978) emphasised that

the interaction is not the product term, but the product term after the first order effects have been removed. This is essentially identical to Brown and Harris' (1978b) view that the "...'interaction'... is left when the independent affects of A and B are subtracted from the effects that A and B have on C when they occur together." (p 585, vide supra). The statistical significance of the parameters of the equation, i.e.  $b_1$ ,  $b_2$ ,  $b_3$ , is determined through the use of hierarchical F tests. These tests indicate whether or not the independent variables contribute to the dependent variables, different patterns of significant parameters being predicted under each of the four hypotheses.

Under the vulnerability hypothesis, the additional variable has no independent impact, and thus  $b_1$  should be equivalent to zero. Life events should have an impact in their own right and thus  $b_2$  should be significantly greater than zero. In the presence of life events, the additional variable should have an impact, and thus  $b_3$  should be greater than zero. Thus, the additional variable has no significant independent influence, although it interacts with life stress to have an impact.

Under the independent causes hypothesis, the additional variable and life events should have independent influences, their influence being independent of the level of the other variables. Thus, both  $b_1$  and  $b_2$  should be significantly greater than zero with  $b_3$  equivalent to zero.

Under the mutual potentiation hypothesis, neither the additional

variable nor the experience of life events should have any impact on their own and, therefore,  $b_1$  and  $b_2$  should be equal to zero. However, the concatenation of the additional variable and life events results in an increased intensity of depression and thus  $b_3$  is greater than zero.

Under the synergism hypothesis both life events and the additional variable influence the intensity of depression, the influence of each variable being dependent on the level of the other. The concatenation of the additional variable and life events produces a disproportionate increase in depression. Thus, the parameters  $b_1$ ,  $b_2$  and  $b_3$  will all be significantly greater than zero.

#### Estimating the Magnitude of Causal Impact

Having specified the pattern of hypothetical relationships, it is necessary to estimate the magnitude of the influence or causal impact of the independent variables. Stolzenberg (1980) indicated that causal impact can be operationalised as the extent to which change in one variable will produce a change in the second variable. Effect coefficients ( $b_1$  or beta) derived from regression equations are commonly used rates of change measure. Unfortunately, however, if non-additivity is present in the general model (i.e.  $b_3$  is significant) then neither  $b_1$  nor  $b_2$  can provide an adequate representation of the rate of change without simultaneous consideration of the parameter for the product term (i.e.  $b_3$ ). This simultaneous consideration cannot be achieved using effect coefficients. It may be achieved through the use of partial derivatives (See table 2).

TABLE 2 PATTERN OF PARAMETERS AND EFFECT COEFFICIENTS UNDER THE FOUR HYPOTHESES

GENERAL MODEL

$$D = a + b_1S + b_2L + b_3 S.L + e$$

where D = Depression, S = Social Support, L = Life events,

e = random error

VULNERABILITY HYPOTHESIS

$$b_1 = 0$$

$$b_2 > 0$$

$$b_3 > 0 \quad \text{which would imply}$$

Effects Coefficients

unstandardised

standardised

$$\partial D / \partial L = b_2 + b_3 S \quad (b_2 + b_3 S) \cdot \frac{\sigma_L}{\sigma_D}$$

$$\partial D / \partial S = b_3 L \quad b_3 L \cdot \frac{\sigma_S}{\sigma_D}$$

INDEPENDENT CAUSES HYPOTHESIS

$$b_1 > 0$$

$$b_2 > 0$$

$$b_3 = 0 \quad \text{which would imply that}$$

Effect Coefficients

unstandardised

standardised

$$\partial D / \partial L = b_2 \quad b_2 \cdot \frac{\sigma_L}{\sigma_D}$$

$$\partial D / \partial S = b_1 \quad b_1 \cdot \frac{\sigma_S}{\sigma_D}$$

MUTUAL POTENTIATION HYPOTHESIS

$$b_1 = 0$$

$$b_2 = 0$$

$$b_3 > 0 \quad \text{which would imply that}$$

TABLE 2 CONTINUED

Effect Co-Efficients

<u>unstandardised</u>	<u>standardised</u>
$\partial D / \partial L = b_3 S$	$b_3 S \cdot \frac{\sigma_L}{\sigma_D}$
$\partial D / \partial S = b_3 L$	$b_3 L \cdot \frac{\sigma_S}{\sigma_D}$

SYNERGISM HYPOTHESIS

$$b_1 > 0$$

$$b_2 > 0$$

$$b_3 > 0 \quad \text{which would imply that}$$

Effect Co-Efficients

<u>unstandardised</u>	<u>standardised</u>
$\partial D / \partial L = b_2 + b_3 S$	$b_2 + b_3 S \cdot \frac{\sigma_L}{\sigma_D}$
$\partial D / \partial S = b_1 + b_3 L$	$b_1 + b_3 L \cdot \frac{\sigma_S}{\sigma_D}$

Stolzenberg (1980) indicated that measures of rate of change are derivatives, when there is only one causal variable in the model and partial derivatives when there is more than one causal variable. Thus, for the simple life event-depression link, the derivative  $dD/dL$  is the rate at which the dependent variable D (depression) changes per change in the independent variable L (Life events). The partial derivative  $\partial D/\partial L$  is the rate at which the dependent variable D (Depression) changes per change in the independent variable L (Life events), net of the effect of the other variables in the model that influence depression, e.g. social support. It should be emphasised that unlike the effect coefficients ( $b_1$  or beta) the partial derivatives summarise the rate of change in depression due to the combination of the direct and non-additive effects of the additional variable. For example, in the case of the vulnerability model, that is where the additional variable only has an interactive effect, the comparison of partial derivatives can summarise the relative importance of the two variables.

Rate of change measures can be expressed in standardised or unstandardised form. Stolzenberg (1980) indicated that when the relative importance of variables within a population or sub-sample of a population is of interest, then the standardised coefficients are of most value since they are adjusted for different scales of measurement of the variables. Standardised and unstandardised rates of change measures derived under each of the four hypotheses are tabulated in table 2. In the absence of a significant interaction, the effect parameters are identical to unstandardised and standardised

regression coefficients. With an interaction effect present, neither  $b_1$  nor  $b_2$  can be meaningfully interpreted without simultaneously considering the coefficient for the product term. This simultaneous consideration is achieved through the use of partial derivatives.

This modelling technique will be used in the analysis of the results of the study.

### SUMMARY

This chapter has examined the issues pertaining to the influence of additional variables on the simple event-syndrome link.

A selective review of additional variables from the sociological, psychological and physiological domains of discourse was presented. The importance that additional variables have in general with regard, firstly, to explaining variability of response to events, and secondly, enhancing our understanding of the causal processes involved and thirdly, the suggested treatment strategies, was discussed.

The next section of this chapter was concerned with the mode of action of additional variables in regard to the simple event-syndrome link. Four current hypotheses, namely the vulnerability hypothesis, the independent causes hypothesis, mutual potentiation hypothesis and synergism hypothesis were described. The empirical support for each of these hypotheses was reviewed. Particular emphasis was given to the work of George Brown and his colleagues. It was concluded that the evidence in support of the vulnerability hypothesis was not overwhelming.



The review of the empirical evidence was based on primitive definitions of the four hypotheses. More rigorous verbal and statistical descriptions were considered with the relative merits and demerits of log-linear approaches being outlined.

The fifth section of this chapter considered the theoretical difficulties of achieving a causal, as distinct from a descriptive, analysis of information expressed in the form of a 2 by 2 contingency table. It was concluded that such analyses could be misleading.

The chapter was concluded by a discussion of the advantage of mathematical models within the context of life event research and a suitable non-additive mathematical model was proposed. The application and interpretation of this model was described with the procedures for estimating the magnitude of causal impact being outlined.

## CHAPTER 3 AIMS, DESIGN AND METHOD

### AIMS

This study aims to investigate the relationships among psychosocial variables and minor affective disorders in the general population. In particular, the relationship among life event, demographic and personality characteristics, social support variables and measures of depressive disorder will be considered.

The plausibility of the hypothesis, that life events produce a change in the level of depressive symptoms, will be examined in the light of the principles detailed above. The influence of additional variables on the association between life events and depressive symptoms will be explored.

The study is to some extent exploratory in nature, however, its aims may be more rigorously stated in terms of various hypotheses. Each hypothesis serves merely as a starting point with additional analyses being performed to clarify the findings.

### Principal Hypotheses

#### (1) Existing Symptoms

- (1) Several clusters of symptoms, or syndromes will be present in the sample.

#### Concerning Life Events and Symptoms

- (2) The level of independent life stress experienced over the

previous twelve months will be associated with minor affective disorder in general.

- (3) Independent life stress will be most strongly associated with depression of a 'reactive' or 'neurotic' form.
- (4) Particular types of independent life events (i.e. 'exits' and 'undesirable' events) will be more strongly associated with depression of a 'reactive' or 'neurotic' form than independent life events in general.

#### Concerning Life Events, Symptoms and Additional Variables

- (5) Demographic and Personality variables will modify the association between general life event stress and depression of a 'reactive' or 'neurotic' form.
- (6) 'Vulnerability' factors, including lack of employment outwith the home, early loss of mother, lack of a confiding relationship and the presence of three or more young children at home, will influence the association between life events and depression in accordance with the vulnerability hypothesis.
- (7) The quality and extent of social relationships will modify the association between general life stress and depression of a 'reactive' or 'neurotic' type.

### THE TRAINING OF THE INTERVIEWERS

Funds were obtained from the Manpower Services Commission to support the overall costs of the survey, including the salaries of six interviewers and one secretary. The six interviewers were selected from a pool of 19 honours psychology graduates. Five of the interviewers were female and one male. They were all in their early to mid-twenties.

The interviewers spent the first three weeks of their employment in training for the survey. They were provided with two instruction manuals developed by the author (see appendix 1 and 2). The first manual, based on the work of Atkinson (1971), detailed the general technique which the interviewers should apply in carrying out social surveys. The manual initially outlined the approach to be adopted during the first contact with the subject. The importance of the correct identification of the organisation involved and the purposes of the survey, together with techniques for overcoming non-co-operation, were considered. This was followed by a section on the manner in which the interview should be conducted once the subjects's co-operation had been obtained. The correct approach to the mechanics of the interview such as the seating of the interviewer and subject, the proper use of documents together with the proper pace, tone and style of questioning were described. A final section detailed the tactics the interviewer should adopt when affected by difficult circumstances such as having to carry out the interview with another person present or having to deal with exaggeration, lying or facetiousness on the part of the subject.

The second manual detailed specific questionnaire and interview procedures to be used in this particular survey. The approach to be used with each instrument, whether self-administered or interviewer rated, was outlined in detail. The scoring criteria and definition of scale anchor points were provided in this manual. The selection of the scales to be used in the analysis, and their characteristics, are described below.

The interviewer spent the initial few days of training learning the details of the manuals. This was followed by role-playing exercises with the interviewers playing subjects in order that their colleagues could practise interview techniques. At this stage in training an attempt was made to ensure accurate and uniform application of the interview techniques in general and of the questionnaire anchor points in particular.

Following the role-playing exercises the interviewers practised their interview techniques in randomised pairs with the subjects being patients from the Whittingehame Gardens Day Hospital. Patients who attend this Day Hospital tend to suffer from chronic neurotic disorders. Each interviewer interviewed at least four patients and observed a colleague interviewing four patients. Following each interview, they provided their partner with feedback regarding general interview procedures and proper application of anchor points.

Following the above training in the hospital, a random sample of one of the electoral wards was drawn and the interviewers began their

field work in the pilot stage of the project. The pilot stage had two general aims. Firstly, it was used to give the interviewers experience in dealing with the general public. Secondly, it was used to test for both inadequacies in the general approach adopted and in the questionnaire and interview procedures used.

One hundred and seven subjects were interviewed in the pilot community sample. This sample was collected primarily for piloting purposes and therefore was not included in the analysis below.

The pilot phase led to two major changes being made in the approach used in the main survey. Firstly, at the outset of the pilot phase, certain interviewers were obtaining higher refusal rates than their colleagues. Role playing with other members of the interview team was used to improve their initial approach to their subject and their refusal rate subsequently diminished. Secondly, the female members of the team expressed concern at being asked to interview alone, particularly during the evening. Accepting that this was a genuine and realistic concern, the design was changed so that for the main survey there were two pairs of interviewers and one single (male) interviewer. The sixth interviewer attempted to collect the hospital sample (vide infra) and acted as a collator and scorer of incoming interview schedules.

This second change in the procedure had the disadvantage of attenuating the potential sample size, however, it had the advantage of allowing inter-rater reliability figures to be obtained in a

realistic setting in concurrence with the principles subsequently advocated by Grove et al (1981).

The specific interview procedures and self-report scales used will now be considered. The reasons behind their selection will be outlined.

#### THE ASSESSMENT OF DEMOGRAPHIC CHARACTERISTICS

The interviewers began with questions about the respondents demographic characteristics. Demographic characteristics were assessed by the established and clearly defined techniques of the Government Social Survey (Atkinson, 1971). The detailed criteria that were applied are described below (see appendix 2). Information relating to the respondent's sex, age, socio-economic status, employment status, marital status, size of household and religious affiliation was collected. Socio-economic status was defined using the Registrar General's Classification of Occupations (OPCS, 1970). Following Atkinson, full-time employment was defined as being in gainful employment for at least 30 hours a week.

#### THE DETECTION OF SYMPTOMS

An extensive range of instruments has been used to detect symptoms of depression (Fahy, 1969; Carroll et al, 1973). The choice of a particular instrument must represent a compromise: known strengths and weaknesses of the scales and the particular needs of the study's design and population were considered.

Fahy (1969), having factor analysed subject scores on six common scales of depression, contended that their content overlapped to only

a limited degree. He argued that studies might be improved if several scales were used. The use of several scales might not only increase the probability of tapping a greater number of the heterogeneous phenomena found within the domain of depressive symptoms (Derogatis et al, 1972; Blumenthal, 1971), but also reduce the risk of mono-method bias (Cook and Campbell, 1979). Snaith (1981) more recently has argued that until the particular properties of scales within different populations can be determined, studies should be based on two or more types of measure. Four instruments were selected with a view to diversity:-

1. Self-Rated Depression Scale (SDS) Zung (1965)
2. Self-Rating Scale of Distress: Anxiety Sub-Scale (SRDS) Kellner and Sheffield (1973)
3. Depressive Adjective Check-List (DACL) Lubin (1965)
4. Rating Scale for Primary Depressive Illness (five selected items) Hamilton (1967)

#### Self-Rating Depression Scale (SDS) Zung (1965)

The SDS is a twenty item self-report questionnaire designed to determine the presence and severity of depressive symptoms. Subjects are asked to respond concerning their experiences over the previous week, rating whether each item applied for less than a day, 1 or 2 days, 3 or 4 days or 5 to 7 days. The items of the scale are balanced to counteract positive response bias, half worded so that a positive response indicates the presence of a symptom while the other half are worded so that negative responses indicate the presence of a symptom.



The SDS has been used extensively in epidemiological studies in settings as diverse as Japan, Australia, the USA and six European countries (Zung, 1967, 1969, 1971, 1972; Zung and Durham, 1973; Blumenthal and Dielman, 1975; Henderson et al, 1979).

Weissman and Klerman (1978) in their review of psychiatric epidemiology noted that it was a suitable instrument and indeed, Blumenthal (1975) indicated that it '...appears to have the validity, reliability and replicability so important in epidemiological research.' (p 971). The widespread use of this instrument with psychiatric, non-psychiatric and general population subjects has generally established its concurrent validity (see Barrett et al, 1978, for a review; Briggs et al, 1978).

While the application of the SDS has received considerable support, this support has not been universal. Carroll et al (1973) questioned the value of the SDS because of its low pre-treatment correlation with the Rating Scale for Primary Depressive Illness (RSPDI) (Hamilton, 1960). That is, with the scale that they adopted as their criterion instrument. Briggs et al (1978) tempered this criticism and pointed out that by examining pre-treatment scores, Carroll et al (1973) narrowed the range of symptom severity considered and this range restriction resulted in attenuation of the correlation coefficients (Guilford and Fruchter, 1973; Hamilton, 1976). Briggs et al (1978) provided empirical as well as theoretical support for this view, reporting correlations between the two scales of up to 0.76.

Briggs et al (1978), therefore, argued that Carroll et al's (1973)

proscription of the SDS was ill-founded, and they supported its use.

Blumenthal (1975), while strongly supporting the use of the SDS in epidemiological contexts, noted that the instrument might be improved by the addition of items. In view of this criticism, the present author added items in an attempt to measure the experiences of self-pity, early morning wakening and guilt. The only other alteration was that the word 'blue' in item one was changed to 'sad' in order to conform with common British usage.

Self-Rating Scale of Distress: Anxiety Sub-Scale (SRSD) Kellner and Sheffield (1967)

The SDS while providing a useful instrument for measuring depression in the general population, tends to emphasize the endogenous rather than the reactive features of depression (Garside, 1977; Personal Communication). The Kellner-Sheffield Self-Rating Scale of Distress: Anxiety Sub-Scale, consisting of 8 items, was used to complement the SDS, because it has a similar format and may be used to detect anxiety symptoms. Within psychiatric samples anxiety features are frequently associated with 'reactive' depression (Kendell, 1976). The SRSD was administered immediately after the SDS, with each item being scored on a four point scale and referring to the previous week.

The scale is reliable and has adequate discriminant validity (Kellner and Sheffield, 1973). It has been used successfully with non-psychiatric subjects (Kellner and Sheffield, 1967; Greene, 1976).

#### Depressive Adjective Check List (DACL) Lubin (1965)

Lubin (1965) developed the Depressive Adjective Check Lists (DACL) as measures of transient subjective depressed mood. Each list consists of 34 adjectives, 22 of which are depressive or pessimistic and 12 non-depressive or optimistic. The subject is required to record which adjectives apply to him at that particular moment. There are seven lists that fall into two sub-sets of parallel forms. Form E was selected because Lubin (1965) indicated that it had acceptable reliability (0.85) with normal population subjects and it has been used successfully in previous epidemiological research (Choi and Comstock, 1975).

#### Rating Scale for Primary Depressive Illness (RSPDI) Hamilton (1960, 1967)

Hamilton (1960, 1967) developed the RSPDI as a standardised procedure to measure the severity of signs and symptoms of depression experienced by a person with a diagnosis of clinical depression. Twenty-one symptoms are rated, usually on five points of severity. The instrument has been used extensively with clinical samples and Carroll et al (1973) contended that because of its frequent use it has been adopted as the standard instrument in clinical samples. Given the necessity of a prior diagnosis of depressive illness, this instrument is not suitable for community studies. It was felt, however, that in order to diversify the procedure used in detecting symptoms and to provide the interviewers with a means of detecting symptoms not readily detected by questionnaires, certain items should be utilised. Five items that could be rated from observations made throughout the interview were selected. These were Anxiety and Tension, Depression, Retardation, Agitation and Paranoid symptoms.

They were rated on a five point severity scale using the criteria provided by Hamilton (1967).

#### THE ASSESSMENT OF LIFE EVENTS

A detailed account of the problems entailed in measuring life events has been provided above, and therefore, they will only be alluded to in this section when it is necessary to justify the procedures adopted.

Examination of the life event literature indicates that there are essentially two traditions in the measurement of life events. The first, which may be characterized as the 'checklist' approach, includes the SRE (Holmes and Rahe, 1967) and its derivatives, while the second is the intensive interview approach developed by Brown and his colleagues (Brown et al, 1973a, 1973b).

When the 'check-list' approach is adopted the subject is asked to indicate which event, from a list of thirty to sixty commonly occurring events, they have experienced in some set period prior to the interview. The quality of the information obtained from this approach is heavily dependent on factors such as memory distortion and the subject's motivation. Brown (1974) has been the most vociferous critic of the 'checklist' approach. In essence, his criticisms relate to the interpretative freedom that this approach allows the subject. Thus, problems such as effort after meaning and the depressed person's tendency to have a lower threshold for reporting events, limit the quality of information obtained in this manner.

In general, these approaches suffer from a poverty of description of the events but, in particular, they cannot be used to provide the critical rating of 'independence'. The approach is not without advantages, however, vis. simplicity, economy and it does not require extensive interview training.

The opposing tradition, developed by Brown and his colleagues entails in-depth interviewing concerning the occurrence of events, their characteristics and the circumstances surrounding the events. In the development of this approach careful pre-experimental specification of not only what could and could not be regarded as an event, but also, the persons covered by each of the events was carried out.

The interviewer was required to rate the context of each event on some thirty rating scales. Contextual ratings of threat, used by Brown and his colleagues in their major publications, were obtained by presenting all the contextual information pertaining to each event to a panel of raters who were ignorant of the subject's response to the event. In this way Brown and his colleagues attempted to minimise direct contamination, that is, the likelihood that a rater might rate a depressed person's event more severely.

Brown's procedure is thus extremely detailed and perforce very lengthy, taking up to several hours to complete. This lengthiness is clearly a disadvantage and there is the additional disadvantage that interviewers require extensive training in the procedure.

While the author generally agrees with the philosophy of Brown's approach rather than the philosophy of the 'checklist' approach, it

was felt to be too lengthy and outwith the resources of this study. In addition, the resources for training interviewers in this method were not available.

#### The Method Adopted

A method developed by Paykel, which, to the author's knowledge is unpublished, perhaps represents a rapprochement between the two traditions. It has the advantage of comparative brevity over the Brown approach, while providing more detail and being less severely influenced by contaminative effects than the 'checklist' approaches. As a rapprochement between the two traditions it is hoped that it has most of their virtues and few of their vices.

Paykel's (1977, Personal Communication) semi-structured interview is designed to elicit detailed information regarding sixty-three life events that had previously formed a conventional checklist. In the main, his approach was used although it was expanded to include some of Brown's principles on dating both events and the onset of symptoms (vide infra). What constituted each event and to whom it was applied was clearly defined. The sixty-three events related to ten categories of roles or activities, namely, work, education, financial, health, bereavement, relocation, dating, legal, family and social, and marital. Other categorisation of events was possible, including 'desirable' and 'undesirable' events and 'exit' and 'entrance' events (vide infra).

The interviewer introduced the life event section by indicating that

it was concerned with the things that had happened to the respondent, their family and their friends in the previous twelve months. The interviewer enquired about each of the 63 events unless it was obviously inapplicable, for example, events from the 'education' category would be excluded when interviewing people who were not involved in any formal educational programme. Once it had been determined that a particular event had occurred, the interviewers applied semi-structured interviewing strategies to obtain information about the circumstances surrounding the event and its impact on the respondent. Nine ratings of each event were made, their rationale and fundamental characteristics will be described below with more detail being available in appendix 2.

#### 1. Life Event Number

The Life Event Number was used to identify which of the sixty-three events listed had occurred. This identification number was used for the various categorisations of the events. Paykel et al (1976) published British norms of consensually derived weights of the degree of upset entailed by most of these 63 events. By reference to these norms it was possible to obtain a totally independent, albeit approximate, estimate of the degree of life event stress experienced by each respondent. This procedure was used to minimise direct contamination. This estimate of life event stress was thus obtained independently of information relating to the respondent's psychological state.

#### 2. Time Between The Interview and Occurrence of the Event

The interviewers attempted to estimate the time between the occurrence

of each event and the interview. This period was estimated in months. The interviewers were taught to identify 'anchor dates' in the previous twelve months such as birthdays, wedding anniversaries or annual holidays and then to attempt to relate events to these dates. On enquiring about the date of particular events, balanced alternatives were presented e.g. if the respondent said 'early in the year', the interviewer did not ask 'January?' but rather 'Before or after Easter?'. The interviewers were instructed that they should not necessarily accept the first response offered but rather they should judge from the respondent's general tone and demeanour whether greater accuracy or detail might be obtained through further questioning. This instruction was applied to all the life-event ratings.

### 3. Time of Event Occurrence in Relation to Symptom Onset

The same principles of dating were applied to establishing the temporal relationship between events and onset. Brown and Harris' (1978a) concept of 'change points' in symptomatology was applied. These change points are defined as clear changes in depressive symptomatology either from 'normality' or from an already established pattern of symptoms. Points of increase, levelling or decrease in symptomatology were rated. The interviewers were instructed to inspect the respondent's responses to the self-administered depression and anxiety questionnaires in order to determine whether they had endorsed any of these symptoms. The interviewers then used the dating procedures outlined above in an attempt to determine the change points of these symptoms. If the respondent had failed to endorse any of the symptoms they were asked about their mood in the previous twelve months. Gentle prompting and probing was applied until the



interviewer was convinced that they had identified all change points. Onset was defined as occurring at a change point where there was a marked quantitative change in symptomatology. It should be noted in passing, that it is argued above that the concept of onset has theoretical and practical shortcomings. This was confirmed in practice.

#### 4. Independence of the Event

A primary pre-condition for arguing causally about events and symptoms is the demonstration of assymetry. That is, it is necessary to determine whether the event might have been the consequence of the respondent's symptoms rather than vice-versa. Brown et al (1973a) argued that it is necessary to rate the 'independence' of the event from the subject's symptoms. Taking cognisance of all the circumstances surrounding the events, the interviewer rated each event on a five point scale ranging from 'The event was almost certainly independent of the illness' to 'The event was almost certainly dependent on the illness'.

#### 5. Control Over the Event

The interviewers rated the extent to which the respondent had choice or control over the initiation and progress of an event on a five point scale. This scale ranged from 'complete control' to 'no control'.

These ratings of events were followed by four ratings of the impact of events; ratings that were designed to entail consideration of the

circumstances peculiar to the respondent. It was hoped that these would provide more sensitive measures than the consensually derived ratings of impact entailed by each event described above.

#### 6. & 7. Subjective Negative and Positive Impact of the Event

The ratings of subjective impact of each event referred to the impact that each event had on the respondent as determined from their report of their feelings about the event and other circumstances indicating the respondent's reaction to the event. It is likely that this rating would be contaminated by the respondent's current psychological status, with, as Brown (1974) suggested, the more depressed respondents reporting the impact of the event in more negative terms.

Both negative and positive impact were assessed and it was assumed a priori, that any particular event could have a mixture of positive and negative qualities. For example, if someone experienced the loss of a parent who had been suffering from senile dementia, then it is possible that they will experience negative impact because of the loss of a loved one and positive impact because of the relief from the burden that such a situation might entail.

Subjective negative impact was assessed on a five point scale from 'severe negative impact' to 'no negative impact', the rating being based on the respondent's report of upset, tension, crying, anger or disappointment as well as other negative feelings. Similarly, subjective positive impact was assessed on a five point scale from 'intense positive impact' to 'no positive impact' and was based on the respondent's report of happiness, pleasure, relaxation or lessening of

stress or other pleasurable or positive feelings arising from the event.

#### 8. & 9. Objective Negative and Positive Impact of the Event

These measures were designed to provide idiographic ratings of the impact of the events, which unlike the above subjective ratings were independent of the respondent's own responses and reports. Their function was to provide a measure that took the particular circumstances of the event into account, but which minimised direct contamination through the respondent's plaintive set. It was hoped that contrasting the subjective and objective ratings might provide an estimate of the influence of this plaintive set. The interviewers were asked to assess the degree of impact that the event would have on someone when the full nature and particular circumstances of the event were taken into account. The respondent's subjective report of the distress experienced following the event was disregarded. Circumstances that might modify the impact of the event, such as previous experience of the event, its desirability and expectedness were taken into account in rating the objective impact of the event. The measures of objective negative and positive impact were measured on five point scales that were identical to those used to measure subjective impact.

#### THE ASSESSMENT OF EARLY LOSS

In the literature on depression, together with the view that recent

life-events might cause depression is the view that events during childhood, particularly loss or bereavement events, might predispose individuals to depression in adulthood. Lloyd (1980) recently reviewed the literature concerned with the latter view, and concluded, that on balance, there is evidence that early parental loss through bereavement may lead to increased risk of depression in adulthood.

Within the current study, the simple relationship between early loss and adult symptomatology was not of primary interest. The effect that early loss might have on the relationship between recent life events and symptoms, however, was thought to be of primary importance. Brown et al (1975) argued that the experience of early loss could modify the response to life events. They argued that women in the community who had lost their mothers before age eleven were vulnerable to the effects of recent life events. That is, those who had experienced such a loss had a disproportionately higher risk of depression following a severely threatening event. Brown et al (1979) argued that early loss could have an additional influence, in that it could act as a 'symptom formation' factor. They argued that amongst a sample of female patients past loss by death was associated with psychotic type depression while loss due to means other than death was related to neurotic type depression.

Given that the experience of early loss was thought to modify the relationship between recent life events and symptoms, an attempt was made to record these experiences.

Following the section on recent life events questions were asked about losses and separations that had occurred more than twelve months prior to the interview. The respondents were asked whether they had been separated from either parent before the age of 17 for a period of more than four months. This period of four months was chosen on a somewhat arbitrary basis. It was felt, however, that it was a sufficiently long period to be recalled after a period of many years. In addition, it would exclude socially sanctioned separations such as prolonged summer holidays or attendance at a boarding school. If respondents confirmed that they had experienced such a separation then the reason was identified. They were asked whether the separation was due to parental death, a parent leaving home because of divorce or separation, prolonged hospitalisation either on the part of the parent or the part of the respondent, or any other reasons. The age at which the separation had occurred was assessed by applying the dating procedures detailed above. The period of separation was also assessed.

The respondents were then asked about the death of siblings, children or spouse. Their age at sibling loss was recorded and the length of time from the death of a child and spouse to the interview was also recorded.

#### THE ASSESSMENT OF PERSONALITY CHARACTERISTICS

Personality characteristics were assessed using a modified version of the Eysenck Personality Questionnaire (EPQ) (Eysenck and Eysenck, 1975). This instrument has been used extensively in British samples and its construct validity is moderately high. The original version appeared to be too long for the purpose of the study and thus it was shortened by removing the items relating to the Psychoticism (P) scale. This particular scale was removed for two reasons. Firstly, there are considerable difficulties in deciding what it measures; Eysenck and Eysenck (1975) stated that '...the nature of the P variable can of course only be guessed at' (p 11). Secondly, the a priori assumption was made that it would be of limited relevance to the current study.

The nature of the constructs measured by the other three scales of the EPQ, namely Neuroticism, Introversion- Extraversion and Lie Scales, has been well established. The scales have been shown to have high test-retest reliability with normal adult subjects (Eysenck and Eysenck, 1975).

#### THE ASSESSMENT OF SOCIAL RELATIONSHIPS

Duncan-Jones (1981) noted '...that the quality of social relationships may affect health is a truism.' (p 55). The manner in which social

relationships may influence psychological wellbeing have been examined in detail by Henderson (1980) and Henderson et al (1978). Within the context of this present study two theoretical positions were of interest. It was argued above that the quality of social support which a person enjoys may both directly influence his level of symptomatology (e.g. Miller and Ingham, 1979; Henderson et al, 1980) and/or indirectly influence it by potentiating the impact of stressful life events (e.g. Brown et al, 1975; Surtess, 1980). An attempt was therefore made to evaluate social relationships.

Numerous scales that have been designed to measure the quality of an individual's social relations and social adjustment are now available. Weissman (1975) provided a detailed analysis and critique of 15 scales that were currently available. She considered their content and form, psychometric properties and appropriateness for different purposes. Weissman et al (1981) recently reviewed a further 12 scales that have been published in the interim period, using the same approach.

The selection of the scale for the current study was made on the basis of Weissman's (1975) review and by examining those of the 15 scales that were available. In making the choice, three of the fifteen scales were excluded because they were of historical interest only. Another three were excluded because they required that information should be provided by a significant other. It was felt that it would be difficult to obtain such information within the context of this study. Two scales, that were in a self-report format rather than an interview format, were excluded because it was felt that interview procedures had significant advantages in this area. Weissman (1975)

argued that semi-structured interviews had significant advantages in that they could provide more information because the interviewer could calm and cajole the informant and indeed, could detect attempts to under-report or falsify information. Two of the remaining scales referred to aspects of social relationships at the present moment or over the past week. These were felt to be inappropriate, It was thought that a longer time period should be covered to allow measurement of longer standing patterns of adjustment rather than run the danger of measuring patterns that had been influenced by the short term disruptive effects of specific events.

Given the danger that social support constructs might be contaminated by items from the symptom domain (Weissman, 1975) it was thought important to be able to differentiate between the instrumental and affective characteristics of social relationships. Two further scales were therefore excluded because they did not differentiate between these two components.

Of the remaining two available scales, the Social Adjustment Scale (SAS) developed by (Paykel et al, 1971; Weissman and Paykel, 1974; Weissman et al, 1974) was selected. The SAS refers to the individual's experiences over the previous two months. It was felt that this would provide the best available balance between the problem of distortion through retrospective evaluation and distortion through the evaluation of essentially transient patterns of social adjustment. It has been shown to have adequate inter-rater reliability and factorial validity (Paykel et al, 1971b) and has been used with both



depressed patients and with non-psychiatric subjects (Weissman and Paykel, 1974; Weissman et al, 1974).

Information was obtained by semi-structured interviewing. For each item the initial questions were specified and additional prompts were available to provide sufficient detail for an accurate rating. Each of the specific items was divided into five defined anchor points. The first point on the scale was designed to denote excellent status or the ideal norm. The second point was designed to denote mild impairment and was designed to represent the estimated statistical norm of the general population. The remaining three anchor points were used to denote increasing degrees of definitely impaired functioning. Global ratings of impairment, ratings designed to provide '...a relatively unstructured and intuitive synthesis of all of the available material.' (Paykel et al, 1971b; p 160) were rated on seven point scales.

The SAS was shortened from the original 48 items to 21 items for two reasons. Firstly, and of least importance, the SAS was reduced in length because of the time involved in its administration. Paykel et al (1971b) suggested that the scale might take between 45 minutes and one and a half hours to administer. This was regarded as too long for the purposes of the current study. Secondly, and of some considerable theoretical importance, the SAS was reduced in length because of the symptom-like characteristics of many of the items.

The 48 item SAS was designed to tap three areas of social adjustment,

i.e. behaviour, friction, and feelings and satisfaction (Paykel et al, 1971b).

Unfortunately, however, many of the items appear to be related to primary symptoms of depression, e.g. guilt, worry, resentment, disinterest in sex, friction and distress. In using the full SAS there was a danger that the social adjustment construct would be contaminated and submerged by measures of depressive symptomatology and that the investigator might merely discover a correlation between two different measures of depressive symptomatology. A problem exists as there are no a priori rules for distinguishing between core symptoms of depression and mere concomitants (Derogatis et al, 1972). The individual investigator must to some extent determine the boundaries of each domain. Paykel and Weissman (1973) argued that they could distinguish between symptoms and symptom-like aspects of social adjustment, 'Variables incorporated in this factor (i.e. an anxious rumination factor) did not directly assess depressive thinking or anxiety, but rather aspects of dissatisfaction and overconcern with function and relationships in the social field.' (p 663). In a longitudinal study scores on the anxious rumination factor were found to be closely related to symptomatic illness (Paykel and Weissman, 1973) and this might support the view that some contamination was present. A conservative and restrictive approach was, therefore, adopted to the definition of symptoms and the 21 items relating to 'non-symptom' behaviour were retained. Two sub-categories of behaviour were considered i.e. 'Performance' and 'Interpersonal behaviour.' (Paykel et al, 1971b).

'Performance' behaviour items examined comparatively concrete and easily quantifiable behaviour, emphasising instrumental role performance in particular. These items included frequency of contact with friends, time spent at work and degree of active involvement with children. Items pertaining to 'Interpersonal behaviour' related to finer and 'softer' measures of interpersonal relationships and included ability to confide with spouse, members of the extended family and friends as well as measures of dependency, submissiveness and domineering behaviour.

Five global ratings of social adjustment were completed immediately after the interview. In completing these items the interviewer was instructed to take into account all of the available information including suspected denial. Maladaptive behaviour not covered was taken into account in these ratings. The five ratings covered particular social roles i.e. Social-Leisure, Extended Family, Marital, Parental and Overall Adjustment.

#### SAMPLING PROCEDURES

The urban catchment area of Gartnavel Royal Hospital was selected as the geographical area to be sampled. This was chosen firstly, because it was the base hospital of the author and, secondly, because an attempt was made to interview all patients referred to the psychiatric services of the hospital during this period to obtain a sample for comparison. In terms of social class this was a mixed community, including large areas of both local authority and owner-occupied housing. The characteristics of the obtained sample are described in

detail below.

#### Method of Sampling

The sampling frame used was the electoral rolls for the electoral wards covered by the catchment area. The subjects were selected by systematic sampling (i.e. pseudo-random selection) of the electoral rolls. Kish (1965) described systematic sampling in detail and indicated that it involves a selection of every  $n$ th subject after a random starting point on a list. Kish (1965) noted that systematic sampling is an easier clerical task than the random selection of subjects from within a large sampling frame. It is also more convenient for the supervisor to check the proper application of the sampling procedure. A further advantage of this procedure, that Kish (1965) noted, is that systematic sampling ensures that the sample yielded will be a proportionate one, that is, it will be evenly spread throughout the electoral wards. This may be particularly important when the demographic characteristics of the sample correlate with the dependent variable.

Three replicated samples were selected to allow for assessment of interviewer effects. The following procedure was used. The electoral rolls were randomly ordered and a start number randomly selected from tables. The sampling interval was calculated using the formula  $k = \frac{N}{n}$  (Kish, 1965) where  $N$  = population size,  $n$  = sample size and  $k$  = the sampling interval. The sampling interval was 761 and therefore every 761st subject on the electoral roll was selected. This procedure was repeated three times and these sub-samples were randomly allocated to the interviewers.

Electoral rolls suffer from the disadvantage that they do not include electors who have either not registered as electors or who have changed their address since registration. The Blyth-Marchant technique (Blyth and Marchant, 1972) was used to counter the effects of incompleteness. This technique requires the interviewer to identify 'non-electors' living at the same address as the target subjects. The interviewers were provided with a list of all the electors listed as living in the same household as the target subjects and they then listed additional residents in alphabetical order. Interviews were then carried out with individuals whose names occurred in previously determined positions on the list. The use of this technique should provide a systematic sample of non-registered electors.

#### Replacement of Subjects

If an interviewer, on calling at the subject's address, discovered that the house was vacant or demolished or that the respondent had permanently moved away from the address, then the subject was replaced. Subjects who were absent from home or temporarily moved away from home or who refused to be interviewed were not replaced. The interviewers were required to attempt to interview the subject on at least six occasions, at different times of the day and the week, before this subject was designated as a non-contact. The new subjects who were selected to replace the original subjects were selected by selecting a random number between 0 and 761 (the sampling interval) and counting from the original name forward.

#### SCORING OF INTERVIEW SCHEDULES

The scoring of the interview schedules was carried out by the interviewers at the time of the interview. Each schedule, however, was checked for completeness by one of the two scorers when it was returned to the research office. The research team met once a week to discuss problems of definition and scoring. The scorers transcribed the scored results onto the computer columns of the schedules. The information contained in the schedules was transcribed onto computer cards and verified by the staff of Glasgow University's Department of Computing.

Approximately 10 per cent of the sample were contacted by telephone following the interviews to determine interviewer compliance. No evidence of non-compliance was found.

#### CHAPTER 4 CHARACTERISTICS OF THE OBTAINED SAMPLE

In this first section, two aspects of the obtained sample will be considered. Firstly, the response rate will be examined, and secondly, the social and demographic characteristics of the respondents will be analysed in detail.

##### RESPONSE RATE

Between September 1977 and February 1978, four hundred and eight subjects were interviewed from an original sample of five hundred and thirty individuals. This represented a response rate of 77 per cent. Eighty individuals refused to take part in the interview giving a refusal rate of 15 per cent. Thirty-two individuals could not be contacted after at least six attempts and ten individuals produced protocols that were judged to be incomplete and inadequate.

MacMahon and Pugh (1970) indicated that this level of response is acceptable for the general purposes of epidemiological research. Eysenck (1970) indicated that correlational analysis is robust to even quite serious deficiencies in sampling, and given that the study is correlational in nature, it could be argued that the level of response achieved was adequate for this purpose.

## SOCIAL AND DEMOGRAPHIC CHARACTERISTICS OF THE RESPONDENTS

### Sex of the Respondents

A significantly higher proportion of the sample was female (56 per cent) (Chi squared = 6.67, df = 1,  $p < 0.01$ ). This difference probably reflected two factors. Firstly, a genuine difference in the male:female ratio in this comparatively old sample (vide infra) and a non-significant trend (see table 3) towards a differential contact rate with men being more difficult to contact than women. There was no evidence for a differential refusal rate across the sexes (see Table 3).

### Age of the Respondents

Respondents who were younger than 17 years would not have been registered on the Electoral Rolls. Above this level, the complete age range was represented in the sample. Details of the age by sex distribution of the respondents are provided in Table 4.

The mean age for the total sample was 49.62 years (S.D. = 17.30) with the males having a mean age of 48.35 years (S.D. = 17.55) and the females a mean age of 50.60 years (S.D. = 17.10). The means were not significantly different ( $t = 1.30$ ). The variance ratio was not significant ( $F = 1.12$ , df = 177,229, N.S.), implying that the variances of the two distributions did not differ significantly. The age of the refusers and non-contacts was estimated where possible (vide supra). The mean of the estimated ages of non-contacts was significantly higher than the mean age of the respondents. There was a non-significant trend towards the refusers being older (see Table 3)



TABLE 3 SEX AND ESTIMATED AGE AND SOCIAL CLASS OF REFUSERS AND NON CONTACTS COMPARED TO RESPONDERS

	1. RESPONDERS			2. REFUSERS			3. NON-CONTACTS		
	MALE	FEMALE		MALE	FEMALE		MALE	FEMALE	
SEX	178	230		36	44		18	14	
	$\chi^2_{1V2} = 0.05$ (N.S.)								
	$\chi^2_{1V3} = 1.91$ (N.S.)								
	MEAN	S.D.	N	MEAN	S.D.	N*	MEAN	S.D.	N**
AGE	49.62	17.30	408	52.71	14.64	70	63.10	13.00	11
	$t_{1V2} = 1.58$ (N.S.)								
	$t_{1V3} = 3.21$ (P < 0.002)								
	MEAN	S.D.	N	MEAN	S.D.	N*	MEAN	S.D.	N**
SOCIAL CLASS	3.60	1.35	408	3.22	1.10	70	4.18	0.87	11
	$t_{1V2} = 1.90$ (N.S.)								
	$t_{1V3} = 1.93$ (N.S.)								

\* Some refusers refused by telephone, therefore, no adequate estimate of age or social class could be made.

\*\* The sex of the non-contacts was judged from the electoral role. Age and social class could not be assessed in this manner.

TABLE 4 DISTRIBUTION OF RESPONDENTS BY AGE AND BY SEX

AGE BAND IN YEARS	NUMBER OF MALES	NUMBER OF FEMALES
17 - 19	4	11
20 - 24	20	18
25 - 29	15	15
30 - 34	20	10
35 - 39	17	17
40 - 44	10	20
45 - 49	14	22
50 - 54	9	19
55 - 59	14	24
60 - 64	10	21
65 - 69	27	21
70 +	18	32
TOTAL NUMBER	178	230
MEAN AGE	48.35	50.60
STANDARD DEVIATION	17.55	17.10

### Social Class of the Respondents

The social class of each of the respondents was determined using the criteria laid down in the Registrar General's Classification of Occupations (OPCS 1970). The social class of the female respondents was based on that of the head of the household (see appendix 2). The social class distribution by respondents' sex is presented in Table 5.

The modal class was 3b 'skilled occupation - manual'. The overall mean social class was 3.60 (S.D. = 1.35) with the males having a mean social class of 3.71 (S.D. = 1.36) and the females a mean social class of 3.51 (S.D. = 1.33). The means of the distributions were not significantly different ( $t = 1.43$ ). The variance ratio was not significant ( $F = 1.10$ ,  $df = 177, 229$ , N.S.) implying that the variance of the distributions did not differ significantly.

The social class of the refusers and non-contacts was estimated where possible (vide supra). There was a non-significant trend towards the refusers being estimated as being of a higher social class. A similar non-significant trend indicated that the non-contacts might have been of a lower social class than the respondents (see Table 3).

### Employment Status of the Respondents

Following Atkinson (1971) respondents were regarded as being in gainful, Full-time employment if they were employed for over 30 hours a week. Fifty three per cent of the sample were in full-time employment. A significant sex difference was present with, as might be expected, a greater proportion of males (63%) being in full-time employment than females (45%) ( $\chi^2 = 11.92$ ,  $d.f. = 1$ ,  $P <$

TABLE 5 DISTRIBUTION OF RESPONDENTS BY SOCIAL CLASS AND BY SEX

SOCIAL CLASS GROUPINGS	NUMBER OF MALES	NUMBER OF FEMALES
1.	14	16
2.	26	41
3a.	22	41
3b.	67	82
4.	34	32
5.	15	18
TOTAL NUMBER	178	230
MEAN SOCIAL CLASS	3.71	3.51
STANDARD DEVIATION	1.36	1.33

0.001).

#### Marital Status of the Respondents

The distribution of respondents by marital status and by sex is presented in Table 6. Sixty four per cent of the respondents were married. A significant sex difference was present. A higher proportion of males (73%) than females (57%) were married (chi-squared = 6.70, d.f. = 1,  $P < 0.01$ ). Twenty two per cent of all of the respondents were single, with 22 per cent of the males and 21 per cent of the females being single. These proportions were not significantly different (chi-squared = 0.00, d.f. = 1, N.S.). Eleven per cent of the respondents were widowed, with 4 per cent of the males and 17 per cent of the females being widowed. These proportions were significantly different (chi-squared = 15.73, d.f. = 1,  $P < 0.001$ ). Three per cent of the sample were divorced or separated. One per cent of the males and 5 per cent of the females were divorced. These proportions were significantly different (chi-squared = 3.91, d.f. = 1,  $P < 0.05$ ).

Thus, within the sample, more men than women were married, and more women than men had lost their spouses through divorce, separation or bereavement.

#### Size of the Respondent's Household

Following Atkinson (1971) the size of the respondent's household was specified by the question "How many people live here regularly who are catered for by the same person as yourself?" (Detailed inclusion and exclusion criteria were specified in Interviewers' Manual 2).

TABLE 6 DISTRIBUTION OF RESPONDENTS BY MARITAL STATUS AND BY SEX

MARITAL STATUS	NUMBER OF MALES	NUMBER OF FEMALES
SINGLE	39	49
MARRIED	130	130
DIVORCED	2	12
WIDOWED	7	39
TOTAL NUMBER	178	230

The distribution of the size of the respondent's household is presented in Table 7.

The overall mean size of household was 3.05 (S.D. = 1.62), the modal frequency being 2. The mean size of household for the males was 3.16 (S.D. = 1.56) while for the females it was 2.97 (S.D. = 1.67). The difference in means was not statistically significant ( $t = 1.19$ , N.S.). The variance ratio was not significant ( $F = 1.31$ , d.f. 229, 177, N.S.) implying that the variances of the distributions did not differ significantly.

#### Religious Affiliation of the Respondent

The respondents' religious affiliation is recorded in Table 8.

Respondents did not have to be actively involved with their religious group, or regular attenders at a place of worship to be assigned to a religious affiliation. Seventy per cent of the respondents indicated that they were of the Protestant faith, 24 per cent indicated that they were of the Roman Catholic faith, while 6 per cent indicated that they had either no affiliation, or were affiliated with a small religious group.

The frequency of affiliation to the Protestant faith was significantly higher than to the other two categories (chi-squared = 264.45, d.f. = 2,  $P < 0.001$ ). There was no significant variation in religious affiliation in relation to the respondents sex (chi-squared = 1.50, d.f. = 2, N.S.).

TABLE 7 DISTRIBUTION OF RESPONDENTS BY SIZE OF HOUSEHOLD AND BY SEX

SIZE OF HOUSEHOLD	NUMBER OF MALES	NUMBER OF FEMALES
1	13	49
2	65	67
3	40	30
4	29	38
5	14	24
6	5	15
7	12	7
TOTAL NUMBER	178	230
MEAN SIZE OF HOUSEHOLD	3.16	2.97
STANDARD DEVIATION	1.56	1.67



TABLE 8 DISTRIBUTION OF RESPONDENTS BY RELIGIOUS AFFILIATION AND BY SEX

RELIGIOUS AFFILIATION	NUMBER OF MALES	NUMBER OF FEMALES
PROTESTANT	118	166
ROMAN CATHOLIC	47	52
OTHER	13	12
TOTAL	178	230

### SUMMARY

The response rate of 77 per cent was regarded as adequate for the purposes of the study. The sample was heterogenous in structure. While females predominated the whole age range was well represented and the social class structure was evenly distributed about the modal class i.e. 3B 'Skilled Occupation - Manual'.

While a significant age difference was detected when the respondents and the non-contacts were compared, in general, it appeared that the respondents did not differ substantially from either the refusers or the non-contacts.

## CHAPTER 5 SYMPTOM SCORES AND THE DERIVATION OF DEPRESSIVE SYNDROMES

This chapter has two main objects. Firstly, to describe the distributions of response to symptom scales and items and secondly, to describe the process of deriving the dependent variables through the application of Principal Component Analysis.

### RESPONSES TO SYMPTOM SCALES

#### Self-Rating Depression Scale

The distribution characteristics for the Self-Rating Depression Scale (SDS; Zung, 1965) are tabulated in Table 9.

The mean score for the total sample was 13.50 (SD = 9.04) with the mean of the male sub-sample being 11.67 (SD = 8.45) and that of the female sub-sample being 14.87 (SD = 9.28). The means of the two sub-samples were significantly different ( $t = 3.64$ ,  $p < 0.002$ ) with the female respondents obtaining significantly higher scores than the male respondents. The variance ratio was significant ( $F = 1.45$ ,  $df = 229, 177$ ,  $P < 0.02$ ) implying that the variances of the distributions were significantly different. The variance of the female sub-sample distribution was larger than that of the male sub-sample indicating a greater spread of scores for the females. Examination of the skewness and kurtosis of the two distributions indicated that both distributions were positively skewed and leptokurtic in form.

Briefly, these results were regarded as being consistent with

TABLE 9 DISTRIBUTION OF RESPONDENTS SCORES ON SELF RATING DEPRESSION SCALE, SELF-RATING SCALE OF DISTRESS AND DEPRESSIVE ADJECTIVE CHECKLIST BY SEX

(a) TOTAL SAMPLE

<u>SYMPTOM SCALE</u>	<u>DISTRIBUTION CHARACTERISTICS</u>			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
Self-Rating Depression Scale	13.50	9.04	1.03	1.29
Self-Rating Scale of Distress	3.49	3.78	1.68	3.79
Depressive Adjective Checklist	5.45	3.08	0.70	0.78

(b) FEMALE SUB-SAMPLE

<u>SYMPTOM SCALE</u>	<u>DISTRIBUTION CHARACTERISTICS</u>			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
Self-Rating Depression Scale	14.87	9.28	0.87	0.81
Self-Rating Scale of Distress	4.06	4.06	1.46	2.59
Depressive Adjective Checklist	5.16	2.89	0.50	0.17

(c) MALE SUB-SAMPLE

<u>SYMPTOM SCALE</u>	<u>DISTRIBUTION CHARACTERISTICS</u>			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
Self-Rating Depression Scale	11.67	8.45	1.33	2.60
Self-Rating Scale of Distress	2.74	3.27	2.09	6.96
Depressive Adjective Checklist	5.79	3.30	0.82	0.98

expectations. The distributions were positively skewed, that is, most respondents acknowledged few or no symptoms. Female respondents experienced a greater variability in symptomatology and a significantly higher mean level (Weissman and Klerman, 1977).

Mean Scores on the SDS do not provide an intuitively satisfactory expression of the level of symptomatology present. Barrett et al (1978) reviewed the extensive literature in which the SDS has been applied and derived ad hoc symptom levels which provide a crude pointer to the severity of symptoms present. Barrett et al (1978) indicated that this classification system is subject to misclassification and it is only presented here as a supplement to the distribution characteristics presented above. The classification system will not be used in any of the forthcoming analyses but it is hoped that it will provide an intuitive 'feel' for the symptom data.

The description of each level together with the number of male and female respondents who attained each level are presented in Table 10. Seventy-six per cent of the respondents obtained scores at level 0, that is, they reported no symptoms or an insignificant number of symptoms. Sixteen per cent reported symptoms at level 1, that is, at a level where symptoms were definitely present but of a severity less than that which would normally lead to referral for medical treatment. Six per cent reported symptoms at level 2, that is, at a level similar to patients who would attend an out-patient psychiatric clinic because their main problem was depression. Two per cent reported symptoms at level 3, that is, at a level similar to psychiatric patients who have depression as their main problem. These results were within the

TABLE 10 DISTRIBUTION OF RESPONDENTS BY LEVEL OF SYMPTOMATOLOGY BY SEX

SYMPTOM LEVEL	NUMBER OF RESPONDENTS		
	TOTAL SAMPLE	FEMALES	MALE
0. No or insignificant symptomatology	306	156	150
1. Definite symptoms of depression present - below level that would result in referral for medical treatment	70	50	20
2. Depressive symptoms of a level similar to outpatients with depression as their main problems	24	19	5
3. Depressive symptoms of a level similar to in patients with depression as their main problems	8	5	3
TOTAL NUMBER	408	230	178

range, albeit high in the range, of results obtained by previous studies which have used questionnaire techniques (Weissman and Myers, 1978). They implied that a sizable minority of the sample were symptomatic. A significant sex difference was present (chi-squared = 15.24,  $df = 3$ ,  $P < 0.01$ ).

#### Self-Rating Scale of Distress

The distribution characteristics for the Self-Rating Scale of Distress (SRSD, Kellner and Sheffield, 1973) were tabulated in Table 9. The mean score for the total sample was 3.49 (SD = 3.78) with the mean of the male sub-sample being 2.74 (SD = 3.27) and that of the female sub-sample being 4.06 (SD = 4.06). The means of the two sub-samples were significantly different ( $t = 3.66$ ,  $P < 0.002$ ) with as might be expected, the mean for the female respondents being higher than that of the males. The variance ratio was significant ( $F = 2.36$ ,  $df = 229, 177$ ,  $P < 0.002$ ) indicating that the variances of the two sub-samples differed significantly. As with the scores for the SDS, the female sub-sample displayed a greater spread of scores. Examination of the skew and kurtosis of the two distributions indicated that they were both positively skewed and leptokurtic in form. The scores of the male sub-sample were particularly peaked in form.

#### Depressive Adjective Checklist

The distribution characteristics for the Depressive Adjective Checklist (DACL) were tabulated in table 9. The mean score for the total sample was 5.45 (SD = 3.08), with the mean of the male sub-sample being 5.79 (SD = 3.30) and the mean of the female sub-sample

being 5.16 (SD = 2.89). The means of the two sub-samples were not significantly different ( $t = 1.84$ ,  $P < 0.1$ ). The variance ratio was significant ( $F = 1.71$ ,  $df = 177, 229$ ,  $P < 0.02$ ) indicating that the spreads of the scores were significantly different. Unlike the distribution of the two previous scales, the variance of the males' scores was significantly greater than that of the females.

Both distributions were positively skewed and leptokurtic in character. This was to a lesser degree than was the case with other symptoms scales.

#### Rating Scale for Primary Depressive Illness

The distribution of scores on each of the items taken from the Rating Scale for Primary Depressive Illness (SRPDI) were tabulated in Table 11. Twenty eight per cent of the total sample were rated as experiencing at least some tension and difficulty in relaxing. With regard to the depression item, 18 per cent of the total sample were rated as experiencing pessimism about the future, feelings of hopelessness, and a further 7 per cent were rated as being more severe, showing signs of tearfulness. This estimate of 25 per cent of respondents showing symptoms of depression is surprisingly close to the estimate of 24 per cent, which was derived independently through the procedure of Barrett et al (1978).

Eleven per cent of the sample were rated at the first point on the Agitation Scale. That is, they were observed to fidget throughout the interview. Thirteen per cent of the sample were rated as symptomatic on the Retardation Scale, 9 per cent being rated as having a slight



TABLE 11 DISTRIBUTION OF RESPONDENTS BY HAMILTON DEPRESSION SCALE ITEMS (RSPDI) AND BY SEX (EXPRESSED AS PERCENTAGES)

(a) TOTAL SAMPLE

SYMPTOM	<u>SCALE POINTS</u>				
	0	1	2	3	4
ANXIETY	72	23	5	0	0
DEPRESSION	75	18	6	1	0
AGITATION	89	11	0	0	0
RETARDATION	87	9	4	0	0
PARANOID	99	1	0	0	0

(b) FEMALE SUB-SAMPLE

SYMPTOM	<u>SCALE POINTS</u>				
	0	1	2	3	4
ANXIETY	71	25	4	0	0
DEPRESSION	72	20	7	1	0
AGITATION	88	11	1	0	0
RETARDATION	85	10	5	0	0
PARANOID	99	1	0	0	0

(c) MALE SUB-SAMPLE

SYMPTOM	<u>SCALE POINT</u>				
	0	1	2	3	4
ANXIETY	73	21	6	0	0
DEPRESSION	79	15	6	0	0
AGITATION	92	8	0	0	0
RETARDATION	89	9	2	0	0
PARANOID	98	2	0	0	0

flattening of affect and fixity of expression and 4 per cent a tendency to sit motionless, be delayed in their responses to questions and to respond in a monotonous voice. Only 1 per cent of the respondents were rated on the Paranoid item. This merely represented doubtfulness or trivial suspicions.

Surprisingly, no significant sex differences were observed on these items (Anxiety,  $X^2 = 0.14$ ; Depression,  $X^2 = 2.31$ ; Agitation,  $X^2 = 1.58$ ; Retardation,  $X^2 = 0.81$ ; Paranoid  $X^2 = 0.54$ ; All  $df = 1$ , NS).

#### THE DEVELOPMENT OF DEPENDENT VARIABLES

Rabkin and Struening (1976) in their influential review of the life-event literature argued that, 'The continued use of one measure to represent an obviously complex domain of symptoms will frequently lead to limited and erroneous conclusions.' (p 1020).

It was argued above that a strong case can be made in support of this view. This case will not be repeated here, but rather the development of suitable dependent variables will be described.

The four symptom scales, that were described above, could have been used as individual dependent variables. This was thought to be inadvisable, however, because of the heterogenous nature of the symptoms that were included in the scales.

Blumenthal (1975) underlined this essential heterogeneity by demonstrating that the SDS alone could be divided into three distinct clusters of symptoms. Given this, it was thought that ad hoc

groupings of symptoms should be avoided and that a systematic method of grouping the symptoms was required.

#### PRINCIPAL COMPONENT ANALYSIS AND THE DEVELOPMENT OF DEPENDENT VARIABLES

Principal Component Analysis is a technique that has been used extensively in the classification of depressive symptoms (Garside and Roth, 1978; Paykel, 1981). It extracts and describes the major dimensions of variation that underpin the interrelationships among variables, in this present case, symptoms.

Following Eysenck (1970) and Garside and Roth (1978), it may be argued that Principal Component Analysis may be applied to identify syndromes; syndromes being composed of symptoms that are correlated amongst themselves but relatively uncorrelated with other symptoms.

Using Principal Component Analysis in the derivation of dependent variables may have advantages with regard to the statistical conclusion validity, internal validity and construct validity of causal statements.

Statistical conclusion validity may be enhanced because applying Principal Component Analysis as an item-analysis technique to the heterogeneous symptoms of depression tends to increase measurement reliability (Armor, 1969).

Similarly, in life event studies such as this current study, the internal validity of causal statements may be enhanced by developing

independent variables in this fashion. A major threat to internal validity comes from direct contamination. That is, when the interviewer's ratings of life event characteristics are influenced by his knowledge of the respondent's symptoms. While Principal Component Analysis does not solve this problem it diminishes it by distancing the interviewer from the derivation of the final syndrome scores. The syndromes and thus the respondent's scores on each syndrome are obtained separately from his ratings on the life event schedule. This is clearly not a foolproof procedure as an interviewer might rate a highly symptomatic respondent as having experienced more events. This is likely to be a general tendency rather than a specific tendency. It is less likely, therefore, that he will rate a respondent as having more events because he observes a particular sub-set of depressive symptoms. The demonstration of specificity, i.e. events relating to only one of several syndromes, would tend to indicate that direct contamination had not occurred.

Construct validity may also be enhanced by using this approach in the development of dependent variables. Cook and Campbell (1979) argued that construct validity is increased by tests of convergence between symptoms and tests of divergence from other symptoms. Principal Component Analysis is an ideal tool for this purpose as it classifies symptoms into intercorrelated clusters and distinguishes them from other symptoms (Eysenck, 1953, 1970; Garside and Roth, 1978).

Eysenck (1953) and Royce (1966) specifically indicated that construct validity may be enhanced through the application of factor analysis -\* the constructs being refined by the exclusion of irrelevant elements or symptoms.

Royce (1966), in supporting this position, defined a factor as '...a construct operationally defined by its factor loadings.' (p 173). The application of Principal Component Analysis, therefore, reduces symptom heterogeneity and provides clear operational definitions of the constructs or syndromes underlying the symptoms.

There has been an extensive debate in the literature on whether depressive disorders are best conceptualised as being quantitatively or qualitatively distinct from normal experience (e.g. Eysenck, 1970; Kendell, 1976; Garside and Roth, 1978). For example, is a patient who is diagnosed as suffering from endogeneous depression experiencing something that is quantitatively or qualitatively distinct from normal experience? Principal Component Analysis can be used to describe the syndromes of depression under either of these assumptions about the nature of depression. Cattell et al (1966) and Garside and Roth (1978) indicated that the specification of syndromes may occur either

\* footnote

Strictly speaking the terms component and factor are not synonymous. Component Analysis aims to summarise the variation in data, working towards a hypothetical model, whereas, Factor Analysis tests the model against data. Garside and Roth (1978) have indicated that the theoretical and computational differences are often insignificant, and they stated "... to maintain a rigid distinction between principal components and factor analysis is rather academic." (p 58)

in the presence or in the absence of discrete types of subjects or taxonomic entities. Maxwell's (1972) influential proscription of the use of Factor Analysis with non-homogeneous samples appears to apply only to certain restrictive forms of Factor Analysis (Garside and Roth, 1978). The construct validity of dependent variables can therefore be increased by the application of this technique to symptoms irrespective of the nature of depression, i.e. whether it is most parsimoniously described in dimensional terms or categorical terms or a mixture of both (Eysenck, 1970; Garside and Roth, 1978; Cooke, 1980a).

#### THE PRINCIPAL COMPONENT ANALYSIS OF SYMPTOM INFORMATION

##### The Selection of Variables for Inclusion

Before the Principal Component Analysis was carried out certain of the symptom scales items were excluded from the analysis. They were excluded for a variety of reasons. Following Ni Bhrolchain et al (1979) symptoms reported by less than 15 per cent of the sample were excluded from the analysis. In correlation analysis, spuriously high correlations can occur between symptoms that are infrequent or rare and this can result in distortion of the obtained component structure (Maxwell, 1972).

Applying this criterion resulted in the exclusion of the SDS item relating to suicidal references (See table 12) and the exclusion of three items from the RSPDI, namely Agitation, Retardation and Paranoid symptoms (See table 11).

TABLE 12 DISTRIBUTION OF RESPONDENTS' SCORES ON ITEMS OF THE SELF-RATING DEPRESSION SCALE, SELF-RATING SCALE OF DISTRESS AND THREE ADDITIONAL ITEMS \* (PERCENTAGES)

Symptoms	Rarely or none of the time ( $< 1$ day)	Some or little of the time(1 -2 days)	Occasion- ally or moderate amount of time (3-4 days)	Most of the time (5-7 days)
Restlessness (KS3) <sup>xx</sup>	62.5	27.5	8.5	1.5
Tension (KS8)	54.5	30.2	11.7	3.6
Feeling Scared (KS1)	76.9	17.5	4.6	1.0
Panic Attacks (KS6)	85.1	10.2	3.2	0.5
Ruminations (KS5)	59.1	25.1	13.9	1.9
Shaking (KS4)	84.2	12.2	2.2	1.5
Crying Spells (Z3)	82.0	11.7	4.4	1.9
Constipation (Z8)	82.8	9.2	2.2	6.3
Increase Heart Rate (Z9)	70.3	16.3	7.1	6.3
Self Pity (A)	84.1	9.0	4.4	2.5
Personal Devaluation (Z19)	73.7	11.7	5.6	9.0
Feelings of Emptiness (Z14)	69.6	11.7	8.8	10.0
Personal Dissatisfaction (Z18)	73.2	7.8	7.8	11.2
Indecisiveness (Z16)	82.5	13.4	2.7	1.5
Subjective Slowing (Z12)	50.9	16.8	12.9	19.5
Feelings of Hopelessness (Z15)	62.0	20.0	11.4	6.6
Subjectively Inefficient Thinking (Z13)	60.3	15.3	9.7	14.6
Difficulty falling asleep (KS7)	64.7	16.1	9.7	9.5
Loss of Libido (Z6)	50.4	9.2	3.6	36.7
Depressed Mood (Z1)	67.9	21.9	6.6	3.6
Fatigue (Z10)	70.3	16.3	7.1	6.3
Appetite Loss (Z5)	60.6	8.8	6.8	23.8
Feelings of guilt (A)	82.5	13.4	2.7	1.5
Diurnal Variation in Mood (Z2)	58.2	9.7	8.5	23.6
Early Wakening (A)	64.5	13.6	6.6	15.3
Agitation (Z11)	61.3	18.5	10.2	10.0
Irritability (Z17)	62.0	20.2	11.4	6.6
Loss of Weight (Z7)	81.5	7.8	3.2	7.5
Suicidal References (Z20)	95.4	2.4	0.7	1.5

\* See chapter 3 the "Detection of Symptoms"

<sup>xx</sup> Item numbers on original scales  
 KS = Kellner-Sheffield Items (SDS)  
 Z = Zung Items (SRSD)  
 A = Additional Items

The item relating to sleep disturbance in the SRSD was excluded because it was essentially identical to an item in the SDS.

Only two RSPDI items remained after the exclusion on the grounds of rarity, it was therefore, decided to dispense with these also. Several advantages may accrue from their exclusion. Firstly, given that the same rater assessed both these symptom ratings and the life-event ratings, there was an obvious danger of direct contamination. Secondly, given that these symptom ratings were obtained using a different procedure from the majority of symptoms ratings, there was a danger that a method factor might arise in the analysis. That is, a factor that might merely reflect the different procedures used to obtain the symptom data.

The total DACL score was also excluded because of the problem of method factors.

#### Methodological Aspects of the Principal Component Analysis

Twenty-nine items remained after the above exclusions were carried out. This gave a subject to variable ratio of 14.07:1, which was well above the conventionally accepted limits for even inherently unreliable information (Everitt, 1975; Garside and Roth, 1978). The subjects' scores on each of the symptom items were log-transformed to reduce the influence of positive skew (Maxwell, 1972).

Principal Component Analysis was then performed followed by the Scree test to determine the number of components that should be interpreted.



Cattell and Vogelman (1977) demonstrated the superiority of the Scree test over the commonly used Kaiser-Guttman Unit Root criterion for determining the number of components in a wide range of psychological data. Following the Scree test four components were interpreted, accounting respectively for 21.7%, 8.5%, 5.3% and 4.5% of the variance.

### Rotation of Components

Within the literature on the classification of depressive symptoms by the application of Factor Analysis, there has been a dispute about the rotation of factor. Some authorities favour the interpretation of the unrotated solution while others eschew this in favour of rotated solutions. Part of this dispute relates to the different purposes to which the components may be applied. Garside and Roth (1978) insisted that unrotated solutions should be given precedence over rotated solutions because they have a discriminative rather than a descriptive function. That is, unrotated solutions may be used to maximise the discrimination between different types of subjects or patients. Garside (1976) argued that unrotated components could be used to discriminate between endogenous and reactive subjects while the rotated components measured the depth of endogenous or reactive depression.

Other authorities, however, have argued that unrotated solutions lack psychological meaning and are predominantly a function of factorial design (e.g. Guilford, 1968; Horst, 1966).

Garside and Roth (1978) illustrated that the discriminating

characteristics of an unrotated solution could have value in situations in which severity of symptomatology was of limited relevance e.g. when applied to discriminating between types of patients, i.e. people who have previously been identified as being ill. They agreed, however, with Eysenck's (1970) contention that it is inappropriate to use a discriminating dimension with a random sample of the general population. Rotated components have their own function. Garside (1976) indicated that rotated components have a descriptive function in that they provide a measure of depth or severity of disorder.

Given that the purpose of this Principal Component Analysis was to establish homogenous measures of symptom severity rather than to discriminate among patients or other groups of respondents, it was decided to carry out Varimax rotation. The four rotated components accounted respectively for 15.5%, 10.6%, 7.9% and 6.02% of the variance.

#### The Interpretation of the Rotated Components

The component loadings are tabulated in table 13.

The first rotated component was interpreted as being an 'anxiety-depression' component. It was characterised by anxiety symptoms such as nervousness, panic attacks and tension together with depressive symptoms such as depressed mood, crying spells and guilt. The second component was characterised by cognitive experiences such as dissatisfaction, personal devaluation, hopelessness and subjective

TABLE 13 SYMPTOM LOADINGS ON PRINCIPAL COMPONENTS AFTER ORTHOGONAL  
ROTATION (VARIMAX) - DECIMAL POINTS OMITTED

Symptom	Components			
	I	II	III	IV
Restlessness	73	05	06	16
Tension	72	09	17	22
Nervousness	71	16	13	13
Feeling Scared	68	16	01	-15
Panic Attacks	68	16	-04	07
Ruminations	51	09	14	34
Shaking	51	16	25	-15
Crying Spells	43	07	41	22
Constipation	38	-05	37	-18
Increased Heart Rate	26	05	33	04
Self Pity	31	15	29	19
Personal Devaluation	13	70	-05	-04
Feelings of Emptiness	14	64	05	05
Personal Dissatisfaction	12	63	30	08
Indecisiveness	17	62	-03	03
Slowed Thinking	16	59	21	-11
Feelings of Hopelessness	-04	57	21	18
Subjectively Inefficient Thinking	06	57	03	-08
Difficulty Falling Asleep	26	01	57	12
Loss of Libido	-06	18	52	05
Depressed Mood	40	07	49	16
Fatigue	41	13	45	14
Appetite Loss	-06	35	36	13
Feelings of Guilt	36	12	-40	33
Diurnal Variation in Mood	-06	-07	-10	57
Early Wakening	11	02	23	56
Agitation	37	-07	12	43
Irritability	41	08	-04	43
Loss of Weight	07	15	28	33

slowing. The third component might loosely be related to vegetative functioning, being characterised by loss of appetite and libido, fatigue, constipation and depressed mood.

The fourth component was specified by the classic endogenous triad of diurnal variation in mood, early wakening and weight loss together with agitation and irritability.

In order to aid communication these four constructs, defined by their factor loadings were labelled as 'anxiety-depression', 'cognitive-depression', 'vegetative-depression' and 'endogenous-depression'. Component scores were computed using a complete estimation method. These scores were used as measures of degree or severity of each of the four syndromes (Eysenck, 1970; Garside, 1976).

#### SUMMARY

The distributions of responses to the symptom scales were described. Responses to the Zung Self-Rating Depression Scale suggested a comparatively high rate of psychological disturbance. Methodological issues pertaining to Principal Component Analysis were discussed. Principal Component Analysis was applied to refine the symptom information into syndromes. Four syndromes, namely 'anxiety-depression', 'cognitive-depression', 'vegetative-depression' and 'endogenous-depression', were defined.

## CHAPTER 6 LIFE EVENTS AND SYNDROMES OF DEPRESSION

This chapter has five specific aims. Firstly, the assessment of the reliability of specific event characteristics is described and the reliability of particular characteristics is discussed. Secondly, the rate of life events reported by the respondents as having occurred in the previous twelve months is described. Thirdly, the estimation of an index of overall life stress is described and respondents' distribution on this index is discussed. Fourthly, the relationship between this index and each of the four syndromes of depression described above is discussed. Fifthly, the linearity of the relationship between this index and 'anxiety-depression' is examined.

### ESTIMATING THE RELIABILITY OF LIFE EVENT RATINGS

The reliability of individual life event ratings was assessed using an inter-rater reliability procedure rather than a test-retest procedure. Maxwell (1977) noted that, for this type of information, consensus of agreement between raters is likely to be an important pointer towards construct validity. The test-retest procedure was not used because of practical and theoretical considerations. Firstly, it was thought that subjects would be unlikely to agree to be subjected to an additional lengthy interview sufficiently soon after the first interview to make test-retest procedures valid. Secondly, Nunnally (1967) indicated that the subjects' tendency to remember previous responses, and indeed a tendency to become blase about topics recently discussed, seriously affected the value of a test-retest approach in this context.

Grove et al (1981) argued that the determination of inter-rater reliability in the real interview situation was a useful procedure. This type of procedure was adopted, with a rater observing and rating simultaneously with, and independent of, the interviewer. While this procedure had the advantage of being an assessment of the real interview situation, rather than an analogue situation, such as video vignettes or readily accessible patients rather than general population respondents, it possessed the hazard that spuriously high agreement might occur because the rater received cues from the interviewer. On balance, however, it was thought that the advantages of this procedure outweighed its disadvantages.

Inter-rater reliability was assessed using the Kappa coefficient for all but one of the reliability estimates. Reliability was assessed using the Kapa coefficient, rather than by percentage agreement or by product moment correlation, because Kappa corrects for inter-rater agreement due to chance (Bartko and Carpenter, 1976; Maxwell, 1977). Weighted Kappa was not used because of the lack of any clear rationale for the determination of weights. The one exception was the estimation of the level of agreement for the 'life event number' rating. This was assessed using a simple per cent agreement coefficient because the level of chance agreement on 64 possible ratings was likely to be low.

#### INTER-RATER RELIABILITY FOR EVENT CHARACTERISTICS

Two pairs of interviewers simultaneously interviewed and rated their respondents (Vide Supra). Joint ratings were available on 122

subjects for pair 1 and 92 subjects for pair 2. This represents 52 per cent of all the obtained interviews or 79 per cent and 71 per cent respectively of the interviews carried out by each of the pairs of interviewers. Failure to achieve joint interviews for all the interviews was due to absence, generally through illness, of one or other of the interviewers within a pair. Given this level of joint interviewing it was thought that the ratings used in the assessment of inter-rater reliability might be representative of the ratings as a whole.

To confirm this impression, however, it was necessary to carry out further analysis. There was a danger that the jointly rated interviews might not be representative of the interviews as a whole, because of a non-random tendency of the interviewers to ensure that they always interviewed particular types of respondents as a pair. It was noted above that the original design of five individually interviewed samples was abandoned because of the female interviewers' fears about interviewing alone. Bias might have emerged, therefore, if single interviewers decide to interview female subjects in the less 'dangerous' areas of the catchment area. This tendency was evaluated by examining the age, sex and social class characteristics of those subjects who were approached by a pair of interviewers in contrast to those who were approached when the interviewer found herself on her own. The analyses were carried out for each of the two pairs of interviewers. No significant sex, age or social class differences emerged (See Table 14).

A further bias might have emerged if interviewers on their own tended

TABLE 14 - MAJOR DEMOGRAPHIC AND LIFE EVENT VARIABLES BY NUMBER OF INTERVIEWERS FOR INTERVIEWER PAIRS 1 AND 2

NUMBER OF INTERVIEWERS								
	1			2				
VARIABLE	MEAN	S.D.	n	MEAN	S.D.	n	t	SIGN
TOTAL NO. OF LIFE EVENTS								
PAIR 1	2.48	1.36	32	2.02	1.58	122	1.64	N.S.
PAIR 2	1.94	1.64	38	2.26	1.94	92	0.97	N.S.
TOTAL LIFE STRESS (T.L.S)								
PAIR 1	30.22	17.74	32	24.48	19.11	122	1.58	N.S.
PAIR 2	24.47	22.54	38	25.70	21.84	92	0.28	N.S.
AGE								
PAIR 1	48.19	19.24	32	48.36	17.64	122	0.05	N.S.
PAIR 2	51.85	16.30	38	47.90	16.75	92	1.23	N.S.
SOCIAL CLASS								
PAIR 1	3.66	1.38	32	3.52	1.14	122	0.54	N.S.
PAIR 2	3.84	1.26	38	3.62	1.49	92	0.88	N.S.
	MALE	FEMALE		MALE	FEMALE		$\chi^2$	SIGN
SEX								
PAIR 1	21	11		64	58		1.78	N.S.
PAIR 2	17	21		43	49		0.04	N.S.



to be less fastidious or more cursory than interviewers operating as a pair. An individual interviewer, who was perhaps anxious, might have had a tendency to rush the interview and therefore, not detect as many events or not detect events of less salience. When the total number of life events recorded by interviewers operating in pairs was compared to interviewers operating on their own, no significant differences emerged (see Table 14).

A further comparison was made using a weighted index called total life stress (*vide infra*), that was derived by weighing each event for severity of impact. It was feared that differences could emerge in this index, even when the total number of life events did not differ, because of differential recording of events of different degrees of salience. No significant differences, however, emerged in cross group comparisons of this index (See Table 14).

It may be concluded from these five analyses that the ratings carried out by interviewers operating in pairs did not differ significantly from those obtained when they operated as individuals. It is likely, therefore, that reliability estimates based upon a sub-sample of the ratings can be generalised to all the ratings carried out by these four female interviewers.

While the results of these analyses were reassuring the danger still existed that interviewer drift might have resulted in ratings across each of the pairs and the fifth individual male rater being different.

It was noted above that three systematic samples of the population were drawn and that each of these was randomly assigned to teams of interviewers. Replication was carried out to allow the determination of interviewer effects.

The sex distribution of each of the replicated samples was compared using the chi-squared statistic and a significant sex difference emerged. Two samples had a very similar sex ratio while the third had a 2:1 female:male ratio (See Table 15). The existence of further demographic differences was explored by comparing the age and social class distributions of the three replicated samples by the use of one way analysis of variance. No significant differences emerged. Differences in demographic factors could have occurred due to selective interviewing or through the effects of chance. The principal concern of the cross-sample analysis was to determine the presence of interviewer drift. The total number of life events recorded within each sub-sample and the weighted total life stress score (vide infra) were compared using one way analysis of variance. No significant differences emerged and this suggested that the life event ratings were unlikely to be affected by rater drift.

In conclusion, it was assumed that firstly, the ratings carried out by interviewers acting in pairs were representative of those obtained by interviewers acting alone, and, secondly, that there was no evidence of cross-sample drift in the recording of number of events or types of events.

Given these conclusions it was assumed that the reliability estimates

TABLE 15 MAJOR DEMOGRAPHIC AND LIFE EVENT VARIABLES ACROSS THREE REPLICATED SAMPLES

	REPLICATED SAMPLES											
	PAIR 1			PAIR 2			OTHER					
VARIABLE	MEAN	S.D.	n	MEAN	S.D.	n	MEAN	S.D.	n	F	SIGN	
TOTAL NO. OF LIFE EVENTS	2.10	1.68	154	2.15	1.80	130	2.09	1.60	124	0.06	N.S.	
TOTAL LIFE STRESS	25.67	20.83	154	25.34	21.94	130	24.97	19.32	124	0.16	N.S.	
AGE	48.32	17.31	154	49.05	16.94	130	48.20	17.68	124	1.54	N.S.	
SOCIAL CLASS	3.60	1.31	154	3.69	1.42	130	3.50	1.25	124	1.39	N.S.	
SEX	MALE	FEMALE		MALE	FEMALE		MALE	FEMALE		$\chi^2$	SIGN	
	54	100		62	68		62	62		7.51	0.02	

were reasonably representative of the life event ratings as a whole. Pair 1 rated 260 individual events while pair 2 rated 221 individual events (See Table 16).

The Kappa coefficients for the number of events experienced in the previous twelve months were 0.95 and 0.89 for pair 1 and 2 respectively. The per cent agreement for the 'life event number' rating was 94 per cent and 90 per cent for pair 1 and 2 respectively. These estimates suggested that the interviewers could agree fairly successfully about the number and type of events experienced by each subject.

In particular, the level of agreement regarding the 'life event number' implied that the consensually derived weights of impact entailed by each event (Paykel et al, 1976) could be reliably assigned to each of the events.

The Kappa coefficients for the date of occurrence, expressed in twelve month categories, of each event were acceptably high (Pair 1, Kappa = 0.85; Pair 2, Kappa = 0.76), while one of those for the 'independence' of events was less satisfactory (Pair 1, Kappa = 0.82; Pair 2, Kappa = 0.62). Kappa coefficients for the other event ratings ranged from 0.52 to 0.84 with the majority of the coefficients being around the 0.60 level.

Nunnally (1967), in discussing standards of reliability, suggested that modest reliabilities, of this level, were adequate in the early stages

TABLE 16 INTER-RATER RELIABILITY COEFFICIENTS FOR EVENT CHARACTERISTICS RATED BY TWO PAIRS OF RATERS. (KAPPA OR PERCENTAGE AGREEMENT).

EVENT CHARACTERISTIC	<u>RELIABILITY COEFFICIENT</u>	
	PAIR 1	PAIR 2
Number of Events Experienced in Previous Twelve Months	0.95	0.89
Life Event Number	94%	90%
Time Between Interview and Occurence of Event	0.85	0.78
Independence of Event	0.82	0.62
Control Over the Event	0.61	0.76
Objective Negative Impact	0.64	0.58
Objective Positive Impact	0.64	0.71
Subjective Negative Impact	0.61	0.52
Subjective Positive Impact	0.67	0.84

of research. He warned, however, that the use of measures of this level of reliability would tend to attenuate the relationships among the variables of interest. Given the differential levels of reliability achieved in the rating of life events, it was thought that an index based on the 'life event number', one of the more reliable ratings, would be appropriate. The construction of this index will be described below after consideration of the rate of events reported by respondents.

#### THE RATE OF LIFE EVENTS REPORTED

The overall mean reported rate of life events was 2.11 over the previous twelve months (See Table 17).

The skewness and kurtosis of the distributions indicated that the distributions were skewed and leptokurtic in form and that only a few subjects experienced more than the mean number of events.

The rate of events reported by men and women did not differ significantly ( $t = 0.30$ , NS). The variance ratio was non-significant ( $F = 1.09$ ,  $df = 229, 177$ ) indicating that there was no difference in the variability of the simple event rate across sex.

Following Paykel et al (1976) individual life events were grouped in terms of area of activity (See Table 18).

It was observed that 'health' and 'bereavement' events were particularly frequently reported by men and by women. Indeed, slightly under half the reported events (45%) fell within these two

TABLE 17 DISTRIBUTION OF THE TOTAL NUMBER OF LIFE EVENTS EXPERIENCED  
IN THE PREVIOUS TWELVE MONTHS BY RESPONDENTS SEX

SAMPLE	DISTRIBUTION CHARACTERISTICS			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
TOTAL SAMPLE (n = 408)	2.11	1.69	0.97	1.23
FEMALE SUB-SAMPLE (n = 230)	2.10	1.73	1.06	1.67
MALE SUB-SAMPLE (n = 178)	2.15	1.64	0.84	0.84

TABLE 18 FREQUENCY OF EVENTS BY CATEGORY BY RESPONDENT'S SEX

EVENT CATEGORY (Event Number in Brackets)	MALE		FEMALE	
	No. Of Events	% of Total Events	No. Of Events	% of Total Events
Work (1-10)	51	14	40	8
Education (11-15)	6	2	11	2
Financial (16-18)	27	8	21	4
Health (19-25)	81	22	119	25
Bereavement (26-30)	79	21	98	20
Relocation (31-33)	4	1	3	1
Dating (34-37)	10	3	12	3
Legal (38-42)	11	3	13	3
Family and Social (43-63)	58	16	97	20
Miscellaneous (64)	35	10	65	14
TOTAL EVENTS	362		479	



categories.

#### THE ESTIMATION OF AN INDEX OF TOTAL LIFE STRESS (T.L.S.)

For the purpose of the initial analysis an index of total life stress (T.L.S.) experienced in the previous twelve months was derived. The adoption of commonly used dichotomous measures of the type used by Brown and Harris (1978a) was avoided because of the problems of statistical power and instability of causal inference referred to above.

Not all events experienced by the respondents in the previous twelve months, were included in the index. To impose the necessary asymmetry in the relationship between events and symptoms (*vide supra*) only those events that were rated as being 'almost certainly independent' or 'probably independent' were included in the analysis. This resulted in the exclusion of 94 or 11.2 per cent of the total number of events reported by respondents. This procedure could not be regarded as totally satisfactory firstly because of the low reliability of the independence ratings of one pair of interviewers (i.e.  $Kappa = 0.62$ ) and secondly, because the 'independence' rating referred to independence from symptoms of depression in general, rather than from particular syndromes of disorder. It is unlikely, however, that 'independence' from particular syndromes could be reliably measured.

Brown and his colleagues (Brown et al 1975, et seq.) only considered

those events that occurred prior to the onset of caseness. It was argued above, however, that there was an arbitrary element in designating events as pre or post onset and indeed, it may be difficult to retrospectively assess the onset of minor affective disorder (Susser, 1973; Tennant et al, 1981a; Cranach et al, 1981).

It was also contended above that the evaluation of the hypothesis that events lead to symptoms requires that events that occur after the onset of depression should be considered. This contention was empirically verified by Surtees and Ingham (1980), when they showed that the effects of 'post-onset' events summate with the effects of 'pre-onset' events to increase the severity of symptomatology reported. Post-onset events were therefore included in the index.

A total life stress score (T.L.S.) was generated by adding the consensually derived weights of the degree of upset entailed by particular events. British norms, that were published by Paykel et al (1976), were used.

Normative scores were not available for six of the events included in the interview schedule. These six events were arbitrarily assigned the average score of ten. Deriving the index in this manner was thought to diminish the threat of direct contamination; that is, the threat that the raters might elevate the stress scores for symptomatic subjects. Deriving the index in this way resulted in a measure of stress that was obtained independently of knowledge regarding the respondent's psychological state. This procedure could not be regarded as totally satisfactory for three reasons. Firstly, since

the correlation between the total number of events experienced by a subject and their T.L.S. score was perforce high, direct contamination could still influence T.L.S. scores if the interviewers had a tendency to question symptomatic subjects more assiduously about the events they had experienced. Secondly, this index resulted in a loss of idiographic sensitivity, because no account was taken of the personal meaning that the event had for the respondent. Thirdly, the index assumes additivity of effects, however, the validity of this assumption can be determined by analysis (vide infra).

The distribution of the T.L.S. scores was presented in Table 19.

The mean score for the total sample was 25.56 (SD = 20.72), with the mean of the female sub-sample being 25.49 (SD = 21.24) and that of the male sub-sample being 26.03 (SD = 20.06). The cross sex difference in means was not significant ( $t = 0.26$ ) and the variance ratio was also non-significant ( $F = 1.25$ ;  $df = 229, 177$ ; NS) implying that the variances of the two distributions did not differ significantly. This result was not surprising given that no significant cross-sex difference was observed in the number of events experienced and few marked differences in the types of events experienced were apparent.

#### TOTAL LIFE STRESS AND SYNDROMES OF DEPRESSION

Pearson's product moment correlation coefficients were calculated for the relationships between T.L.S. scores and scores on each of the four derived symptom components (See Table 20).

TABLE 19 DISTRIBUTION OF RESPONDENTS' SCORES ON TOTAL LIFE STRESS  
SCALE (T.L.S.) BY SEX

SAMPLE	DISTRIBUTION CHARACTERISTICS			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
TOTAL SAMPLE	25.56	20.72	0.88	0.62
FEMALE SUB-SAMPLE	25.49	21.24	0.93	0.76
MALE SUB-SAMPLE	26.03	20.06	0.81	0.47

TABLE 20 PEARSON PRODUCT MOMENT CORRELATIONS OF TOTAL LIFE STRESS (T.L.S.) WITH FOUR DEPRESSIVE SYNDROMES BY SEX

TOTAL LIFE STRESS	<u>SYNDROMES</u>			
	'ANXIETY DEPRESSION'	'COGNITIVE DEPRESSION'	'VEGETATIVE DEPRESSION'	'ENDOGENOUS DEPRESSION'
Total Sample (n = 408)	0.22*	0.03	0.05	0.04
Female Sub- Sample (n = 230)	0.32*	0.01	0.07	0.02
Male Sub- Sample (n = 178)	0.07	0.06	0.03	0.06

\* P < 0.002 (Two-Tailed)

For the total sample the correlation between T.L.S. and 'anxiety-depression' was significant ( $p < 0.002$ ) while the correlation between T.L.S. and each of the other three syndromes of depression did not depart significantly from zero. The index of overall life stress appeared, therefore, to be related to only one symptom pattern.

Hotelling's t-test procedure was used to determine the significance of difference of the correlation coefficients when the coefficients of correlation are correlated (Guildford and Fruchter, 1973). It indicated that the correlation between 'anxiety-depression' and T.L.S. not only departed significantly from zero, but was also significantly different from the correlation between T.L.S. and the other three symptom components ( $p < 0.005$ ).

Although significant, the relationship between T.L.S and 'anxiety-depression' was modest in size, accounting for only 4.8 per cent of the variance. When the sample was divided by sex, a marked cross-sex difference emerged. Within the female sub-sample T.L.S. was significantly correlated with 'anxiety-depression' but not with any of the other three syndromes. This significant relationship accounted for 10.2 per cent of the variance. The correlation between 'anxiety-depression' and T.L.S. within the female sub-sample was significantly different from the correlation between T.L.S. and each of the other three syndromes ( $p < 0.005$ ).

Within the male sub-sample there were no significant correlations between T.L.S and any of the four syndromes. The failure to

demonstrate a correlation between T.L.S. and 'anxiety-depression' in the male sub-sample similar to that found in the female sub-sample, could have resulted from range restriction in the 'anxiety-depression' scores of the men. It was noted above that, while there was no significant cross-sex difference in the variance of the T.L.S. scores there was a significant cross-sex difference in the variance of the 'anxiety-depression' scores. Using the procedure for correcting for range restriction (Guilford and Fruchter, 1973) the corrected correlation between T.L.S. and 'anxiety-depression' was calculated for the male sample, after making the strong assumption that the variance of their 'anxiety-depression' scores should have been the same as that for the female sub-sample. The corrected correlation was 0.09 and therefore not statistically significant. Thus, the absence of a significant correlation could not be attributed to the effects of range restriction.

Similarly, the failure of T.L.S. scores to correlate significantly with any of the three other syndromes of depression, apart from 'anxiety-depression', could not be attributed to the effects of range restriction.

The relationship between T.L.S. and 'anxiety-depression' was further explored. It was noted above that the presence of a linear relationship i.e. a dose response relationship, was persuasive of a causal relationship (Susser, 1973). The linearity of the relationship between T.L.S. and 'anxiety-depression' was examined through the use of Hierarchical Polynomial Regression (Cohen and Cohen, 1975) (See Table 21).

TABLE 21 HIERARCHICAL POLYNOMIAL REGRESSION OF ANXIETY-DEPRESSION WITH  
TOTAL LIFE STRESS = TOTAL SAMPLE AND FEMALE SUB-SAMPLE

TOTAL SAMPLE

VARIABLE	MULTIPLE R	BETA	SIGNIFICANCE OF R <sup>2</sup> CHANGE (F)
Total Life Stress	0.22	0.18	20.88*
(Total Life Stress) <sup>2</sup>	0.22	0.05	0.13
R <sup>2</sup> = 0.05	F = 10.55	P < 0.0001	n = 408

FEMALE SUB-SAMPLE

VARIABLE	MULTIPLE R	BETA	SIGNIFICANCE OF R <sup>2</sup> CHANGE (F)
Total Life Stress	0.32	0.20	25.73*
(Total Life Stress) <sup>2</sup>	0.32	0.13	0.60
R <sup>2</sup> = 0.10	F = 13.15	P < 0.0001	n = 230

\* P < 0.001



The addition of a quadratic term did not improve the variance explained in either the total sample or the female sub-sample. A cubic term could not be added to the regression equation within the tolerance limit of 0.01. These results implied that the relationship between T.L.S. and 'anxiety-depression' was essentially linear in quality (Cohen and Cohen, 1975) and thus conformed to a dose-response relationship. Therefore, within the total sample and sub-sample, subjects with higher levels of overall life stress tended to experience proportionately more 'anxiety-depression'.

#### SUMMARY

The reliability of the life event ratings was determined using inter-rater reliability procedures. Individual and paired interviews were compared and it was concluded that the paired interviews were representative of the interviews as a whole. Comparison of the three replicated sub-samples failed to provide evidence of cross sample drift in the recording of the number of, or type of events experienced. Following these comparisons, it was assumed that the inter-rater reliability coefficients derived from paired interviews were representative.

Inter-rater reliability coefficients for the 'Life Event Number' rating and the date of occurrence of the event were acceptable. The rating of independence was less satisfactory for one pair of raters. Kappa coefficients for the less concrete ratings ranged from 0.52 to 0.84.

The overall rate of life events was described and events were divided into areas of activity.

The rationale underpinning the index of total life stress (T.L.S) was outlined and the index was derived.

The product moment correlations between T.L.S. and each of the four syndromes were calculated.

Within the total sample the T.L.S. index only correlated with 'anxiety-depression'. The same pattern emerged in the female sub-sample with no significant T.L.S. - syndrome correlations being obtained in the male sub-sample.

The relationship between T.L.S. and 'anxiety-depression' was examined further in the total sample and the female sub-sample. Hierarchical polynomial regression analysis indicated that the relationships were essentially linear in quality.

## CHAPTER 7 THE INFLUENCE OF PARTICULAR TYPES OF EVENTS AND PARTICULAR EVENT CHARACTERISTICS

### TYPES OF LIFE EVENTS

The total life stress index (T.L.S.) which was discussed above, provided a consensually derived, normative measure of the overall level of stress experienced by each respondent over the previous twelve months. It was argued above that a move from general to more specific measures of life stress might be advantageous. Therefore, following Paykel et al (1969), life events were divided into contrasting types, viz. exits versus entrances and desirable versus undesirable. Exits events were defined as those events that entailed the departure of a significant other from the social field of the respondent (Paykel et al, 1976). Events in this category included bereavements and the departure of children from home. In contrast, entrances were defined as those events that entailed the entry of a significant other into the respondent's social field. This category of event included the birth of a child and marriage. Fourteen of the sixty three events were defined as exits, while seven were defined as entrances. The exits were further subdivided into deaths (4 events) and non-deaths (10 events).

Following Paykel et al (1976), desirable and undesirable events were defined as those events that were clearly socially desirable or undesirable. Nine of the sixty three events were defined as desirable, while thirty five were defined as undesirable.

Following the sub-division of events into these pairs of contrasting categories, the normative weights of upset entailed by each event (Paykel et al, 1976) were added together within each category, to provide indices of the extent of life event stress experienced within each of these categories. Each subject received a score that, for example, represented the degree of upset entailed by the exit events which he had experienced in the previous twelve months. Six indices were derived in this manner. Each of these indices were directly analogous to the T.L.S. index, except that they related to particular types of event rather than to events as a whole. It should be noted that while there was no overlap within pairs of these indices there was overlap across pairs of indices. Their distribution characteristics were tabulated in Table 22.

The mean level of life stress associated with undesirable life events, within the total sample, was 19.48. This contrasted markedly with the mean levels of stress associated with desirable and entrance events (i.e. 0.92 and 0.52 respectively). The majority of the distributions were skewed and excessively leptokurtic in character with the distributions for undesirable life stress being so to the least degree.

Four of the cross sex comparisons of variance were significant (variance ratios for exits,  $F = 1.36$ ,  $df = 229, 177$ ,  $P < 0.05$ ; entrances,  $F = 1.74$ ,  $df = 229, 177$   $P < 0.02$ ; undesirable,  $F = 1.23$ ,  $df = 229, 177$ , NS; desirable,  $F = 1.75$ ,  $df = 177, 229$ ,  $P < 0.02$ ; death exits,  $F = 1.16$ ,  $df = 177, 229$ , NS; non-death exits,  $F = 2.25$ ,  $df = 229, 177$ ,  $P < 0.02$ ). The female sub-sample had greater variance on

TABLE 22 DISTRIBUTION OF LIFE STRESS ASSOCIATED WITH PARTICULAR  
EVENT CATEGORIES: 'EXITS', 'ENTRANCES', 'UNDESIRABLE', 'DESIRABLE',  
'DEATH EXITS' AND 'NON-DEATH EXITS' BY SEX

VARIABLES	DISTRIBUTION CHARACTERISTICS			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
EXITS				
Total Sample (n = 408)	7.32	11.22	1.91	4.85
Female Sub-Sample (n = 230)	7.28	11.62	2.17	6.42
Male Sub-Sample (n = 178)	7.49	10.76	1.47	2.09
ENTRANCES				
Total Sample (n = 408)	0.52	2.11	5.69	44.07
Female Sub-Sample (n = 230)	0.48	2.24	6.68	57.06
Male Sub-Sample (n = 178)	0.57	1.95	3.64	12.89
UNDESIRABLE				
Total Sample (n = 408)	19.48	19.42	1.40	2.92
Female Sub-Sample (n = 230)	19.28	19.86	1.57	3.94
Male Sub-Sample (n = 178)	20.06	18.86	1.14	1.44
DESIRABLE				
Total Sample (n = 408)	0.92	2.47	3.18	11.18
Female Sub-Sample (n = 230)	0.87	2.32	3.42	14.49
Male Sub-Sample (n = 178)	1.01	2.67	2.91	8.29
DEATH EXITS				
Total Sample (n = 408)	5.30	9.70	1.93	3.72
Female Sub-Sample (n = 230)	5.14	9.57	1.86	3.04
Male Sub-Sample (n = 178)	5.61	9.94	1.99	4.46
NON-DEATH EXITS				
Total Sample (n = 408)	2.02	5.27	2.10	7.84
Female Sub-Sample (n = 230)	2.14	5.72	1.87	8.06
Male Sub-Sample (n = 178)	1.88	4.67	2.48	5.59

three of the four indices, with the male sub-sample only having greater variance on the desirable life stress index.

All of the cross sex differences in means, however, failed to reach significance (Separate variance t-test for exits,  $t = 0.19$ ; entrances,  $t = 0.40$ ; desirable,  $t = 0.54$ ; non-death exits,  $t = 0.51$ ; all NS) (Pooled variance t-test for undesirable,  $t = 0.40$ ; death exits,  $t = 0.48$ ; all NS).

#### THE RELATIONSHIP AMONG TYPES OF LIFE EVENT AND SYNDROMES OF DEPRESSION

Pearson product moment correlations for the relationships between each of the six indices of life stress and each of the four syndromes of depression were computed. Product moment correlations were used, despite the non-normality of the distributions of the event indices, because they have been shown to be robust to even extreme violations of the normality assumption (Havlicek and Peterson, 1977). Correlations were calculated for the total sample, male sub-sample and female sub-sample and are tabulated in Table 23.

Seventy-two correlation coefficients were calculated, seven of these were significant at or beyond the 5% level of significance. In general, therefore, these indices of specific types of life stress failed to correlate with the syndromes of depression.

At least three of the seventy two correlation coefficients were expected to be significant at the 5% significance level, through the

TABLE 23 PEARSON PRODUCT MOMENT CORRELATIONS OF PARTICULAR EVENT CATEGORIES WITH FOUR DEPRESSIVE SYNDROMES BY SEX

<u>VARIABLE</u>	<u>SYNDROMES</u>			
	'ANXIETY DEPRESSION'	'COGNITIVE DEPRESSION'	'VEGETATIVE DEPRESSION'	'ENDOGENOUS DEPRESSION'
<hr/>				
EXITS				
Total Sample	0.03	-0.01	-0.03	-0.05
Female Sub-Sample	0.09	0.00	-0.05	-0.07
Male Sub-Sample	-0.07	-0.02	0.00	-0.01
<hr/>				
ENTRANCES				
Total Sample	-0.01	-0.05	-0.02	0.18***
Female Sub-Sample	-0.06	-0.02	-0.02	0.13*
Male Sub-Sample	0.09	-0.11	-0.02	0.25***
<hr/>				
UNDESIRABLE				
Total Sample	0.21***	0.02	0.06	-0.03
Female Sub-Sample	0.32***	0.01	0.09	-0.05
Male Sub-Sample	0.04	0.04	0.02	-0.01
<hr/>				
DESIRABLE				
Total Sample	0.01	0.03	-0.07	0.06
Female Sub-Sample	0.09	0.01	-0.06	0.11
Male Sub-Sample	0.15*	0.06	-0.07	0.00
<hr/>				
DEATH EXITS				
Total Sample	0.01	0.03	0.00	0.06
Female Sub-Sample	0.08	0.01	0.02	-0.10
Male Sub-Sample	-0.07	-0.02	0.00	-0.01
<hr/>				
NON-DEATH EXITS				
Total Sample	0.04	-0.07	-0.06	0.04
Female Sub-Sample	0.05	-0.00	-0.12	0.03
Male Sub-Sample	-0.00	-0.18*	0.02	0.08
<hr/>				

\* P < 0.05 (two-tailed)

\*\* P < 0.01 (two-tailed)

\*\*\* P < 0.001 (two-tailed)

effects of chance. Since the three correlation coefficients that reached this level of significance did not form any recognisable pattern, they were left uninterpreted.

The level of life stress associated with entrance events was positively associated with 'endogenous depression' at the 0.001 probability level for both the total sample and the male sub-sample. The variance explained by this relationship was modest, being only 3% and 6% in the total sample and the male sub-sample respectively. This relationship may have been partly due to the influence of age. Within the total sample, age correlated negatively with both entrance life stress and 'endogenous depression' ( $r = 0.23$  and  $r = 0.24$  respectively,  $P < 0.002$ , two-tailed). It appeared, therefore, that the extent of entrance stress diminished with age as did the intensity of 'endogenous depression'. Partialling out the effects of age, using the partial correlation procedure (Guildford and Fruchter, 1973), diminished the correlation between entrance life stress and 'endogenous depression' to  $r = 0.13$  ( $P < 0.01$ , two-tailed) from  $r = 0.18$  in the total sample. Similarly, within the male sub-sample, partialling out the effects of age diminished the correlation between entrance life stress and 'endogenous depression' from 0.25 to 0.18 ( $P < 0.05$ , two-tailed). Thus the influence of age appeared to account for some of the counter-intuitive relationship between entrance life stress and 'endogenous depression'.

The relationship between undesirable life stress and the four syndromes of depression appeared to be very similar in pattern and in



magnitude to those obtained with the T.L.S. index. This result was not unexpected because the correlation between T.L.S. and undesirable life stress was very strong ( $r = 0.92$ ,  $P < 0.001$ , two tailed).

#### COMBINATIONS OF LIFE STRESS INDICES AND ANXIETY-DEPRESSION

Multiple Regression Analysis, with 'anxiety-depression' as the dependent variable and life stress indices as the independent variables, was carried out on the total sample, and the male and female sub-samples. The two aims of this analysis were firstly, to determine whether combinations of specific types of life stress increased the variance predicted in 'anxiety-depression' over that predicted by T.L.S. and secondly, to determine which types of life stress had the greatest predictive power.

Given that the purpose of these analyses was to maximise the variance explained in 'anxiety-depression', and since there was no compelling theoretical rationale that allowed the ordering of variables, Stepwise Regression Analysis was employed (Cohen and Cohen, 1975). Stepwise Regression Analysis enters the independent variables into the equation in an order that maximises the variance explained.

Within the total sample, one of the 15 inter-independent variable correlations was above 0.85. Althausar (1971) indicated that multicollinearity can distort the results of Multiple Regression Analysis when correlations reach the 0.85 level. Given that high correlations were rare, the analysis was performed, however, the

results should be interpreted with the caveat that multicollinearity may have influenced them (Table 24).

Within the total sample two life stress indices, namely, undesirable life stress and exit life stress, were entered into the regression equation within the tolerance level of 0.05. The variance explained in 'anxiety-depression' by these two variables was seven per cent compared to the five per cent explained by T.L.S. The beta coefficients associated with each of these variables were essentially equal in magnitude. This indicated that they both contributed equally to the prediction of 'anxiety-depression'. The beta coefficient associated with exit life stress, however, was negative. This together with its positive simple correlation with 'anxiety depression', indicated that it functioned as a suppressor variable.

Within the female sub-sample, two variables, namely undesirable life stress and exit life stress were entered into the regression equation within the tolerance limit of 0.05. The variance explained in 'anxiety depression' by these two variables was thirteen per cent. This compares to ten per cent explained by T.L.S. Examination of the beta coefficients indicated that undesirable life stress contributed fifty per cent more to the prediction of 'anxiety depression' than exit life stress. As with the total sample, exit life stress acted as a suppressor variable.

Within the male sub-sample, the life stress index concerned with desirable life stress entered the equation within the tolerance limit

TABLE 24 STEPWISE MULTIPLE REGRESSION ANALYSIS OF ANXIETY-DEPRESSION  
AND LIFE STRESS TYPE INDICES

TOTAL SAMPLE

VARIABLE IN EQUATION	MULTIPLE R	BETA	F
Undesirable Life Stress	0.21	0.21	18.97**
Exit Life Stress	0.26	-0.20	14.41**
<hr/>			
$R^2 = 0.07$ $F = 14.42$ $P < 0.00001$ $n = 408$			
<hr/>			

FEMALE SUB-SAMPLE

VARIABLE IN EQUATION	MULTIPLE R	BETA	F
Undesirable Life Stress	0.32	0.32	26.31**
Exit Life Stress	0.36	-0.21	16.84**
<hr/>			
$R^2 = 0.13$ $F = 16.94$ $P < 0.00001$ $n = 230$			
<hr/>			

MALE SUB-SAMPLE

VARIABLE IN EQUATION	MULTIPLE R	BETA	F
Desirable Life Stress	0.15	0.15	3.94#
<hr/>			
$R^2 = 0.02$ $F = 3.94$ $P < 0.05$ $n = 178$			
<hr/>			

\*  $P < 0.05$

\*\*  $P < 0.001$

of 0.05. It was argued above, however, that this correlation probably reflected the influence of chance on multiple significance tests and therefore, this result was left uninterpreted.

On considering these three analyses together, it appeared that these indices of specific types of stress added to the variance predicted by T.L.S. In the female sub-sample thirty per cent more variance was explained. In terms of predictive power, the undesirable life stress index was more important than the other five indices. This may support the theoretical view that negative change rather than change per se is of importance, however, it may also reflect the better distribution qualities of the undesirable life stress index. Wender (1967) indicated that rare events tend to have low predictive power, and, for example, although deaths may be important at the individual level they are sufficiently rare to diminish prediction in a group analysis.

#### SUMMATED LIFE EVENT RATINGS AND SYNDROMES OF DEPRESSION

Life event indices based on consensually derived normative weights of upset entailed by events have been criticised because of their lack of idiographic sensitivity (Brown, 1972). Indices based on consensually derived weights fail to take into account the particular circumstances of the event and the personal meaning that the event held for the person experiencing it.

The life event ratings of objective positive and negative impact,

subjective positive and negative impact and control over events (vide supra), used in the study, were designed to provide measures which were sensitive to the particular circumstances surrounding the events and the personal meaning which they held for the respondent.

It was noted above that these ratings tended to have comparatively moderate inter-rater reliabilities and for this reason they were not given primary importance in the analysis. Despite the modest reliability of these ratings, their relationships to the syndromes of depression were considered to determine whether measures designed to have idiographic sensitivity improved or qualified the relationships obtained above.

Indices were derived from the individual ratings by adding the ratings for each event experienced by each respondent. Thus, each respondent obtained scores on four indices which were named total objective positive impact, total objective negative impact, total subjective positive impact and total subjective negative impact. In addition, the summated ratings of degree of control were divided by the number of events experienced by the subject to give an index of the average control over events. This latter index was felt to be more easily interpretable than a total degree of control index. The distribution characteristics of these indices are tabulated in Table 25.

All of the distributions other than those for average control of event were positively skewed. Although these distributions were leptokurtic in form, they were not excessively so. All cross-sex comparisons of

TABLE 25 DISTRIBUTION OF SUMMATED LIFE EVENT RATINGS BY SEX

<u>VARIABLE</u>	<u>DISTRIBUTION CHARACTERISTICS</u>			
	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
<hr/>				
TOTAL OBJECTIVE NEGATIVE IMPACT				
Total Sample	7.20	5.86	0.99	1.46
Female Sub-Sample	7.13	5.97	1.08	1.77
Male Sub-Sample	7.40	5.74	0.89	1.09
<hr/>				
TOTAL OBJECTIVE POSITIVE IMPACT				
Total Sample	8.74	7.00	0.79	0.17
Female Sub-Sample	8.64	6.99	0.82	0.29
Male Sub-Sample	8.99	7.02	0.76	0.09
<hr/>				
TOTAL SUBJECTIVE NEGATIVE IMPACT				
Total Sample	7.20	5.71	0.81	0.51
Female Sub-Sample	7.06	5.73	0.78	0.20
Male Sub-Sample	7.38	5.69	0.85	0.95
<hr/>				
TOTAL SUBJECTIVE POSITIVE IMPACT				
Total Sample	8.84	7.24	0.95	0.74
Female Sub-Sample	8.70	7.19	0.89	0.61
Male Sub-Sample	9.15	7.31	1.00	0.93
<hr/>				
AVERAGE CONTROL OVER EVENTS				
Total Sample	3.44	1.80	-1.01	-0.45
Female Sub-Sample	3.43	1.82	-0.99	-0.51
Male Sub-Sample	3.45	1.78	-1.02	-0.36
<hr/>				

variance failed to reach significance (variance ratio for total objective impact,  $F = 1.17$ ,  $df = 229, 177$ , N.S.; total subjective positive impact,  $F = 1.02$ ,  $df = 177, 299$ , N.S.; total subjective positive impact,  $F = 1.02$ ,  $df = 177, 299$ , N.S.; total subjective negative impact,  $F = 1.07$ ,  $df = 177, 229$ , N.S.; average control over events,  $F = 1.09$ ,  $df = 229, 177$ , N.S.).

In addition, all cross sex differences in means failed to reach significance (pooled variance t-test, total objective positive impact,  $t = 0.51$ , N.S.; total objective negative impact,  $t = 0.47$ , N.S.; total subjective positive impact,  $t = 0.62$ , N.S.; total subjective negative impact,  $t = 0.57$ , N.S.; average control over events,  $t = 0.23$ , N.S.).

Pearson's Product Moment correlations were calculated for the relationships between the respondents' scores on each of the five indices and the four syndromes of depression. These correlation coefficients were calculated for the total sample, and the male and female sub-samples.

Eighty correlation coefficients, therefore, were calculated. Seventeen of these correlation coefficients were statistically significant at, or beyond, the five per cent level of significance. It would be expected that approximately four correlation coefficients would be significant at the five per cent level of significance, through the effects of chance alone. Consideration of Table 26 indicated that four correlations were significant at this level. Since they did not appear to fit an obvious pattern, these correlation

TABLE 26 PEARSON PRODUCT MOMENT CORRELATIONS OF SUMMATED LIFE EVENT RATINGS WITH THE FOUR SYNDROMES BY SEX

VARIABLE	SYNDROME			
	'ANXIETY DEPRESSION'	'COGNITIVE DEPRESSION'	'VEGETATIVE DEPRESSION'	'ENDOGENOUS DEPRESSION'
<hr/>				
TOTAL OBJECTIVE NEGATIVE IMPACT				
Total Sample	0.17***	0.01	-0.03	0.11*
Female Sub-Sample	0.21***	-0.03	-0.01	0.12
Male Sub-Sample	0.14	0.06	-0.05	0.09
<hr/>				
TOTAL OBJECTIVE POSITIVE IMPACT				
Total Sample	-0.22***	-0.03	-0.04	-0.05
Female Sub-Sample	-0.32***	-0.00	-0.05	-0.03
Male Sub-Sample	-0.08	-0.08	-0.04	-0.08
<hr/>				
TOTAL SUBJECTIVE NEGATIVE IMPACT				
Total Sample	0.17***	0.01	-0.04	0.11*
Female Sub-Sample	0.21**	-0.02	-0.03	0.13*
Male Sub-Sample	0.14	0.06	-0.05	0.09
<hr/>				
TOTAL SUBJECTIVE POSITIVE IMPACT				
Total Sample	-0.21**	-0.03	-0.04	-0.05
Female Sub-Sample	-0.31***	-0.00	-0.05	-0.04
Male Sub-Sample	-0.08	-0.08	-0.04	-0.09
<hr/>				
AVERAGE CONTROL OVER EVENTS				
Total Sample	0.13**	0.04	0.08	0.00
Female Sub-Sample	0.14#	0.01	0.09	-0.03
Male Sub-Sample	0.10	0.08	0.09	0.04
<hr/>				

\* P < 0.05 (two-tailed)

\*\* P < 0.01 (two-tailed)

\*\*\* P < 0.001 (two-tailed)



coefficients were not interpreted.

All the other significant correlation coefficients, that is, those significant at or beyond the 1 per cent or 0.1 per cent levels of statistical significance, were between the life event indices and 'anxiety-depression' alone. The life event indices did not correlate significantly with any of the other three syndromes of depression.

The relationship for all the indices, other than average control over events, conformed to the pattern observed with the T.L.S. index. Thus, the relationships were apparent in the total sample, with division of the sample in terms of sex, indicating that the relationships were significant only in the female sub-sample.

The relationship between 'anxiety-depression' and total objective negative impact and total subjective negative impact were, as might be expected, negative in form; that is, increased negative impact was related to increased symptomatology. The opposite was true for the indices of positive impact and indeed, these indices explained a greater amount of the variance than did the negative indices. For example, within the female sub-sample, the correlation between 'anxiety-depression' and total objective negative impact was 0.21, while that between 'anxiety-depression' and total objective positive impact was -0.32. Applying Hotelling's procedure for differences between correlated correlations (Guildford and Fruchter, 1973), the magnitude of these two correlations were found to be significantly different ( $t = 5.08$ ,  $P < 0.01$ ).

The difference in the two correlations may have been partly due to the difference in the variance of the two indices of life stress (test for difference of correlated variances,  $t = 5.04$ ,  $P < 0.01$ ). The correlation between total objective impact and 'anxiety-depression', corrected for range restriction, was 0.24. It appeared that restriction accounted for some of the difference in the magnitude of these relationships.

The average level of control over events correlated with 'anxiety-depression' in the total sample, less control being correlated with more symptoms. While significant, this relationship resulted in only 2 per cent of the variance being shared.

#### COMBINATIONS OF SUMMATED LIFE EVENT RATINGS AND ANXIETY-DEPRESSION

Multiple Regression Analysis, with 'anxiety-depression' as the dependent variable, was carried out for the total sample, the female sub-sample and for the male sub-sample.

A mixed model approach was used in the Multiple Regression Analysis (Cohen and Cohen, 1975). The analysis was designed to determine whether any of the summated life event ratings added to the variance predicted by the T.L.S. score.

The T.L.S. score was, therefore, forced into the regression equation on the first step with the summated life event ratings being entered

stepwise thereafter. This procedure was adopted to determine whether the measures designed to be idiographically sensitive increased the amount of variance predicted in 'anxiety-depression' over that predicted by T.L.S.

Within each of the three samples, 10 of the 21 inter-independent variable correlations were above 0.84. This level of multicollinearity was likely to adversely affect these analyses (Althauser, 1971). The results of these analyses, therefore, should be interpreted with this caveat in mind (See Table 27).

Within the total sample and the male sub-sample none of the summated life events ratings could be entered into the equation within the tolerance limit of 0.05. Within the female sub-sample, however, total objective negative impact contributed to the prediction of 'anxiety-depression' after the influence of T.L.S. had been taken into account. The predictive influence of this index was approximately half that of T.L.S. with the negative beta coefficients indicating that it functioned as a suppressor variable.

In summarising the relationships between the summated life event ratings and the syndromes of depression, it would appear that overall these idiographically sensitive measures of life events added little to the predictive power of normative weights. This general failure to improve the variance explained may in part have been due to the attenuative effects of the comparatively low reliability of these indices.

TABLE 27 MIXED MODEL MULTIPLE REGRESSION ANALYSIS OF ANXIETY-  
DEPRESSION WITH TOTAL LIFE STRESS AND SUMMATED LIFE EVENT RATINGS

TOTAL SAMPLE

VARIABLES IN EQUATION	MULTIPLE R	BETA	F
Total Life Stress	0.22	0.22	21.03**
$R^2 = 0.05$ $F = 21.03$ $P < 0.00001$ $n = 408$			

FEMALE SUB-SAMPLE

VARIABLES IN EQUATION	MULTIPLE R	BETA	F
Total Life Stress	0.32	0.57	24.33**
Total Objective Negative Impact	0.35	-0.29	4.82*
$R^2 = 0.12$ $F = 15.71$ $P < 0.00001$ $n = 230$			

MALE SUB-SAMPLE

VARIATIONS IN EQUATION	MULTIPLE R	BETA	F
Total Life Stress	0.06	0.06	0.75
$R^2 = 0.00$ $F = 0.75$ N.S. $n = 178$			

\*  $P < 0.05$

\*\*  $P < 0.001$

These analyses tend to draw attention to the pattern of relationships between the summated ratings and 'anxiety-depression' alone amongst the four syndromes of depression. While this pattern tends to support the results produced by the T.L.S. index, this should not be regarded as providing evidence of external validity or generalisation amongst measures. This is because all the indices were based on, and, perforce, highly correlated with, the number of events experienced by the respondent.

#### SUMMARY

The life events reported were sub-divided into contrasting types. Indices designed to measure exit and entrance stress, desirable and undesirable stress and death exit and non-death exit stress were developed. The relationships among these six indices and each of the syndromes were examined. Undesirable stress correlated with 'anxiety-depression' in the same manner as the T.L.S. index. Entrance life stress formed a counter-intuitive relationship with 'endogenous depression' in the total sample and the male sub-sample. It was argued that part of this relationship could be attributed to the effects of respondents' age.

Stepwise Multiple Regression Analysis was carried out to determine firstly, whether these indices could increase the variance explained in 'anxiety-depression' and, secondly, whether particular types of life stress had particular predictive power. Within the total sample and the female sub-sample the combination of undesirable life stress

and exit life stress increased the variance explained in 'anxiety-depression'. In general, undesirable life stress was the most important of these six variables. This may reflect either the importance of negative change rather than change per se, or the better distribution characteristics of this index.

Five indices based on the individual life event ratings were derived. Their relationships with each of the four syndromes were estimated.

## CHAPTER 8 THE INFLUENCE OF ADDITIONAL VARIABLES

The simple relationships between several life event indices and individual dependent variables have been considered. It was argued in detail above, that the influence that additional variables have on this simple event-syndrome link was of considerable theoretical interest and potentially of practical value. In brief, it was argued that additional variables should be considered because they may (a) increase the variance explained in the dependent variable, (b) increase understanding of the causal mechanisms involved, and (c) indicate possible treatment strategies. The influence of three types of additional variable were considered; firstly, demographic and personality variables, secondly, Brown's 'Vulnerability Factors' and thirdly, social adjustment variables. Within this chapter the general approach to the description of additional variables will be outlined followed by the results pertaining to demographic and personality variables.

### GENERAL APPROACH TO THE DESCRIPTION OF ADDITIONAL VARIABLES

The influence of additional variables was examined with the procedure for specifying non-additive models that was described in detail above.

The general model, which was estimated for each additional variable, can be expressed by the following equation.

$$D = a + b_1 A + b_2 L + b_3 A L + e$$

where D = 'anxiety-depression' score

A = Additional variable

L = Total Life Stress (T.L.S.)

e = Random error

and all other symbols are parameters.

Hierarchical Regression Analysis was used. Following Cohen and Cohen (1975), the additional variable generally was entered into the equation before the T.L.S. score because it was assumed to be causally prior to the experiences of life events. The demographic factors were thought to be clearly causally prior in that they ante-dated the experience of life events. A similar assumption was made regarding each of the personality variables. This assumption was based on less firm grounds, with the personality variables being thought to measure stable tendencies that were only slightly reactive to external circumstances. Eysenck (1967) argued in detail that these personality variables have a substantial biological basis.

The total life score generally was entered into the equation after the additional variable. The interaction was entered into the equation last because of the structural requirement of the model. Cohen and Cohen (1975) and Cohen (1978) have indicated that an interaction can only be adequately represented by the product of the first order effects, when the first order effects have been partialled from the product term. This structural constraint requires the two first order effects to be entered before the product term.

The statistical significance of the overall model was determined using



the F statistic. This test indicated whether the multiple correlation (R) departed significantly from zero. This was equivalent to determining whether any of the parameters departed significantly from zero (Nie et al, 1975).

The statistical significance of the individual parameters of the model i.e.  $b_1$ ,  $b_2$ ,  $b_3$  was determined through the use of hierarchical F tests. These tests indicated whether or not the independent variables contributed to the prediction of the dependent variable, with different patterns of significant parameters being predicted under each of the four hypotheses, i.e. independent causes hypothesis, mutual potentiation hypothesis, vulnerability hypothesis, synergism hypothesis (vide supra). The pattern of significant parameters, as judged by the hierarchical F tests varies to some extent with the order of entry of the first order effects. Where no causal priority dictated the order of entry of the first order effects, both orders were used to confirm the pattern.

Although the order of entry did affect the pattern or significance of the parameters it did not affect their magnitude. The parameters of the model, or standardised partial regression coefficients (Beta coefficients), were unaffected by the order of entry of the first order effect because they indicate the magnitude of effect of the particular variable given the other variables in the equation.

Following the Hierarchical Regression Analysis, the relative magnitude of the causal impact of each of the independent variables was calculated using partial derivatives as outlined by Stolzenberg (1980)

(Vide Supra). This procedure allowed the estimation of effect magnitudes when non-additivity was present in the general model, i.e. when  $b_3$  was significant (Table 2). In the absence of non-additivity the relative magnitude of causal impact was assessed by the examination of the Beta coefficients.

The influence of age, social class, Neuroticism and Extraversion on the simple T.L.S. 'anxiety-depression' link was determined for the total sample and for the male and female sub-samples. The influence of sex was determined for the total sample. The analyses were carried out in the male sub-sample, despite the absence of a significant correlation between T.L.S. and 'anxiety-depression', because of the possibility that the additional variable might reveal event-syndrome links which might otherwise be obscured (Tennent et al, 1981).

#### THE INFLUENCE OF DEMOGRAPHIC AND PERSONALITY VARIABLES

The general model was estimated on the total sample for each of the five additional variables and the results obtained were tabulated in Table 28. In each case the overall F test was significant indicating that the multiple correlations departed significantly from zero. When the respondents' sex was entered as an additional variable the standardised coefficient for T.L.S. was 0.19 and for the respondents' sex was 0.22. The standardised effect coefficient for respondents' sex was within the confidence interval for T.L.S. Thus, despite the presence of the non-additive relationship, it was possible to demonstrate that each of the variables contributed to the prediction of 'anxiety-depression', to an essentially similar degree.

TABLE 28 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
TOTAL SAMPLE n = 408

ADDITIONAL VARIABLE					TOTAL LIFE STRESS					INTERACTION						
VARIABLE	MEAN	S.D.	F1	BETA	5% CONFID LIMITS		MEAN	S.D.	F	BETA	5% CONFID LIMITS	F	R <sup>2</sup>	F	MODEL	
Sex	0.56	0.49	15.84	0.02	0.22 ± 0.01		25.56	20.72	38.46	0.06	0.19 ± 0.19	8.59	0.28	0.10	15.65	Synergy
Age	49.62	17.30	20.38	-0.16	± 0.16		25.56	20.72	36.23	0.24	± 0.21	0.22	-0.05	0.08	12.15	Independent causes
Social Class	3.60	1.35	0.64	-0.04	± 0.15		25.56	20.72	21.19	0.19	± 0.26	0.05	0.03	0.05	7.11	No effect
Neuroticism	9.84	6.11	171.95	0.46	± 0.13		25.56	20.72	16.34	0.10	± 0.15	1.24	0.10	0.32	63.31	Independent causes
Extra-version	9.41	6.08	0.77	-0.07	± 0.18		25.56	20.72	22.25	0.23	± 0.18	0.00	0.00	0.05	7.75	No effect

\* P < 0.05    \*\* P < 0.01    \*\*\* P < 0.001    \*\*\*\* P < 0.0001

In summary, the influence of the additional variable respondents' sex, on 'anxiety-depression' increased the variance explained. The influence of respondents' sex appeared to be of a similar magnitude to the influence of T.L.S. The presence of a synergistic effect, however, implied that the respondents' sex was not a simple risk factor because under adversity females had a disproportionately higher risk of developing increased levels of 'anxiety-depression'.

Ten per cent of the variance in 'anxiety-depression' was predicted by the three independent variables. All three hierarchical F tests reached significance indicating that each of the three variables contributed significantly to the production of 'anxiety-depression'. This pattern of hierarchical F tests was consistent with the synergy hypothesis. Therefore, as was noted above, the additional variable and life stress had independent influences on psychological distress, but in addition, their concatenation produced a disproportionate increase in the level of psychological distress.

It appeared that both the respondents' sex and T.L.S. contributed to the level of 'anxiety-depression'. In addition to these independent effects, however, they interacted so that the effect of T.L.S. was disproportionately strong for women. Thus, when T.L.S. was elevated women reported substantially higher levels of 'anxiety-depression'.

Examination of the parameters associated with each of the variables in the model, highlighted the difficulty of interpretation that arose when a significant degree of non-additivity was present. The beta

coefficients associated with both sex and T.L.S. were fairly small, while that associated with their interaction was comparatively large. Using the procedure of partial derivatives it was possible to evaluate the relative importance of respondents' sex and T.L.S. as predictors of 'anxiety-depression'. The partial derivatives summarised the rate of change in 'anxiety-depression' due to the combination of the main effect and the non-additive effect of each of the variables. The calculation of these partial derivatives and their associated confidence intervals is illustrated below (See Table 29).

When the general model was estimated with age as the additional variable, it explained eight per cent of the variance in 'anxiety-depression' scores. Examination of the hierarchical F tests associated with the first order effects and the interaction term indicated that this model was consistent with the independent causes hypothesis. Therefore, both T.L.S. and age influenced the level of 'anxiety-depression' detected, with the influence of T.L.S. being independent of the respondents' age. Consideration of the beta coefficients indicated that the influence of the respondents' age was slightly less than half of the influence of T.L.S. The relationship between age and 'anxiety-depression' was negative in direction, that is, the level of reported 'anxiety depression' declined with age. The absence of a significant non-additive effect excluded the need to calculate the partial derivatives because the beta coefficients were equivalent for this type of model.

Similarly, when the general model was estimated with the respondents'

TABLE 29 HIERARCHICAL REGRESSION ANALYSIS OF 'ANXIETY DEPRESSION'  
WITH TOTAL LIFE STRESS (T.L.S.), RESPONDENTS' SEX AND THEIR  
INTERACTION (TOTAL SAMPLE n = 408)

VARIABLE	R <sup>2</sup>	R <sup>2</sup> CHANGE	B	BETA	F	PROB
Sex	0.04	0.04	0.03	0.02	15.84	0.001
TLS	0.09	0.05	0.01	0.06	38.46	0.001
Sex x TLS	0.10	0.02	0.01	0.28	8.59	0.001

CALCULATING THE STANDARDISED EFFECT COEFFICIENTS FOR SEX AND TLS

VARIABLE	MEAN	STANDARD DEVIATION (σ)
Sex (A)	0.56	0.49
TLS	25.56	20.72

UNDER THE SYNERGISM HYPOTHESIS

$$\partial D / \partial L = (b_2 + b_3 A) \cdot \frac{\sigma_L}{\sigma_D}$$

$$\partial D / \partial L = (b_1 + b_3 L) \cdot \frac{\sigma_A}{\sigma_D}$$

$$\partial D / \partial L = 0.22 \text{ and } \partial D / \partial A = 0.19$$

Neuroticism score as the additional variable, it was consistent with the independent causes hypotheses. Thus, both Neuroticism and T.L.S. influenced the level of 'anxiety-depression' with the impact of T.L.S. being unaffected to any significant degree by the respondents' level of Neuroticism.

In sharp contrast to the model estimated with age, this model explained 32 per cent of the variance in 'anxiety-depression'. Examination of the beta coefficients of this model indicated that the influence of the respondents' Neuroticism score in predicting 'anxiety-depression' was four and a half times that of their T.L.S. score.

When the general model was estimated with either the respondents' social class or their Extraversion score as the additional variable, the overall F statistics were significant. However, the hierarchical F statistics associated with either the additional variable or their interactions with T.L.S. were not significant. In both cases it appeared that these additional variables did not have any impact in a direct or an indirect fashion on 'anxiety-depression'. These models were, therefore, not relevant to the four hypotheses detailed above.

The general model, with each of the appropriate variables, was then recalculated with the data from the female sub-sample. The overall pattern of the results was identical to that obtained with the total sample (See Table 30). In all cases the overall F statistic was significant and the models estimated with age and Neuroticism as the additional variables were consistent with the independent causes

TABLE 30. HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
FEMALE SUB-SAMPLE  $n = 230$

ADDITIONAL VARIABLE					TOTAL LIFE STRESS					INTERACTION					
VARIABLE	MEAN	S.D.	F1	5% CONFID LIMITS	BETA	5% CONFID LIMITS	MEAN	S.D.	F1	5% CONFID LIMITS	BETA	R <sup>2</sup>	F	MODEL	
Age	50.60	17.10	10.22	-0.14	±0.20	25.49	21.24	21.56	0.31	±0.28	0.01	-0.01	0.15	10.59	**** Independent causes
Social Class	3.51	1.33	0.00	-0.03	---	25.49	21.24	25.93	0.91	---	0.76	0.16	0.11	8.89	**** One cause
Neuroticism	11.00	6.17	100.28	0.50	±0.18	25.49	21.24	22.56	0.24	±0.21	0.01	0.02	0.35	40.93	**** Independent causes
Extra-version	9.89	6.14	1.14	-0.11	±0.19	25.49	21.24	26.84	0.31	±0.22	0.39	0.03	0.11	9.34	**** One cause

\*  $P < 0.05$     \*\*  $P < 0.01$     \*\*\*  $P < 0.001$     \*\*\*\*  $P < 0.0001$



hypothesis. The models, estimated with social class and Extraversion as the additional variables, however, showed no direct or indirect effect of the additional variables. In every case the variance explained by the models in the female sub-sample was higher than for the total sample, implying that a better fit had been achieved.

The general model was then re-estimated, for each of the additional variables, using the data from the male sub-sample (See Table 31). The overall F statistics associated with the 'age model' and the 'Neuroticism model' were significant while those associated with social class and Extraversion were non-significant. Examination of the hierarchical F tests associated with T.L.S. or its interaction with age and Neuroticism indicated that they were non-significant. This implied that T.L.S. had neither a direct nor an indirect effect on 'anxiety-depression'. It appeared, therefore, that within the male sub-sample both age and Neuroticism affected 'anxiety-depression' but they did not influence the impact of life events.

The analyses of these demographic factors and personality variables within the total sample and the two sex sub-samples indicated that only one additional variable, respondents' sex, influenced the impact of T.L.S with respect to the respondents' 'anxiety-depression' scores. Two variables, namely age and Neuroticism, aided the prediction of 'anxiety-depression' in their own right, but did not influence the impact of T.L.S. Finally, two variables, namely social class and extraversion, did not contribute in a statistically significant way to the prediction of 'anxiety-depression'.

TABLE 31 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
MALE SUB-SAMPLE  $n = 178$

ADDITIONAL VARIABLE				TOTAL LIFE STRESS				INTERACTION							
VARIABLE	MEAN	S.D.	F1	BETA	5% CONFID LIMITS	MEAN	S.D.	F	BETA	5% CONFID LIMITS	F	R <sup>2</sup>	F	MODEL	
Age	48.35	17.55	16.46	***	-0.24 ± 0.24	26.03	20.06	1.35	0.09	± 0.30	0.19	-0.08	0.09	5.55	** One cause
Social Class	3.71	1.36	---	---	---	26.03	20.06	---	---	---	---	---	0.01	0.67	No effect
Neurot- icism	8.35	5.72	56.23	***	0.50 ± 0.22	26.03	20.06	0.05	0.02	± 0.24	0.00	-0.01	0.24	18.76	**** One cause
Extra- version	9.41	6.08	---	---	---	26.03	20.06	---	---	---	---	---	0.01	0.46	No effect

\*  $P < 0.05$     \*\*  $P < 0.01$     \*\*\*  $P < 0.001$     \*\*\*\*  $P < 0.0001$

## SUMMARY

The procedure for specifying non-additive models was described briefly. The influence of demographic and personality variables on the simple T.L.S. - 'anxiety-depression' link was assessed.

## CHAPTER 9 THE INFLUENCE OF BROWN'S VULNERABILITY FACTORS ON THE LINK BETWEEN TOTAL LIFE STRESS AND 'ANXIETY-DEPRESSION'.

Brown et al (1975) and Brown and Harris (1978a) argued that four factors act to make women particularly vulnerable to the impact of life stress. These four vulnerability factors, namely, loss of mother before eleven, lack of employment outwith the home, lack of a confiding relationship and the presence of three or more children at home were identified, in a post-hoc manner, by Brown et al (1975). The empirical support for these four factors was considered in considerable detail above and thus will not be repeated here. The general conclusion reached, from the examination of previous studies, was that the case was non-proven.

### THE MEASUREMENT OF VULNERABILITY FACTORS

The current study was designed following the publication of Brown et al (1975) and a specific prediction was made that these four factors would influence the link between T.L.S. and 'anxiety-depression' in the manner of the vulnerability hypothesis. These four variables, therefore, had a higher theoretical status than the demographic variables that were considered above or the social adjustment variables that are considered below.

The design of the study and the analysis of the results obtained did not represent an attempt to replicate the results of Brown et al (1975) and Brown and Harris (1978a) in an absolute or complete fashion. The variables used in this study were designed to measure

the same constructs as Brown and his colleagues, however, they were quite different in operational form. In addition, the sample under study was probably quite different from the Camberwell samples, particularly in terms of class structure.

To attempt absolute replication was probably not possible and indeed, was probably not desirable. Rather, the purpose of this study was considered to be an attempt to assess the external validity of the vulnerability model. Susser (1973) noted that it is important to demonstrate a constant or reproducible association between the constructs of interest under varying conditions. Cook and Campbell (1979), in their discussion of external validity, indicated that the varying conditions entailed varying condition of both sampling and of measures.

The discussion of the following analysis must be considered in the light of the caveat that the measures used were different from those used by Brown and his colleagues. Both the independent and dependent variables, although conceptually related, were operationally very different.

The general model was estimated with each of the following seven variables as the additional variable.

- (1) Loss of, or separation from mother before the age of eleven.
- (2) Loss of, or separation from mother before the age of fifteen.
- (3) Number of children under fifteen years living at home.
- (4) Lack of full-time employment outwith the home.
- (5) Extent of confiding in spouse.

(6) Number of confiding relationships available.

(7) Total number of vulnerability factors.

The first two variables, which measured loss of, or separation from mother, entailed loss together with separation for four months or longer. Reasons for separation, which were considered, were the presence of either the respondent or their mother in hospital, the divorce or separation of the respondent's parents, the death of the respondent's mother and 'other' reasons. 'Other' reasons included the respondent being fostered or having been sent to live with relations.

The extent of confiding in spouse variable was measured using the five point scale item of the Social Adjustment Scale (S.A.S., Paykel et al, 1971b) that related to reticence with regard to the respondents' spouse. The level of inter-rater agreement, corrected for chance agreement, of this variable was within the satisfactory range (pair one, Kappa = 0.82; pair two, Kappa = 0.70) (See below for detailed account of reliability estimates for all S.A.S. items).

The sixth variable, which assessed the number of confiding relationships available to each respondent, was assessed by considering the three S.A.S. items pertaining to reticence with regard to spouse, relatives and friends. The number of items on which a scale score of one was rated was calculated to give the index of the number of confiding relationships available. This method of calculating the number of confidants available was far from ideal as the quality or intensity of confiding required to achieve a 'one' rating varied across the relationships considered. Despite this

considerable drawback, it was felt that some measure, albeit an unsatisfactory measure, of the availability of close relationships was required.

The seventh and final variable, the total number of vulnerability factors, was calculated using a procedure derived from the work of Brown and Prudo (1981). Each respondent was given a score of one on this variable for each of the following factors, having three or more children at home, not having full-time employment outwith the home, having lost, or been separated from their mother before the age of eleven, and not having a 'one' rating on the S.A.S. item pertaining to confiding with their spouse. The variable was calculated by adding each of these item scores together thus giving a possible range of scores of zero to four.

Hierarchical Regression Analysis, identical in form to that described above, was used. The three additional variables relating to loss of mother and the number of children at home were entered into the general model before the T.L.S. score. This was done because these additional variables were assumed to be causally prior in that they ante-dated the experience of life events. The other four additional variables were entered into the model in both orders so that the pattern of significant parameters could be confirmed.

The general model, with each of these seven variables as the additional variables, was calculated for each of the following three sub-samples:

- (1) All women.
- (2) All women under sixty-five years.
- (3) All married women under sixty-five years with one or more children under fifteen years of age.

The models were estimated for each of these samples because of the following considerations. Firstly, men were excluded and women alone considered because Brown et al's (1975) sample was of women only. Secondly, the models were estimated for all the women who were aged under sixty-five years because Brown and Harris (1978a) indicated that the vulnerability factors operated for women in this age group. Their sample did not include women over the age of sixty-five years. While there was some evidence that vulnerability factors may operate in those women over sixty five years (Murphy, 1982), it was thought that comparison might be facilitated by making the age range the same as in Brown and Harris' sample. Thirdly, the model was estimated for married women under sixty-five years, with one or more child under fifteen years, because Brown et al (1975) contended that all the vulnerability factors, except for the lack of a confiding relationship, operated only for women with children. It was thought that the examination of this sub-sample alone might facilitate the replication of the finding of Brown et al (1975). It should perhaps be noted in passing, however, that the results of Brown et al (1975), pertaining to women with children alone, were generalised by Brown and Harris (1978a) to all women.



## VULNERABILITY FACTORS IN THE FEMALE SUB-SAMPLE

### Loss of Mother Before the Age of Eleven

Twenty six of the 230 female respondents reported having experienced separation from their mother, at or before the age of eleven, as a consequence of all the circumstances that were considered (See table 32).

This sub-group represented approximately eleven per cent of the female respondents. Wender (1967) indicated that the predictive value of rare events was small. Maxwell (1972) similarly indicated that correlational analysis, in which the proportions of dichotomous values depart substantially from the 20% - 80% range, could be distorted.

Given the comparative rarity of separation from, or loss of, mother before the age of eleven, the decision was made that the variable should not be further sub-divided into different reasons for loss or separation. The analysis of the effects of loss of mother because of death on the link between T.L.S. and 'anxiety-depression' would probably have provided distorted results as only 5% of the sub-sample had experience this.

The general model was estimated, therefore, with a loss of, or separation from mother before the age of eleven due to all causes acting as the additional variable (See Table 33).

The overall F test was statistically significant indicating that the

TABLE 32 FREQUENCY OF CAUSE OF EARLY SEPERATION FROM MOTHER BY AGE AT  
EARLY SEPARATION FROM MOTHER - ALL WOMEN    n = 230

CAUSE	AGE (YEARS)															TOTAL	
	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14		15
Respondent in Hospital	1							1				1			1		4
Mother in Hospital							1								1		2
Divorce/ Separation												1			1		2
Death of Mother		1	3		1	1	1	1			1	2	2	2	4	2	21
Other	1	1	1			1			1		2	3	1			3	14
TOTAL	2	2	4		1	2	2	2	1		3	7	3	2	7	5	43

TABLE 33 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
FEMALE SUB-SAMPLE  $n = 230$

ADDITIONAL VARIABLE		TOTAL LIFE STRESS						INTERACTION										
		CONFID				5% CONFID		LIMITS										
VARIABLE	MEAN	S.D.	F1	F2	BETA	LIMITS	MEAN	S.D.	F1	F2	BETA	R <sup>2</sup>	F	MODEL				
Loss of mother before 11 years	0.11	0.32	1.55	-	0.13	-	25.49	21.24	***	25.24	-	0.34	0.93	-	-0.09	0.11	9.23	One cause
Loss of mother before 15 years	0.19	0.39	1.99	-	0.08	-	25.49	21.24	***	25.16	-	0.32	0.01	-	0.00	0.11	9.00	One cause
No of Children under 15 years	0.62	1.09	3.07	-	0.04	-	25.49	21.24	***	25.73	-	0.29	0.78	-	2.10	0.12	9.77	One cause
Employment outwith home	0.55	0.50	0.51	0.30	0.12	-	25.49	21.24	***	26.00	25.75	0.40	1.22	1.22	-0.13	0.11	9.06	One cause
Degree of confiding in spouse	1.36	0.87	3.64	1.77	0.17	-	28.08	22.39	***	12.60	14.48	0.36	0.22	0.23	-0.10	0.11	5.50	One cause
No of confidants	1.67	0.89	3.47	3.73	-0.11	-	25.49	21.24	***	26.31	26.05	0.35	0.05	0.05	-0.03	0.12	9.89	One cause
Total No vulnerability factors	0.87	0.79	5.37	3.62	0.18	0.19	25.49	21.24	***	24.30	26.04	0.36	0.19	0.59	-0.09	0.12	10.06	Independent causes

\*  $P < 0.05$  \*\*  $P < 0.01$  \*\*\*  $P < 0.001$  \*\*\*\*  $P < 0.0001$

multiple correlation was significantly different from zero. Eleven per cent of the variance in 'anxiety-depression' was explained by the general model. One of the hierarchical F tests, that pertaining to T.L.S., was statistically significant. Loss of, or separation from, mother before the age of eleven had neither a direct effect on 'anxiety-depression', nor an indirect effect as a modifier of T.L.S. This model did not conform, therefore, to any of the four hypotheses described above. The beta coefficients were thus left uninterpreted.

#### Loss of Mother Before the Age of Fifteen

Forty three, or nineteen per cent of the 230 female respondents had experienced separation from, or loss of their mother, for four months or more before the age of fifteen, as a consequence of all the circumstances that were considered (See Table 32).

The variable was not sub-divided into reasons for loss or separation because of the reasons given immediately above. It was entered into the general model and the overall F test was significant, indicating that the multiple correlation departed significantly from zero. The general model explained eleven per cent of the variance in 'anxiety-depression'. Only the hierarchical F test that related to T.L.S. attained statistical significance. It appeared, therefore, that loss of, or separation from, mother before the age of fifteen had neither a direct influence on 'anxiety-depression', nor an indirect modifying influence through T.L.S. The parameters of this model, therefore, were not interpreted.

#### Number of Children Under Fifteen Years Living at Home

The mean number of children under the age of fifteen, in this comparatively old sample, was 0.62 (SD = 1.09). This variable was not dichotomised, in the manner of Brown and Harris (1978a), into respondents with less than three children and respondents with three or more children, because of the loss of information and power that results from dichotomisation (Cohen and Cohen, 1975). The results obtained when the general model was estimated were tabulated in Table 33.

The overall F test was significant with the general model explaining twelve per cent of the variance in 'anxiety-depression'. Only one of the three hierarchical F tests, that associated with T.L.S., was significant at or beyond the conventional levels of statistical significance. The hierarchical F test associated with the number of children, however, approached significance beyond the 0.1% level. Even if this relationship had reached conventional levels of statistical significance, the pattern of parameters would tend to suggest a direct effect and not the non-additive effect that the vulnerability model demands.

#### Lack of Full-Time Employment Outwith the Home

One hundred and twenty six, or fifty five per cent, of the female respondents lacked full-time employment outwith the home. This variable differed from that used by Brown and Harris (1978a) in that they considered both part-time and full-time employment.

When the general model was estimated with lack of full-time employment outwith the home as the additional variable, the overall F test reached statistical significance (See Table 33). The general model explained eleven per cent of the variance in 'anxiety-depression'. Only one hierarchical F test, that pertaining to T.L.S., reached significance. This was the case irrespective of the order of entry of the additional variable. It appeared, therefore, that the lack of full-time employment outwith the home had neither a direct nor an indirect impact on the prediction of 'anxiety-depression'. The beta coefficients were, therefore, not interpreted.

#### Extent of Confiding in Spouse

Brown et al (1975), Brown and Harris (1978a) considered the influence that the presence of a confiding relationship had on the link between life events and depression. In their studies the respondent could be rated as having a confiding relationship with anyone, although in actuality these relationships were usually with husbands or boyfriends. The presence of a confiding relationship was the vulnerability factor with the highest external validity (vide supra) although it was also the 'softest' measure.

In this initial analysis the effect of extent of confiding in spouse was considered with the number of confiding relationships available being considered below. One hundred and thirty of the female respondents were married. This analysis applied to this sub-group of female respondents. The mean level of confiding was 1.36 (SD = 0.87).

Estimation of the general model with the extent of confiding in spouse

as the additional variable resulted in a significant overall F statistic (See Table 33). The general model explained eleven per cent of the variance in 'anxiety-depression'. Irrespective of the order of the entry of the first order effects, only those hierarchical F tests that were associated with T.L.S. reached statistical significance. When the additional variable was entered into the model on the first step, however, the hierarchical F test just failed to reach statistical significance at the 5% level of significance ( $F = 3.64$ , N.S.). If this hierarchical F statistic had achieved the conventional level of significance, the pattern of parameters would have been consistent with the independent causes model and not the vulnerability model. Given the lack of significance, however, it appeared that the additional variable had neither a direct nor an indirect modifying effect through T.L.S. on the prediction of 'anxiety-depression'. The beta coefficients, therefore, were left uninterpreted.

#### Number of Confiding Relationships Available

The mean number of confidants reported by the total female sub-sample was 1.67 (SD = 0.89). Eighty eight per cent of the female respondents were rated as having one or more confiding relationships. This compared with 63 per cent of the female respondents in the Camberwell sample who were rated as having a confiding relationship (Brown and Harris, 1978a; p 176). This difference probably reflected the difference in the criteria used in assessing the presence of a confiding relationship, with Brown and his colleagues applying far more demanding criteria. Use of this current, less rigorous index was justified, however, by the findings of Miller and Ingham (1976), that

friends, confidants and others influenced the level of symptoms reported by subjects under stress.

When the general model was estimated with the number of confidants as the additional variable, the overall F statistic was significant (See Table 33). The general model explained 12 per cent of the variance in 'anxiety-depression'. Irrespective of the order of entry of the first order effects, only those hierarchical F tests associated with T.L.S. reached the conventional level of statistical significance. The hierarchical F tests associated with the number of confidants was significant beyond the 10 per cent level of significance, and just failed to reach the 5 per cent level ( $F_1 = 3.45$ ,  $F_2 = 3.73$ , NS). If these hierarchical F tests had achieved statistical significance, the pattern of parameters would have been consistent with the independent causes hypothesis and not the vulnerability hypothesis. The beta coefficients were not interpreted.

#### Total Number of Vulnerability Factors

The summated vulnerability index, which measured the number of Brown's vulnerability factors which each female respondent reported, had a range of 0 to 4 and a mean value of 0.87 (SD = 0.79). The overall F test for this general model was significant with the multiple correlation explaining 12 per cent of the variance in 'anxiety-depression' (See Table 33).



The hierarchical F test associated with the T.L.S. scores was significant irrespective of the order of entry of the first order effects. When the additional variable was entered into the regression equation on the first step this contribution reached significance at the 5 per cent level of significance. However, when it was entered on the second step, it just failed to reach significance at the 5 per cent level ( $F_1 = 5.37$ ,  $F_2 = 3.62$ ). In neither case did the hierarchical F statistic associated with the interaction achieve or approach statistical significance. The appropriate order of entry of this variable was difficult to determine because two of the component items, namely, loss of mother before eleven and the number of young children at home, clearly ante-dated the experience of life events. The other two component items, however, lack of employment outwith the home and absence of a confiding relationship with spouse, may have either ante-dated or post-dated the experience of life events. On balance, it was considered that the first order of entry was most important, because, while employment status for the level of confiding might be affected by the experience of life events in some individuals, these factors are likely to be comparatively stable for the majority of individuals. Indeed, only 2 per cent of the female respondents had been made unemployed in the previous 12 months, that is 98 per cent of the female respondents' employment status was stable during the period covered by the life event interview. Further, Murphy (1982) presented evidence that supported the view that the absence of a confiding relationship was often a longstanding feature reflecting longstanding personality characteristics.

When the first order of entry was considered, the general model was not consistent with the vulnerability model, but rather it was consistent with the independent causes model. Similarly, when the second order of entry was considered, in which the additional variable approached, but did not achieve significance, the general model tended to support the independent causes model rather than the vulnerability model. Examination of the beta coefficients of the model indicated that the contribution of the vulnerability index to the prediction of 'anxiety-depression' was exactly half the contribution of T.L.S.

In summary, when all the female respondents were considered, the only additional variable that contributed to the prediction of 'anxiety-depression' was the total number of vulnerability factors. The number of young children at home, the extent of confiding with spouse and the number of confidants available approached but did not achieve a statistically significant contribution. In all cases, whether significance was achieved or merely approached, the general models were consistent with the independent causes hypothesis and not the vulnerability hypothesis. There was no evidence of non-additivity or the effects of T.L.S. being modified by any of the additional variables.

#### VULNERABILITY FACTORS IN WOMEN UNDER 65 YEARS OF AGE

Seven additional variables were again considered with the females aged over 65 years of age being excluded. These analyses were carried out, because as was noted above, the samples used by Brown and his colleagues similarly excluded women over 65 years of age. Failure to replicate the vulnerability hypothesis above may have been due to the

different age structure of the sample, therefore, the following analyses were performed. The caveats that applied to the analysis of the total female sub-sample applied equally in this set of analyses, however, because of the danger of redundancy, they will not be repeated here.

Examination of Table 34 revealed that the results obtained with this sub-sample were essentially similar to those achieved within the total female sub-sample. In general terms, the overall F statistics were all significant indicating that the multiple correlations were significantly different from zero in every case. When the effects of the additional variables were considered, it was shown that neither of the early loss variables, or the number of children under the age of 15, nor the lack of full-time employment outwith the home influenced the prediction of 'anxiety-depression' to a significant degree.

When the extent of confiding with spouse was considered as an additional variable it appeared that irrespective of the order of entry of the first order effects, only those hierarchical F tests associated with T.L.S. achieved statistical significance at the conventional level. When the additional variable was entered into the model on the first step, however, the hierarchical F test was significant at the 10 per cent level of statistical significance but failed to reach the 5 per cent level ( $F = 3.14$ , NS). This result was essentially similar to that obtained with the total female sub-sample. If this hierarchical F statistic had achieved statistical significance, the pattern of parameters would have been consistent

TABLE 34 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
ALL WOMEN UNDER 65 YEARS OF AGE  $n = 177$

ADDITIONAL VARIABLE				TOTAL LIFE STRESS					INTERACTION									
VARIABLE	MEAN	S.D.	F1	F2	BETA	5% CONFID LIMITS	MEAN	S.D.	F1	F2	BETA	R <sup>2</sup>	F	MODEL				
Loss of mother before age 11	0.13	0.34	0.08	-	0.00	-	27.38	21.33	22.05	***	0.34	-	0.00	0.11	7.41	**** One cause		
Loss of mother before age 15	0.19	0.42	2.18	-	0.02	-	27.38	21.33	22.17	***	0.30	-	0.07	0.10	0.13	8.31	**** One cause	
No of children under age 15	0.80	1.19	1.37	-	0.02	-	27.38	21.33	23.74	***	0.30	-	0.79	0.11	0.13	8.41	**** One cause	
Employment outwith home	0.44	0.50	1.75	1.61	0.14	-	27.38	21.33	22.24	22.37	0.37	-	0.27	0.27	0.12	8.12	**** One cause	
Degree of confiding in spouse	1.39	0.89	3.14	1.39	0.16	-	28.02	22.40	11.75	13.47	0.36	-	0.23	0.23	-0.11	0.11	5.04	** One cause
No of confid- ants	1.77	0.86	4.75	3.28	-0.07	0.23	27.38	21.33	21.16	22.63	0.41	0.30	0.35	0.36	-0.10	0.13	8.78	**** Independent causes
Total No Vulnerability factors	0.84	0.87	5.35	3.02	0.13	0.22	27.38	21.33	20.30	22.63	0.33	0.20	0.00	0.00	0.00	0.13	8.54	**** Independent causes

\*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*  $p < 0.001$  \*\*\*\*  $p < 0.0001$

with the independent causes hypothesis and not the vulnerability hypothesis. Given the absence of statistical significance, the parameters of the model were left uninterpreted.

When the influence of the number of confiding relationships available was determined in the total female sub-sample it appeared to approach, but not achieve, a significant effect. The result achieved with women under 65 years of age was slightly different. When the number of confidants available was entered into the model on the first step the hierarchical F test attained statistical significance. This was not the case, however, when this variable was entered on the second step, although the hierarchical F test approached statistical significance ( $F = 3.28$ , NS). The hierarchical F test associated with the T.L.S. index was statistically significant irrespective of the order of entry of the variables. Examination of the hierarchical F tests, therefore, suggested that the model was consistent with the independent causes model when the additional variable was entered on the first step. Consideration of the standardised partial effect coefficients indicated that the beta coefficients associated with the number of confidants was negative. This was consistent with the view that the absence of confidants was related to high levels of 'anxiety-depression'. The magnitude of this effect was only 17 per cent of the effect of T.L.S. It appeared, therefore, that the number of confidants was inversely related to the level of 'anxiety-depression' and was independent of the influence of reported life stress. The number of confidants available did not act to modify the impact of life events.

Finally, the results achieved when the total number of vulnerability factors was entered into the general model were essentially similar to those achieved within the total female sub-sample. The hierarchical F tests associated with the T.L.S. scores were significant irrespective of the order of entry of the first order effects. When the summated vulnerability index was entered into the regression equation on the first step its contribution reached statistical significance at the 5 per cent level of significance, however, when it entered on the second step it failed to reach statistical significance at this level ( $F = 3.02$ , NS).

It was argued above that a moderately strong case can be made to give the first order of entry precedence over the second. If this was accepted then this model was consistent with the independent causes hypothesis. Examination of the standardised effect coefficients indicated that a high number of vulnerability factors was predictive of a high level of 'anxiety-depression'. The impact of the vulnerability index, however, was only 39 per cent of the impact of the T.L.S. index. The summated vulnerability index, therefore, did not behave as the vulnerability hypothesis would suggest. Indeed, its impact was independent of the impact of life event stress.

In summary, when all the female respondents under the age of 65 years of age were considered, only two additional variables, namely number of confidants and the summated vulnerability index, contributed to the prediction of 'anxiety-depression'. In both cases the models were consistent with the independent causes hypothesis and not the

vulnerability hypothesis. There was no evidence of non-additivity; that is, no evidence that the deleterious effects of life events were modified by any of the additional variables considered.

VULNERABILITY FACTORS IN MARRIED WOMEN UNDER 65 YEARS OF AGE WITH ONE OR MORE CHILDREN OF 15 YEARS OR LESS

The seven additional variables were again considered with those females who were under the age of 65, married and had one or more children of 15 years or less. This sub-sample was composed of 67 respondents. These analyses were carried out because the original paper on vulnerability factors, by Brown et al (1975), held that the vulnerability model operated for this sub-group of women alone. Failure to replicate the vulnerability model in the two sets of analyses above may have been due to the vulnerability model's low external validity. That is, the vulnerability model may only have applied to a small sub-set of female respondents, not to female respondents as a whole. The caveats attached to each of the variables in the analyses of the total sub-samples also applied in the analyses presented below.

The results obtained by estimating the general model for each of the seven additional variables in this sub-group differed from the previous two sets of analyses (See Table 35). The principal difference was that only two of the overall F statistics achieved significance at the conventional level. This difference could partly be explained by the influence of sample size on significance testing. Statistical significance was achieved in those models containing the

TABLE 35 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH ADDITIONAL VARIABLES AND THEIR INTERACTIONS  
ALL WOMEN UNDER 65 YEARS WITH ONE OR MORE CHILDREN OF 15 YEARS OR LESS  $n = 67$

ADDITIONAL VARIABLE				TOTAL LIFE STRESS				INTERACTION										
5% CONFID LIMITS				5% CONFID LIMITS				5% CONFID LIMITS										
VARIABLE	MEAN	S.D.	F1	F2	BETA	MEAN	S.D.	F1	F2	BETA	R <sup>2</sup>	F	MODEL					
Loss of mother before age 11	0.18	0.39	0.19	-	-0.03	-	26.13	20.56	6.06 *	-	0.30	-	0.01	-	-0.02	0.09	2.08	No effect
Loss of mother before age 15	0.24	0.43	0.25	-	0.06	-	26.18	20.56	6.11 *	-	0.30	-	0.00	-	0.00	0.09	2.13	No effect
No of child-ren	2.02	1.04	0.53	-	-0.18	-	26.13	20.56	6.61 *	-	0.30	-	3.51	-	0.52	0.15	3.58 *	One cause
Employment outwith home	0.49	0.50	0.09	0.63	0.07	-	26.18	20.56	6.70 *	6.17 *	0.30	-	0.04	0.04	0.04	0.10	2.27	No effect
Degree of confiding in spouse	1.53	0.98	2.46	2.50	0.34	-	26.18	20.56	4.62 *	4.58 *	0.32	-	0.42	0.42	-0.23	0.11	2.45	(P < 0.07) No effect
No of confid-ants	1.95	0.99	2.05	1.46	-0.15	-	26.18	20.56	5.64 *	6.24 *	0.28	-	0.00	0.00	0.00	0.11	2.56	(P < 0.06) No effect
Total No vulner-ability factors	1.22	0.98	2.16	1.64	0.04	-	26.18	20.56	5.86 *	6.38 *	0.16	-	0.58	0.58	0.00	0.12	2.83 *	One cause

\*  $P < 0.05$  \*\*  $P < 0.01$  \*\*\*  $P < 0.001$  \*\*\*\*  $P < 0.0001$



number of children at home and the summated vulnerability index as their additional variables. The models with degree of confiding in spouse and number of confidants available approached, but did not achieve, overall significance. These two models were not subjected to further analysis.

In both cases, where the models attained overall significance, examination of the hierarchical F tests revealed that neither of the additional variables contributed to the prediction of 'anxiety-depression', either directly or in conjunction with T.L.S. It should perhaps be noted, however, that the interaction term in the model concerned with the number of children at home approached but did not achieve statistical significance at the 5 per cent level ( $F = 3.51$ , NS). Had the interaction achieved statistical significance the model would have been consistent with the vulnerability hypothesis. Given the absence of the statistically significant relationship, the model was left uninterpreted.

In summary, when all the married women under 65 years with young children at home were considered, only two of the models achieved overall significance at the conventional level. These were the models with the number of children at home and the summated vulnerability index as the additional variables. The other models approached but did not achieve overall significance. In no case was any evidence provided that the additional variables influenced the prediction of 'anxiety-depression', in either a direct or an indirect fashion.

## SUMMARY

The influence of seven additional variables, which were conceptually but not operationally similar to Brown's vulnerability factors, was considered. The general model was estimated for each of the seven variables on three sub-samples of women.

When the total female sample was considered, only the total number of vulnerability factors contributed to the prediction of 'anxiety-depression'. The other variables approached but did not achieve statistical significance. There was no support for the vulnerability hypothesis.

When the female respondents under 65 years were considered, the total number of vulnerability factors and the number of confidants available contributed to the prediction of 'anxiety-depression'. These analyses were consistent with the independent causes hypothesis.

When the female respondents under 65 years with young children were considered, none of the additional variables influenced the prediction of 'anxiety-depression'.

## CHAPTER 10 THE INFLUENCE OF SOCIAL RELATIONSHIP VARIABLES

A recurring theme, that was discernible in the life event literature, was that both the quality and quantity of social relationships have a mediating role on the impact of life events. Authors who have argued this include Lowenthal and Haven (1968), Brown et al (1975), Cobb (1976), Miller and Ingham (1976) and Henderson et al (1980). Their work was considered in detail above.

Within this current study, an attempt was made to measure aspects of social relationships using items drawn from the Social Adjustment Scale (S.A.S.). This Scale was developed by Paykel et al (1971b), Weissman and Paykel (1974) and Weissman et al (1974) (vide supra).

Two types of behaviour were considered by the items used in this study, i.e. 'performance behaviour' and 'interpersonal behaviour' (Paykel et al, 1971b). 'Performance' items were items that related to comparatively concrete and easily quantifiable behaviour. They included items concerned with the number of friends and the number of social interactions that the respondent had. 'Interpersonal' items were items which related to more subtle and 'soft' measures of social relationships and these included the respondents' ability to confide in their spouse, friends and relatives. Twenty-one items relating to social relationships were considered (see Table 36).

TABLE 36 INTER-RATER RELIABILITY COEFFICIENTS (KAPPA) FOR SOCIAL ADJUSTMENT SCALE (SAS) ITEMS RATED BY TWO PAIRS OF RATERS

S.A.S. ITEM	NUMBER OF RATINGS		KAPPA' COEFFICIENTS	
	PAIR 1	PAIR 2	PAIR 1	PAIR 2
Time lost from work	122	92	0.74	0.84
Number of close friends	122	92	0.92	0.95
Degree of confiding in friends	122	92	0.74	0.84
Number of special interactions	122	92	0.77	0.91
Loneliness	122	92	0.72	0.92
Degree of confiding in relative	117	88	0.69	0.73
Dependency upon relatives	117	88	0.83	0.66
Rebelliousness towards relatives	117	88	0.50	0.40
Resentment towards relatives	117	88	0.90	0.58
Degree of confiding in spouse	70	64	0.82	0.75
Domineering behaviour towards spouse	70	64	0.42	0.58
Submissiveness towards spouse	70	64	0.52	0.58
Dependency upon spouse	70	64	0.41	0.55
Lack of involvement with children	31	27	0.40	0.77
Impaired communication with children	31	27	0.61	0.54
Economic inadequacy	122	92	0.54	0.77
Global social leisure	122	92	0.38	0.56
Global extended family	117	88	0.32	0.47
Global marital rating	70	64	0.29	0.43
Global parental rating	31	27	0.37	0.48
Global overall social adjustment rating	122	92	0.61	0.35

#### INTER-RATER RELIABILITY OF SOCIAL ADJUSTMENT SCALE ITEMS

The inter-rater reliability of the Social Adjustment Scale items was assessed by applying the procedure used above to assess the reliability of life event ratings. Thus, as was indicated above, two pairs of interviewers simultaneously interviewed and rated the respondents. Joint ratings by pair 1 were available on 122 respondents and by pair 2 on 92 respondents. Inter-rater reliability was assessed using the Kappa coefficient on all the 21 items.

The results of these analyses are present in Table 36. The values of Kappa that were obtained ranged from 0.95 down to 0.29. In general, it appeared that items pertaining to 'performance behaviour', e.g. number of close friends and number of social interactions were more reliably rated than the more subtle, qualitative items such as a degree of dependency upon spouse, or the more general global ratings. Given the low reliability of some items, it was decided to exclude them from further analysis. A comparatively conservative approach was adopted in choosing the exclusion criterion because of the absence of information regarding the consistency of the fifth interviewer. Nunnally (1967) indicated that reliabilities in the range of 0.50 to 0.60 were adequate in the early stages of research. In order to adopt a conservative criterion, therefore, only those items on which both pairs of interviewers achieved inter-rater reliability of 0.60 or above were considered.

After exclusion of items on this basis, 8 of the original 21 items remained. Two of these items, namely, 'loneliness' and 'time lost from work' were also excluded because it was felt that they might

represent symptoms or concomitants of depression rather than markers of the quality of the respondents' social network. The item pertaining to the 'degree of confiding in spouse' was considered above in chapter 9 on Brown's vulnerability factors.

The influence that the remaining five variables had on the simple link between T.L.S. and 'anxiety-depression' was considered below.

- (1) Number of Close Friends.
- (2) Number of Social Interactions.
- (3) Degree of Confiding in Friends.
- (4) Degree of Confiding in Relatives.
- (5) Dependency Upon Relatives.

The general model was estimated on the total sample and male and female sub-samples for the five variables. The male sub-sample was considered, despite the absence of a simple event-syndrome link, to determine whether certain social support variables might reveal an otherwise obscured link. The female respondents were not sub-divided into three sub-samples, as was done when Brown's vulnerability factors were considered, because there was no a priori justification for doing this. Consideration of smaller more specific sub-samples would have reduced the generality or external validity of any substantive findings.

#### THE INFLUENCE OF SOCIAL RELATIONSHIP VARIABLES WITHIN THE TOTAL SAMPLE

##### Number of Close Friends

The S.A.S. item pertaining to the number of close friends that the

respondent had either seen or telephoned regularly in the previous two months, achieved a comparatively high level of inter-rater agreement (Pair 1, Kappa = 0.92, Pair 2, Kappa = 0.95). Examination of the distribution characteristics of this item indicated that it did not depart, to an extreme degree, from normality (See Table 37).

The modal rating on this item was 2. This indicated that the majority of respondents reported having between five and eight friends who could be regarded as close. When the general model was estimated, for the total sample, with the number of close friends as the additional variable, the resulting overall F statistic was significant (See Table 38). The multiple correlation was, therefore, significantly greater than zero and the general model accounted for 8 per cent of the variance in 'anxiety-depression'. The hierarchical F tests pertaining to both the additional variable and to T.L.S. were all statistically significant, irrespective of the order of entry of the first order effects. The interaction term failed to approach or achieve statistical significance following either orders of entry of the first order effects. The model was, therefore, consistent with the independent causes hypothesis. Examination of the standardised coefficients indicated that the effects of T.L.S. was about twice as large as the effect of the additional variable.

It should be noted that this item was scored so that the more friends the respondent reported the lower the value they achieved. That is, the closer the respondent was to the ideal norm the lower his score on this item. As might be expected, therefore, the standardised effect coefficients indicated that, in general, the fewer friends the

TABLE 37 DISTRIBUTION OF RESPONDENTS' SCORES ON SELECTED SOCIAL  
ADJUSTMENT SCALE (S.A.S) ITEMS

S.A.S. ITEM	MEAN	STANDARD DEVIATION	SKEWNESS	KURTOSIS
Number of close friends (n = 408)	2.21	1.28	0.80	-0.38
Number of social interactions (n = 405)	2.31	1.38	0.69	-0.78
Degree of confiding in friends (n = 379)	1.84	1.19	1.33	0.73
Degree of confiding in relatives (n = 399)	1.94	1.35	1.16	-0.07
Dependency upon relatives (n = 399)	1.52	0.77	1.45	1.79



TABLE 38 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH SOCIAL ADJUSTMENT SCALE ITEMS AND THEIR INTERACTIONS TOTAL SAMPLE

VARIABLE	5% CONFID				BETA	LIMIT	MEAN	S.D.	F1	F2	BETA	LIMIT	5% CONFID				MODEL				
	MEAN	S.D.	F1	F2									F1	F2	BETA	R <sup>2</sup>		F			
No. of Close friends n = 408	2.21	1.28	**	8.19	**	0.14	-	25.73	20.71	***	26.11	***	25.40	0.25	-	0.00	0.00	0.00	0.08	11.37	**** Independent causes
No. of social inter- actions n = 405	2.31	1.38	**	7.87	*	0.13	-	25.73	20.55	***	23.86	***	26.06	0.25	-	0.03	0.04	-0.02	0.07	10.61	**** Independent causes
Degree of con- fiding in friends n = 379	1.84	1.19	0.83	0.79	-0.05	-	25.79	20.09	20.01	***	19.06	0.22	-	0.00	0.00	0.00	0.00	0.00	0.06	6.98	*** One cause
Degree of con- fiding in rel- ations n = 399	1.94	1.35	**	6.81	5.52	0.17	-	25.37	20.56	***	23.61	***	24.93	0.30	-	0.71	0.72	-0.09	0.07	10.43	**** Independent cause
Depend- ency upon relatives n = 399	1.52	0.77	1.65	1.66	0.03	-	25.69	20.73	23.34	***	23.38	0.21	-	0.42	0.43	0.03	0.03	0.06	0.06	8.34	**** One cause

\* P &lt; 0.05    \*\* P &lt; 0.01    \*\*\* P &lt; 0.001    \*\*\*\* P &lt; 0.0001

respondent reported the higher his level of 'anxiety-depression'. This effect of the number of friends available was direct in nature and did not act to modify the deleterious impact of life events.

#### Number of Social Interactions

The S.A.S. item pertaining to the number of social interactions had by the respondent in the previous two months, also attained a satisfactory level of inter-rater reliability (Pair 1, Kappa = 0.77, Pair 2, Kappa = 0.91). Examination of the distribution characteristics of the item indicated that it did not depart, to a substantial degree, from normality. The modal rating was 2 indicating that the majority of respondents reported having attended between 8 and 15 social events in the previous two months.

When the general model was estimated, for the total sample, with the number of social interactions as the additional variable, the overall F statistic achieved statistical significance (See Table 38). The multiple correlation was, therefore, statistically significant and the general model explained 7 per cent of the variance in 'anxiety-depression'. The hierarchical F tests pertaining to both the additional variable and to T.L.S. were also statistically significant; this was the case irrespective of the order of entry of the variables. The hierarchical F tests associated with the interaction term failed to achieve statistical significance following both orders of entry of the first order effects. The model appeared, therefore, to be consistent with the independent causes hypothesis. Examination of the standardised effect coefficients indicated that the effect of the

additional variable was about half the size of the effect of T.L.S

As with the previous additional variable, the number of social interaction item was coded so that the further the subject departed from the idealised norm the higher their rating on this item. Therefore, in general, the fewer social interactions reported by the subject, the higher their level of 'anxiety-depression'. The effect of the number of social interactions, and presumably the effect of the extent of the respondent's social life, was directly related to his level of 'anxiety-depression'. This analysis failed to provide evidence in support of the view that an intense social life acts as a buffer against the impact of life events.

The first two items considered above attempted to measure the more concrete, quantitative aspects of social relationships. The following four variables, in contrast, were concerned with the more 'subtle' and qualitative aspects of social relationships.

#### Degree of Confiding in Friends

Only 29 of the 408 respondents reported that they had no close friends. The remaining 379 respondents were each asked about the quality of their relationships with their friends and the extent to which they could talk openly about their feelings to these friends. The inter-rater reliability coefficients for this variable were rather less satisfactory than those for the more concrete items that were considered immediately above (Pair 1, Kappa = 0.74, Pair 2, Kappa = 0.84). Examination of the distribution characteristics of this variable indicated that it was positively skewed and leptokurtic in

character. The majority of respondents, therefore, felt that they could confide in their friends to some degree. The general model was estimated with the degree of confiding in friends as the additional variable (See Table 38).

The overall F statistic achieved statistical significance and the general model explained 6 per cent of the variance in 'anxiety-depression'. Only those hierarchical F tests that were associated with the T.L.S. index achieved statistical significance. This was true irrespective of the order of entry of the first order effects. The model thus failed to conform to any of the four hypotheses that were considered above. The standardised effect coefficients were, therefore, not subjected to interpretation.

#### Degree of Confiding in Relatives

Only 9 of the 408 respondents reported that they had no contact with relatives other than their spouse or children. The remaining 399 respondents were each asked about the quality of their relationships with their relatives and the extent to which they could talk openly about their feelings to these relatives. The inter-rater reliability coefficients for this item were close to the exclusion criterion (Pair 1, Kappa = 0.69, Pair 2, Kappa = 0.75). Examination of the distribution characteristics of this variable indicated that it was positively skewed and leptokurtic in character. The majority of respondents, therefore, felt that they could confide in their relatives to some degree.

The general model was estimated, with the degree of confiding in

relatives, as the additional variable (See Table 38). The overall F statistic was statistically significant with the general model explaining 7 per cent of the variance in 'anxiety-depression'. The hierarchical F tests associated with T.L.S. and the additional variable were all highly statistically significant, irrespective of the order of entry of the first order effects. Both hierarchical F tests associated with the interaction term failed to achieve or approach statistical significance. The model was, therefore, consistent with the independent causes hypothesis. Examination of the parameters of the model indicated that the effect of the additional variable was about 56 per cent of the effect of T.L.S. The direction of the effect was in the predicted direction, respondents who reported low levels of confiding in their relations generally reporting high levels of 'anxiety-depression'. The quality of relationships with relatives did not appear to influence the impact of life stress.

#### Dependency Upon Relatives

Three hundred and ninety-nine respondents were rated as to their degree of dependency upon their relatives other than their spouse and children. This item was concerned with emotional dependency, in terms of help and advice as well as practical dependency in terms of finance and babysitting etc. The inter-rater reliability coefficients for this item were moderate (Pair 1, Kappa = 0.83, Pair 2, Kappa = 0.66). Examination of the distribution characteristics of the item indicated that it was positively skewed and leptokurtic in form. The majority of respondents, therefore, admitted to some, and perhaps an appropriate, degree of dependency on their relatives.

The general model was estimated with dependency upon relatives as the additional variable. The overall F test achieved statistical significance and the general model explained 6 per cent of the variance in 'anxiety-depression' (See Table 38). The hierarchical F tests associated with T.L.S. were significant irrespective of the order of entry of the first order effects. The hierarchical F tests associated with the additional variable did not achieve statistical significance. Given the failure to achieve statistical significance the parameters of the model were left uninterpreted.

#### THE INFLUENCE OF SOCIAL RELATIONSHIP VARIABLES WITHIN THE FEMALE SUB-SAMPLE

When the five general models were estimated for the female sub-sample the overall pattern of results was the same as those described above (See Table 39). All the overall F statistics were significant and only three of the five additional variables showed an effect on 'anxiety-depression'. The models containing significant additional variables were each consistent with the independent causes model, there being no evidence of non-additivity.

Two differences between the analyses on the total sample and those on the female sub-sample, however, were apparent. Firstly, the overall level of variance explained in the female sub-sample was approximately 15 per cent compared to approximately 6 per cent in the total sample. The quality of the overall fit was thus substantially better in the female sub-sample.

TABLE 39 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH SOCIAL ADJUSTMENT SCALE ITEMS AND THEIR INTERACTIONS  
FEMALE SUB-SAMPLE

ADDITIONAL VARIABLE		TOTAL LIFE STRESS						INTERACTION										
VARIABLE	MEAN	S.D.	F1	F2	BETA	5% CONFID LIMITS	MEAN	S.D.	F1	F2	BETA	5% CONFID LIMITS	F1	F2	BETA	R <sup>2</sup>	F	MODEL
No of close friends n = 230	2.24	1.25	5.62 *	7.58	0.08	± 0.18	25.49	21.24	34.97	34.24	0.24	± 0.23	1.75	1.75	0.07	0.16	14.43	Independent causes
No of social inter- actions n = 230	22.35	1.38	10.79	6.27 *	0.13	± 0.18	25.49	21.24	29.03	33.55	0.28	± 0.25	0.19	0.19	0.07	0.15	13.11	Independent causes
Degree of confid- ing in friends n = 202	1.69	1.10	0.00	0.00	-0.04	-	25.97	20.77	24.07	24.07	0.28	-	0.16	0.16	0.06	0.10	8.16	One cause
Degree of confid- ing in relat- ives n = 225	1.75	1.24	14.21	13.38	0.16	± 0.19	25.61	20.93	35.76	36.60	0.29	± 0.20	0.81	0.81	0.12	0.19	16.90	Independent causes
Depend- ency on relatives n = 225	1.58	0.81	3.33	2.36	0.10	-	25.61	20.93	31.42	32.39	0.36	-	0.00	0.00	0.01	0.14	11.70	One cause
* P < 0.05    ** P < 0.01    *** P < 0.001    **** P < 0.0001																		

\* P < 0.05 \*\* P < 0.01 \*\*\* P < 0.001 \*\*\*\* P < 0.0001

Secondly, when the general model was estimated with dependency upon relatives as the additional variable the hierarchical F test associated with the additional variable approached, but did not achieve statistical significance ( $F = 3.33$ , NS), when the additional variable was entered into the regression equation on the first step. The hierarchical F tests associated with the interaction term failed to approach significance. If the hierarchical F test associated with the additional variable had achieved significance then the model would have been consistent with the independent causes hypothesis and not the vulnerability hypothesis. Given the failure to achieve statistical significance the parameters of the model were left uninterpreted.

#### THE INFLUENCE OF SOCIAL RELATIONSHIP VARIABLES WITHIN THE MALE SUB-SAMPLE

The general model was then estimated for each of the additional variables on the male sub-sample (Table 40). In every case the overall F statistic failed to achieve, or approach, significance, implying that the multiple correlation did not depart significantly from zero. This sub-sample was not, therefore, subjected to further analysis.

#### SUMMARY

When five items of appropriate inter-rater reliability designed to tap



TABLE 40 HIERARCHICAL REGRESSION ANALYSIS OF ANXIETY-DEPRESSION WITH SOCIAL ADJUSTMENT SCALE ITEMS AND THEIR INTERACTIONS  
MALE SUB-SAMPLE

ADDITIONAL VARIABLE				TOTAL LIFE STRESS				INTERACTION					
				5% CONFID LIMITS				5% CONFID LIMITS					
VARIABLE	MEAN	S.D.	F1	F2	BETA	MEAN	S.D.	F1	F2	BETA	R <sup>2</sup>	F	MODEL
No of close friends n = 178	2.17	1.31	-	-	-	26.03	20.06	-	-	-	0.03	1.8	No effect
No of social inter- actions n = 175	2.04	1.28	-	-	-	25.81	19.20	-	-	-	0.01	0.35	No effect
Degree of confid- ing in friends n = 175	2.26	1.39	-	-	-	25.81	19.20	-	-	-	0.02	1.45	No effect
Degree of confid- ing in relat- ives n = 174	2.17	1.45	-	-	-	25.98	20.12	-	-	-	0.01	0.68	No effect
Depend- ency upon relatives n = 174	1.45	0.70	-	-	-	26.16	20.01	-	-	-	0.01	0.51	No effect

social relationships were considered, three aided the prediction of 'anxiety-depression'. In the total sample and the female sub-sample these items were firstly, the two 'performance' variables, namely, the number of friends and the number of social interactions that the respondent had, and secondly the 'interpersonal' item relating to the degree of confiding in relatives. In all cases of significant effect, the effect was direct. There was no evidence to support the view that any of these variables ameliorated or potentiated the impact of life events. No significant effects were found in the male sub-sample.

## CHAPTER 11 DISCUSSION OF RESULTS

### INTRODUCTION

The discussion will be divided into six areas.

Firstly, the results obtained will be summarised and the major findings will be highlighted.

Secondly, the causal validity of the simple association between T.L.S. and 'anxiety-depression' will be discussed in detail, in the light of both the design of the study and the results obtained. The causal validity will be examined in terms of the four forms of validity identified by Cook and Campbell (1979) and discussed in detail above.

Thirdly, the influence of additional variables on the simple link between T.L.S. and 'anxiety-depression' will be discussed. Their predictive value, in general, their importance for increasing our theoretical understanding of life events and 'anxiety-depression', and their implications for treatment, will be considered. Moving from the general to the particular, the significance of and explanations for the synergistic effect of respondents' sex will be examined in detail.

Fourthly, the very apparent failure to replicate the results of Brown and Harris (1978a) will be examined. Methodological and theoretical reasons for this failure will be discussed.

Fifthly, the advantages of the statistical model will be highlighted.

Sixthly, and finally, the importance of specificity in the life event literature will be discussed. In particular, the signal failure of previous studies to corroborate the traditional psychiatric view that 'neurotic' depression specifically is triggered by life events will be examined. This study demonstrated this type of association. An explanation for this contrast with the findings of previous studies is presented.

#### OVERVIEW OF RESULTS

Examination of the demographic features of the obtained sample suggested that it was heterogeneous in structure. Females predominated over males, the whole age range was well represented, and the social class structure was fairly evenly distributed around the modal class, 3 B i.e. 'Skilled Occupation - Manual'.

Comparison of the respondents with the refusers and non-contacts indicated that there were no significant differences in terms of sex or social class. The mean age of the non-contacts, however, was higher than the mean age of the respondents. The refusers, however, did not differ significantly from the respondents. In general, it appeared that the respondents did not differ substantially from the refusers or non-contacts. It was assumed, therefore, that the results obtained could be generalised to the population sampled.

The analysis then focused on symptoms of depression. The principal

scale used was the Zung Self-Rating Depression Scale (Zung 1965). Many of the symptoms were frequently reported. The Zung scores were computed in order to capitalise on the construct validity of the scale established by extensive use and to provide a rough impression of the levels of depression present. Converting these scores by the procedure of Barrett et al (1978), it was found that 74.5 per cent of the sample were symptom-free, 16.4 per cent evinced definite symptoms of depression but of low intensity and 8.2 per cent had depressive symptoms severe enough for referral. These rates of depression, although high, were consistent with previous studies that have used questionnaire techniques (Weissman and Myers, 1978).

Given the fairly frequent occurrence of depressive symptoms, it was possible to examine their relationships with other variables. It was argued above, however, that because of the essential heterogeneity of depressive symptoms (Blumethal, 1975) refining of the symptoms into theoretical and analytically appropriate dependent variables was desirable. Principal Component Analysis was applied to the symptom results to describe the major dimensions of variation that underpinned them. Four principal components, or syndromes (Eysenck, 1953, 1970) were extracted and described as 'anxiety-depression', 'cognitive-depression', 'vegetative-depression', and 'endogenous-depression'.

The structure of depression found in this general population sample showed strong similarities to that found in psychiatric populations (e.g. Kiloh and Garside, 1963; Kendell and Gurlay, 1970; Roth et al, 1972; Kiloh et al, 1972; Paykel, 1974). There were also important

differences. The first component, being composed of anxiety symptoms, depressed mood, self pity, and crying spells, fitted the pattern of neurotic or reactive depression found in psychiatric samples (Kendell, 1976). The other three components were specified by symptoms traditionally linked to endogenous depression (Kendell, 1976); the traditional unitary syndrome was divided into three discrete syndromes.

The relative magnitude of the second component (i.e. cognitive component) was consistent with the view of many theorists (e.g. Beck, 1967; Brown and Harris, 1978; Seligman, 1975) that the cognitive features are of central importance in depression.

Authors such as Paykel (1974) have noted that vegetative symptoms such as fatigue, appetite loss and constipation are related to the endogenous depression pattern. The substantial loading of depressed mood on this component served to specify it as a syndrome of depression. Scores on this component correlated significantly with age ( $r = 0.34$ ,  $P < 0.001$ , two-tailed). This result is consistent with the psychiatric tradition (see for example, Kiloh and Garside, 1963; Paykel, 1974) that endogenous depression tends to occur in older patients.

The fourth component, being specified by diurnal variation in mood, early wakening, weight loss and guilt, conformed to the pattern which Kendell (1976) suggested was central to endogenous depression as it is found in psychiatric samples.

The observation of four syndromes suggests that the traditional binary view of depression does not hold in this sample. Indeed, Weckowicz (1973) has argued that the binary view of depression is inappropriate even for a psychiatric sample. He contended that the principal protagonists in the classification debate have distorted their factor structures by under-extraction and non-rotation of factors.

The four syndromes extracted, however, may represent unrecognised syndromes or syndrome variants. Leighton (1979) indicated that such syndromes may be detected by psychiatric epidemiology. He argued that their detection may be important. They may be more responsive to treatment or environmental factors than classic syndromes. Components two, three and four may, as Leighton (1979) suggested, be sub-syndromes of the grosser endogenous syndrome found in psychiatric samples.

The influence of life events on these four dependent variables was considered.

The inter-rater reliability of the life events ratings ranged from 0.52 for the more subtle features of life events to 0.95 for the more concrete features. The more subtle features were rated less reliably than those rated by Tennant et al (1979) and perhaps, by Parry et al (1981), however, the date, type, independence and number of events were rated reliably.

An index of general life stress (T.L.S.), based on consensually

derived weights of upset entailed by events (Paykel et al, 1976), was developed. Its relationship with each of the four independent variables was determined. Within the total sample T.L.S. correlated significantly with 'anxiety-depression' alone. Sub-division of the sample by sex indicated that this pattern held only for females. Analysis of the relationship between T.L.S. and 'anxiety-depression' indicated that it was consistent with a dose-response relationship.

The influence of six specific types of life stress on the four syndromes of depression was examined. Measures of stress associated with exit events, death and non-death exits, and desirable stress all failed to correlate significantly with any of the four dependent variables. Stress associated with entrance events displayed a counter-intuitive relationship with endogenous depression in the male sub-sample. Part of this relationship may have been spurious due to the effects of age.

Undesirable life stress displayed the same pattern of relationships as did T.L.S. The general failure of indices of specific life stress to correlate with the syndromes of depression might have been due, in part, to their poor distribution characteristics.

While individual indices of specific life stress may have failed to increase the prediction of 'anxiety-depression' over that predicted by T.L.S., it was thought that combinations of indices might have been more productive. Stepwise Multiple Regression analysis was performed with 'anxiety-depression' as a dependent variable, and specific life event indices as the independent variables. Within the total sample,



the combinations of undesirable and exit life stress increased the variance explained in 'anxiety-depression', over that explained by T.L.S. The two indices explained approximately equal amounts of variance. This combination of indices also explained more variance in 'anxiety-depression' in the female sub-sample. These results tended to support the views, firstly, that undesirable change rather than change per se is important, and secondly, that combinations of life event indices may have increased predictive power over single measures (Miller and Ingham, 1983; Cooke and Greene, 1981).

Similar analyses were performed using indices based on interviewer ratings that were designed to have enhanced idiographic sensitivity. The pattern obtained with simple bivariate correlation between four of the five indices (other than average control over events) and the four syndromes was identical to that obtained with T.L.S. This consistency of pattern should not be over-emphasised because these indices and T.L.S. were all based on, and perforce highly correlated with, the number of events experienced by the respondents. To determine whether these idiographically sensitive measures had greater predictive power than the consensually derived T.L.S. measure, multiple regression analysis was performed, with T.L.S. being forced into the equation on the first step. Only within the female sub-sample did one of these measures, total objective negative impact, increase the variance explained in 'anxiety-depression' over that explained by T.L.S. The variance explained was increased by 2 per cent.

The general failure of these indices may in part be attributed to their comparatively low reliability. It may suggest also that the apparent advantages of idiographically sensitive indices over nomothetic indices had been over-emphasised (Brown, 1974).

The focus of the analysis then moved from the simple link between life event indices and the syndromes of depression towards consideration of the influence of additional variables on the T.L.S. - 'anxiety-depression' link.

Demographic and personality variables were considered first. Respondents' sex displayed a synergistic effect with T.L.S. Extensive epidemiological and non-epidemiological evidence indicates that being female is a significant risk factor for depression (Weissman and Klerman, 1977). The presence of a synergistic effect, however, implies that the respondent's sex was not merely a simple risk factor because under adversity, females are at additional risk with life events producing a disproportionate increase in symptomatology.

Age and Neuroticism, but not social class nor Extroversion, showed relationships with 'anxiety-depression'. These relationships were independent of the influence of T.L.S. and did not modify the impact of T.L.S.

The influence of Brown's vulnerability factors on the simple event-syndrome link was considered using variables that were conceptually related, but operationally different from those used by Brown and his colleagues (Brown and Harris, 1978a).

The general model was estimated for each of the seven variables for three sub-samples of women. There was evidence that both the combined vulnerability index and the number of confidants available predicted the level of 'anxiety-depression'. The pattern of relationships observed, however, was consistent with the independent causes hypothesis.

The analysis of results was concluded by examining the influence of social adjustment scale ratings on the simple T.L.S. 'anxiety-depression' link. After exclusion of items because of unreliability or contamination by symptoms, five items remained.

Within the total sample and the female sub-sample, three variables namely, number of close friends, number of social interactions and degree of confiding in relatives influenced the prediction of 'anxiety-depression'. The mode of influence of these variables was consistent with the independent causes model and did not support the view that they might modify the deleterious effects of life events.

The theoretical and practical implications of these principal findings will now be considered.

#### THE PLAUSIBILITY OF THE ASSUMPTION THAT LIFE EVENTS CAUSE SYMPTOMS

The case was argued above that the nature of life event research precludes the application of conventional criteria of causality. Conventional experimental designs cannot be applied, therefore, other criteria for inferring causality are necessary. The plausibility and

validity of causal propositions can be assessed in terms of four types of validity i.e. statistical conclusion validity, internal validity, construct validity and external validity. These four forms of validity refer to four questions about the relationship between the putative cause and putative effect. Firstly, are the variables significantly correlated? Secondly, could the cause variable plausibly produce the change in the effect variable? Thirdly, which theoretical constructs underpin the measured variables? Fourthly, does the relationship apply for different people across different settings and measures?

Evidence in support of each of the forms of validity aggregates to enhance the plausibility of the hypothesis that life events cause symptoms. No absolute criteria for a causal effect are available. The evaluation of validity entails a certain degree of trade-off between different forms of validity. For example, statistical conclusion validity may be enhanced by selecting homogeneous groups of subjects, however, such a strategy generally results in a decrement of external validity due to a lack of generality beyond the subjects selected. The evaluation of any causal hypothesis, therefore, cannot be exact.

Given these caveats, the plausibility of the case made in this current study will now be evaluated. Each of the four types of validity will be considered in turn and the resulting aggregation of evidence in support of the causal hypothesis will be detailed.

### Statistical Conclusion Validity

Statistical conclusion validity refers to the validity of the statement that the cause and effect measures are correlated. The ability to detect the small correlations that typify life event research (Cochrane and Sobol, 1980; Lin et al 1979) is influenced by sample size, the availability of reliable, continuous measures, and the appropriate application of the generally more powerful parametric statistics.

Within this present study, the sample of respondents achieved (i.e.  $n = 408$ ) was sufficiently large to provide a reasonable chance of detecting population correlations down to 0.20 (Cohen and Cohen, 1975). That is, relationships that explained less than 4 per cent of the variance.

With regard to the reliability of the dependent variable, an appropriate level of reliability was achieved initially by capitalising on the reliability of a published and extensively used instrument (Zung, 1965). In addition, however, the internal reliability of the dependent variable was enhanced through the use of Principal Component Analysis (Armor, 1969).

With regard to the independent measure, i.e. the life event inventory, an instrument was chosen in which the life events were defined in fairly specific detail. It was hoped that the use of this type of instrument, rather than instruments in which the events are only vaguely defined, would reduce error variance and thereby enhance reliability (Brown, 1974; Tennant et al, 1981a).

When inter-rater reliability was assessed following the study, the level of inter-rater reliability achieved for the more concrete life event ratings was well within the range that Nunally (1967) indicated was desirable for basic research. The analysis concentrated on these more concrete ratings.

The study was designed to enhance statistical conclusion validity by the use of continuous rather than dichotomous measures of both the dependent and the independent variables. All the measures used tended to be skewed. Parametric statistical tests, however, appear to be robust to even severe deviations from normal distributions (Havlicek and Patterson, 1977). With regard to the life event measures and the syndrome measures, since they were skewed in the same direction, it is unlikely that departure from normality would result in a failure to detect significant correlations.

Following these principles of design and analysis a highly significant correlation ( $r = 0.22$ ,  $P < 0.002$ , two-tailed) was observed between total life stress (T.L.S.) and 'anxiety-depression' in the total sample. It can therefore be argued that this relationship possesses adequate statistical conclusion validity.

#### Internal Validity

Having demonstrated that the putative cause and effect measures correlate, the next step in the evaluation of the causal hypothesis entails the assessment of internal validity. Internal validity is concerned with the extent to which the observed correlation between the measures can be said to be causal and also with the direction of causality between the measures.

As was the case with statistical conclusion validity, evidence in support of internal validity may be derived from both aspects of the design and also from the results achieved.

A fundamental meta-theoretical criterion for assessing the direction of a hypothesised causal effect is temporal priority of cause over effect. It was argued in detail above that two approaches to the imposition of temporal priority are possible; either events that occurred prior to symptom onset are considered or events prior to the interview are considered. Both approaches have advantages and disadvantages. The first approach is adversely affected by the difficulty of retrospectively identifying onset and the consequential arbitrariness in defining events as pre or post onset events (Cranach et al, 1981). The other approach suffers from the disadvantage that the presumed decay in the impact of events over the extended time period between the occurrence of events and the measurement of symptoms will reduce the correlation between events and symptoms (Surtees, 1978).

The study was designed on the assumption that temporal priority would be imposed through the application of the onset concept. In retrospect, however, the author feels justified in abandoning this approach in the analysis and imposing temporal priority by examining events prior to symptoms measured at interview. Going beyond the arguments detailed above, an additional argument must be included. Namely, the specification of four independent syndromes in the analysis necessarily precluded the use of the onset measure. Cranach

et al (1981) noted the difficulties inherent in retrospectively specifying the onset of general psychiatric caseness. If the retrospective dating of the onset of general caseness is problematic then the retrospective dating of the onset of four discrete syndromes is likely to be highly unreliable.

The empirical correlation between T.L.S. and 'anxiety-depression' suggests that any decay in the impact of events over time was not large enough to limit the association. This would tend to vindicate the method of imposing temporal priority. The possibility remains, however, that the failure to achieve an association between T.L.S. and the other three syndromes may have been due to an inappropriate degree of temporal contiguity. That is, the length of time between the occurrence of the events and the measurement of the syndromes might have been too remote to have a reasonable effect. This possibility is unlikely on two counts. Firstly, reanalysis of the relationships between T.L.S. and each of the four syndromes, using the albeit unsatisfactory measure of onset, resulted in an identical pattern of results (Cooke, 1981a). Secondly, the pattern of symptoms associated with 'anxiety-depression' is similar to reactive depression, a condition thought to be more transient than other forms of depression (Bebbington, et al, 1981). The lengthening of the time period between the occurrence of events and the measurement of a transient syndrome is more likely to weaken the event-syndrome association than when less transient disorders are considered.

It was noted above that the assessment of internal validity involved the assessment of the direction of causal effect. The validity of the causal hypothesis in the recent life event literature rests heavily on



the validity with which events can be identified as 'illness-independent' events. That is, the accuracy with which it can be established that events were not produced by the disorder of interest. The merits and demerits of this concept were discussed in detail above. A theoretical difficulty that emerged in the analysis of the results, which could adversely influence the internal validity of the causal hypothesis, should be noted. Illness-independent events, in this study, were operationalised in terms of independence from psychological symptoms in general rather than in terms of independence from each of the four syndromes. In practical terms, as with the operationalisation of four separate onsets, it would be extremely difficult, if not impossible, to operationalise independence from each of the four syndromes. A threat to the internal validity of the T.L.S. 'anxiety-depression' link therefore exists. It is possible that events that were rated as independent from one symptom cluster e.g. 'cognitive-depression' were dependent on another symptom cluster e.g. 'anxiety-depression'. Internal validity would be threatened if events were independent of psychological symptoms in general but dependent on 'anxiety-depression'. The probability that this type of contamination may have occurred is reduced by the observation that the symptoms that specify 'anxiety-depression' were likely to be the most apparent to the interviewer.

The demonstration of a dose-response relationship enhances internal validity and is persuasive of a causal relationship (Susser, 1973; Weiss, 1981). The association between T.L.S. and 'anxiety-depression' was shown to conform to a linear or dose-response relationship.

Finally, with reference to internal validity, perhaps the greatest threat comes from third variable explanations (Cook and Campbell, 1979). Brown (1974), in a seminal paper, enumerated the procedures that should be used in life event interviews to delimit the threat of third variable explanation. The assessment and ratings of life events in this study were designed to conform to Brown's (1974) suggestions for countering the threat of both direct and indirect contamination (Vide Supra). There was one major departure, however, from the rationale advocated by Brown. A crucial tactic in avoiding contamination is that the rating of stress should be maintained independent of the rating or knowledge of symptoms.

This was accomplished in two ways. Firstly, the ratings of event severity were obtained from an independent study. Secondly, the syndromes of depression were derived statistically and independent of the interviewers. There are admittedly difficulties with this approach. It is probably legitimate to argue that any tendency for an interviewer to be more assiduous in interviewing symptomatic patients (direct contamination, Brown, 1974) would be a general tendency and not related to a specific syndrome e.g. 'anxiety-depression'. This is particularly true when the interviewers are unaware of the distinction amongst syndromes.

The form of third variable explanation that is most dangerous to the internal validity of a causal hypothesis is the threat of spurious correlation (Blalock, 1964; Cook and Campbell, 1979). Spurious correlation can occur even in the idealised case of faultless

measurement (Brown, 1974). Spurious correlation could occur, for example, when a third variable such as personality disorder, produces both an elevated rate of events and an elevated rate of symptoms. Any apparent causal relationship between events and symptoms would merely be the result of a mutual correlation with personality disorder.

It was argued above that attempts by Brown and his colleagues (Brown, 1974; Brown and Harris, 1978a) to deal with the problem of spurious correlations merely by careful interviewing procedures was unsatisfactory. Blalock (1964, 1968) argued that, to evaluate whether a correlation is spurious or not, it is necessary to measure potentially important third variables and partial out their effects, and thereby determine whether the correlation between the cause and effect measures disappears.

In the absence of a cogent theoretical model, there are difficulties in achieving this because a very large number of variables could be of some importance. Given this caveat, however, an attempt will be made to assess how severe a threat third variable explanations are to the internal validity of the T.L.S. 'anxiety-depression' link. The impact, of an albeit limited range of third variables, can be assessed through the examination of the three variable models. Hierarchical Regression Analysis with 'anxiety-depression' as a dependent variable and the additional variable entered on the first step, followed by the respondents' T.L.S. score, is formally equivalent to the more common partialling procedures recommended by Blalock (1964) and Cohen and Cohen (1975). Blalock (1968) indicated that spuriousness can be

detected when the regression coefficient of interest approaches zero following the partialling procedure.

In all cases examined above, where there was a significant correlation between 'anxiety-depression' and T.L.S. (i.e. within the total sample, and the female sub-sample) the addition of a third variable did not reduce the regression coefficient toward zero. This was true whether demographic variables, personality variables, Social Adjustment Scale items or vulnerability factors were considered. In each case, following the partialling procedure, the regression coefficient for the relationship between T.L.S. and 'anxiety-depression' remained significantly greater than zero. It would appear, therefore, that this association cannot be explained in terms of the mutual correlation of T.L.S. and 'anxiety-depression' with one of these additional variables. To the extent that these variables represent likely candidates for under-pinning a spurious relationship, this analysis tends to enhance the internal validity of the association between 'anxiety-depression' and T.L.S. There are, however, many other variables that may under-pin a spurious association.

Miller and Ingham (1979) and Tennant et al (1981a) argued that the life event hypothesis might be accounted for, in part, by personality variables with disturbed personality producing both an excess of events and an excess of symptoms. The two personality measures used in this study, namely Neuroticism and Extroversion, failed to modify the association between T.L.S. and 'anxiety-depression'. The former result was consistent with the result obtained by Denney and Frisch (1981). Other personality variables, however, may be of more

relevance or more importance. Surtees (1978) study was suggestive of this. Surtees (1978) obtained the rather disturbing result, in a prospective study, that 'illness-independent' events reported at follow-up could be predicted by prior depressive symptoms. This result could be parsimoniously explained as being due to underlying personality characteristics. Further assessment of personality constructs might be necessary to rule out third variable explanations of this type.

To the extent that appropriate variables were used in the partialling procedure, the internal validity of the association between T.L.S. and 'anxiety-depression' seemed unaffected by the threat of third variable explanations.

Given the design used and both the presence of a dose-response relationship and the absence of any detectable spurious relationships, it can be argued that the relationship between total life stress and 'anxiety-depression' possesses adequate internal validity.

#### Construct Validity

Having firstly determined that the measure of interest correlated significantly and secondly determined that a plausible case can be made in support of the view that life events produce a change in the syndrome measure, the next step is to determine which higher order constructs underpin the relationship.

Evidence in support of construct validity, may be derived from both aspects of the design and aspects of the results. The study was

designed to avoid the dangers of construct under-representation of either the dependent or the independent variables. Heterogeneous measures of the symptom and event domains were used.

Categories of events were established before the field work was started, with distinction being made between 'desirable' and 'undesirable' events and 'exit' and 'non-exit' events. Symptoms were aggregated and distinguished, using statistical techniques, with homogeneous measures of the heterogeneous symptom domain being produced. Items from the Social Adjustment Scale (S.A.S.) (Paykel et al, 1971b), that might have measured symptoms of depression, were excluded prior to the field study in order to maintain Social Adjustment as a distinct construct.

The definition of stressfulness and the weights used in producing the T.L.S. measure were maintained separated from information about the psycho-social context of the individual. Tennant et al(1981a) indicated that three variable models containing contextual measures of threat might be subject to subtle distortions. They suggested that measures of 'contextual threat', while being uncontaminated by symptoms, might be contaminated by additional variables or vulnerability factors. Measures of personality dimensions were based on the EPI, an instrument possessing high construct validity (Eysenck and Eysenck, 1975). These aspects of design were used to enhance the construct validity of the putative causal relationship.

The construct validity was further enhanced by the results obtained from the data analysis. In particular, Principal Component Analysis

was applied to increase the construct validity of the dependent variables by grouping symptoms into those that tended to occur together and distinguishing these from other groups of symptoms (Eysenck, 1953; Royce, 1966). The construct validity of the association between T.L.S. and 'anxiety-depression' was enhanced by the demonstration of specificity. T.L.S. correlated significantly only with 'anxiety-depression', that is, only with one of the four syndromes derived by Principal Component Analysis. Susser (1973) indicated that high specificity is achieved when one construct is related to another construct but unrelated to another conceptually distinct but similar construct. In his discourse on causal analysis in epidemiology Susser, (1973) argued that while the absence of specificity does not negate or refute a causal hypothesis, the presence of specificity increases its plausibility.

Paykel (1974) indicated that specificity can be found by refining both sides of the cause-effect equation and that it may be achieved by analysing the relationships among different cause and different effect constructs. The nature of specificity is well illustrated by the literature on smoking and ill-health. Early studies reported an association between all types of smoking and death through all causes. Later studies found a stronger association between cigarette smoking in particular and death through all causes. Finally, studies demonstrated a very strong association between cigarette smoking and lung cancer.

Specificity was achieved on the effect side of the equation in this study. The general measure of life stress (i.e. T.L.S.) only

correlated with 'anxiety-depression' from the four syndromes or independent theoretical effect constructs. This pattern of associations was reminiscent of the Endogenous-Reactive distinction in the psychiatric literature but inconsistent with previous life-event studies. Possible reasons for the discrepancy with previous studies will be considered in detail below.

Unfortunately, however, examination of the cause side of the equation failed to reveal any compelling evidence in support of specificity. Surprisingly, Paykel et al's (1969) distinction between 'exit' and 'entrance' events failed to enhance the prediction of 'anxiety-depression'. Similarly, dividing 'exit' events into those that entailed death and those that did not entail death did not improve the strength of the association between the cause and effect constructs. It is possible that this failure to correlate might be due to low statistical conclusion validity as a result of the poor distribution characteristics of these variables. The refining of the cause construct into 'undesirable' and 'desirable' events suggested that only 'undesirable' events were relevant. Given the caveat about the poor distribution characteristics of the 'desirable' event index, this result may refine the cause construct to some degree. If this finding were replicated it would imply that only negative change rather than change per se is important in producing 'anxiety-depression'.

This strong evidence for specificity on the effect side of the equation together with much weaker evidence on the cause side of the equation tends to enhance the construct validity of the association



between T.L.S. and 'anxiety-depression'.

### External Validity

Having established a plausible case that life events in general, and perhaps 'undesirable' events in particular only produce a change in 'anxiety-depression' out of the four syndromes of depression, it is necessary to determine how general this relationship is. That is, does this effect apply across different people, settings or times? This type of generality of a relationship is termed external validity by Cook and Campbell (1979).

The evaluation of external validity across different settings and different times requires multiple studies. However, Cook and Campbell (1979) indicated that external validity can be enhanced, within a study, by deliberately increasing the heterogeneity of the subjects selected. Systematic sampling, or pseudo-random sampling, was used in this study to maximise heterogeneity.

The generality of a relationship within a sample can be most easily assessed by examining the influence that third variables have on the association of interest. If a significant interaction effect, sometimes described as an effect modification, is detected then it indicates that the magnitude of the association of interest varies with the third variable. The generality of the relationship is thus diminished.

It was indicated above that when a variety of additional variables, including demographic, personality, social adjustment and 'vulnerability' variables were considered, with one important exception, there was a complete absence of significant interaction effects. Within this sample, therefore, the association between T.L.S and 'anxiety-depression' had considerable external validity. The association was consistent irrespective of age or class, Neuroticism or Extraversion, or social support or 'vulnerability' factors..

The one exception to this rule was respondents' sex. This will be discussed in detail below. In passing it should be noted, however, that simple division of the obtained sample revealed that the association between T.L.S. and 'anxiety-depression' was only significant for the female respondents. This result was underlined by the synergistic effect found in the three variable model calculated with respondents' sex as the additional variable. It may be argued, therefore, that within the female sub-sample, the relationship between T.L.S. and 'anxiety-depression' had considerable external validity.

#### ADDITIONAL VARIABLES AND THE ASSOCIATION BETWEEN TOTAL LIFE STRESS AND ANXIETY-DEPRESSION

Having discussed the evidence in support of the view that the observed association between total life stress and 'anxiety-depression' may have causal significance, the influence of additional variables on this association will now be considered.

In the review essay, it was noted that additional variables should be

considered for three principal reasons. Firstly, they may increase the variance explained in 'anxiety-depression'. Secondly, they may expand our understanding of why particular sub-groups of individuals develop 'anxiety-depression'. Thirdly, they may suggest potential treatment strategies. Each of these three aspects of additional variables will be considered in turn.

### Enhancing Predicted Variance

The inclusion of certain additional variables enhanced the variance predicted in 'anxiety-depression' over that predicted by total life stress alone. Within the total sample of respondents, the simple association between total life stress and 'anxiety-depression' explained 5% of the variance. Certain additional variables dramatically increased the variance predicted. Neuroticism, for example, increased the variance explained some six-fold to 32%. Part of this association, however, might be attributed to contamination of this personality construct with symptom variables.

Other variables contributed to the increase in variance explained to a less dramatic extent. The respondents' sex and age were comparatively important additional variables from the demographic domain, while the number of friends and number of social interactions, together with the extent of confiding to relatives were comparatively important variables from the domain of social relationships.

The sub-division of the obtained sample by respondents' sex, resulted in a significant simple association between total life stress and

'anxiety-depression' in the female sub-sample alone. The implications of the sex difference will be discussed in detail below.

Analysis of the effects of the additional variables was then pursued in the female sub-sample. The simple association between total life stress and 'anxiety-depression' in the female sub-sample explained 10 per cent of the variance. Excluding the male respondents doubled the variance explained.

Neuroticism was once again the most important additional variable with its inclusion in the analysis increasing the variance explained to 35 per cent. From the demographic domain, age contributed an additional 5 per cent to the variance predicted by total life stress, while perhaps surprisingly in the light of Brown and Harris' (1978a) findings, social class did not make a significant contribution.

Within the domain of social relationships, the quality of relationships with relatives was particularly important, almost doubling the variance predicted by total life stress alone. To a lesser degree, the number of friends and number of social interactions also contributed to the prediction of 'anxiety-depression'.

When the measures derived from Brown's vulnerability factors were included in the analysis, only one, the combined vulnerability index contributed to the prediction of 'anxiety-depression' within the female sub-sample. When women over the age of sixty-five years were excluded from the analysis, however, both the vulnerability index and the number of confidants available contributed to the prediction of

'anxiety-depression'. In each case, the increase in variance predicted was a modest 3 per cent.

Before concluding this section, it should perhaps be noted that within the male sample, while there was no significant influence of total life stress on 'anxiety-depression', both age and neuroticism contributed significantly to the prediction of 'anxiety-depression'. While these two findings are of little theoretical importance, they are of some practical significance. The strong correlations between these variables and 'anxiety-depression' tend to reduce the likelihood that some scaling or distribution characteristic of the 'anxiety-depression' scores in the male sub-sample was responsible for the failure to find a significant association between total life stress and 'anxiety-depression'.

In summary, it may be argued that the first purported advantage of additional variables was confirmed. The level of variance predicted in 'anxiety-depression' was increased between a modest and a substantial degree when particular additional variables were included in the analysis.

#### Additional Variables and our Understanding of the Role of Life Events

It was argued above, that the second advantage that accrues from considering additional variables is that it identifies those respondents at most risk of developing excessive symptoms of depression and aids our understanding of why people become symptomatic following life events.

Apart from the synergistic effect of respondents' sex, none of the

additional variables interacted with, or modified the effect of total life stress on 'anxiety-depression'. In a strict sense, the additional variables, other than respondents' sex, do not aid our prediction of which respondents are likely to succumb to the deleterious effects of total life stress.

In another sense, however, the additional variables can be regarded as useful predictors of which respondents are at risk of developing significant symptoms of depression. It can be argued, for example, that for an individual who has a high level of Neuroticism, the additional deleterious influence of life events is sufficient to push them through some notional threshold of symptom severity that leads to consultation and the diagnosis of caseness. These additional variables, while of considerable theoretical interest in their own light, add little to our understanding of the mode of action of life events.

Respondents' sex was the only additional variable that did not conform to the independent causes hypothesis, but rather was consistent with the synergism hypothesis. Weiss (1981) argued that our theoretical understanding of the mode of action of a causal agent can be enhanced if the presumed causal association varies with certain 'host' characteristics. The specification of a synergistic effect implies that the strength of the associations varies with respondents' sex, and therefore, the implications of this finding for our understanding of the effects of life events should be considered.

It is a fairly well established finding in the epidemiological

literature that being female is a significant risk factor for depression. Weissman and Klerman (1977) and Hirschfeld and Cross (1982), on reviewing the relevant studies, have indicated that there are twice as many female cases of depression as male cases. For current purposes it is necessary to go beyond these studies to those studies that have considered life events in mixed sex samples. If 'check-list' studies are excluded because of the compelling methodological reasons noted above, then only a few mixed sex community studies, based on interview assessed life events, are available. Surtees et al (1981) found a correlation between life events and depression in their female community subjects, but not in their male community subjects. This result might be partly attributed to low sample size. Bebbington et al (1981) reported a weak and unstable association between life events and neurotic depression in their male subjects, in contrast to a much stronger and stable association in their female subjects. Miller and Ingham (1979) did not detect a significant interaction effect between sex and life event stress, in general. However, more recent analysis on independent life events has demonstrated a synergistic effect (Miller, 1982, Personal Communication).

Previous work, together with the evidence presented in this current study appears to indicate variation in the strength of the association between life events and 'anxiety-depression' with a 'host' characteristic, namely respondents' sex. Weiss (1981) indicated that such variation should suggest theoretical explanations about the deleterious action of life events. Several explanations for this

apparent heightened susceptibility of woman to the impact of events are available.

The first possibility is that the quality of life event stress experienced by women is different from that experienced by men. While there were no significant cross-sex differences in either the total number of events or the total life stress scores, aggregation of scores may have obscured important qualitative differences in the events experienced. Perhaps the female respondents were subjected to events that were intrinsically more stressful. Sub-division of the total life stress score into different types of stress scores, however, failed to reveal any significant sex differences in their mean values. To the extent that sub-division of events into 'exits' versus 'entrances' and 'desirable' versus 'undesirable' is relevant, these results tend to rule out the possibility that the sex difference can be explained in terms of qualitative differences in events. It must be noted that the qualitative differences may be of a more subtle form than would be revealed by the simple sub-division of events.

A second possible explanation of the sex effect is that it results from the application of consensually derived weights of the degree of upset entailed by events across all subjects. This approach may obscure important cross-sex differences in the significance, salience or personal meaning that particular events possess. The birth of a child, while significant for both partners, probably has more profound psychological and social impact on the mother. Paykel et al (1971a), however, failed to find a significant sex difference in the weights assigned to events. Therefore, while there were no significant



differences in the nomothetic total life stress scores, perhaps differences in the idiographically more sensitive measures might reveal the source of the sex difference.

Unfortunately, however, cross sex comparison in terms of the total subjective impact and total objective impact scores all failed to achieve or even approach statistical significance. Failure to attain statistically significant results on cross-sex comparisons of these indices might merely reflect the comparatively poor reliability of these indices. Given this caveat, there is no compelling evidence from the study to suggest that the female respondents experience different levels or different types of stress when compared with their male counterparts.

A third explanation of the sex difference is that men respond to life events by developing different types of symptoms. In this present study, no significant associations were observed between total life stress scores and any of the four syndromes derived to map out affective symptomatology. Weissman and Klerman (1977) have speculated that the sex difference in the rate of depression may occur because women respond to life events by becoming depressed while men respond by abusing alcohol. The author is unaware of any evidence to support this view. In this current study, however, alcohol consumption in the week prior to the interview was estimated and this did not correlate significantly with life event indices in either men or in women (Cooke and Allan, 1983). Explanation of the sex difference in terms of this mechanism cannot be more than speculation.

Several authors (e.g. Gove, 1972; Radloff, 1975) have suggested that women are more susceptible to depression than men because of restriction of access to economic, social or legal resources or due to the application of less effective coping strategies. Extending this view, it could be argued that the sex difference in response to life events occurs because the necessary resources or coping strategies are not available to counter the impact of life events. If access to legal, social, economic and educational resources were critical, one might expect that the social class of the respondents would modify the association between life stress and 'anxiety-depression'. This was not the case in this study.

Finally, Cochrane and Stopes-Roe (1980) echoing Brown and Harris (1978a) have argued that the apparent susceptibility of women to life events may be mediated by low self-esteem. They suggest that the conventional feminine role, which entails passivity and low self esteem, may either make women less able to cope with stressful life events, or more likely to cope with them in a pathological manner. These interpretations, however, remain highly speculative.

The role of respondent's sex has been discussed in detail. Two other demographic variables, namely age and social class, are of interest, as understanding of their influence may enhance our understanding of the influence of life events.

The age of the respondents appears to have an independent influence on the level of 'anxiety-depression' reported - symptomatology decreasing with age. Previous epidemiological studies have failed to provide any

consensus regarding the relationship between age and depression and thus respondents' age cannot be regarded as an established risk factor (Weissman and Myers, 1978; Cooke, 1982). The experience of life events and respondents' age appear to be surface markers of separate etiological mechanisms. The absence of a significant interaction effect tends to indicate that the influence of total life stress does not vary with age and this tends to rule out the possibility that age facilitates the development of appropriate coping strategies or the acquisition of relevant material resources. The decline of symptom intensity with age must, therefore, be attributed to other psychological or physiological processes.

Social class did not contribute significantly to the link between life events and 'anxiety-depression'. This may at first appear surprising in view of some previous results in the life event literature that suggested that social class determined either differential exposure to stressors or differential impact of comparable stress (Brown et al, 1975; Kessler, 1979). This effect of social class has not been generally replicated in epidemiological studies. Henderson et al (1978) and Miller and Ingham (1976) failed to detect even a simple link between social class and psychiatric symptoms. It would appear, therefore, that the normal concomitants of social class such as education, access to material, social and economic resources do not influence the impact of life events with regard to this particular syndrome of depression.

If discussion moves from the domain of demographic variables to the

domain of social relationships certain results of interest emerge. The influence of certain of the Social Adjustment Scale items on 'anxiety-depression', while not consistent with the vulnerability hypothesis, may add to our understanding of 'anxiety-depression'. This is particularly the case with those items that conform to the independent causes hypothesis. It was argued above, that when this occurred, items could be considered to combine with life events to push more people through a notional threshold into caseness.

Within both the total obtained sample and the female sub-sample, the quantitative measures of social relationships, namely the number of social interactions and the number of close friends and also the qualitative measure of degree of confiding with relatives, increased the prediction of 'anxiety-depression'. The magnitude of influence of the two quantitative items was between one-third and one-half of the influence of T.L.S. in the female sub-sample. The variance predicted being increased, in each case, by about 50 per cent. Similarly, the influence of the degree of confiding in relatives was also about half the influence of T.L.S., however, the variance predicted was almost doubled.

The improvement in variance explained by the inclusion of each of these variables, is both significant and desirable within the context of life event studies (Dean and Lin, 1977; Miller and Ingham, 1979; Cochrane and Sobol, 1980). Although, they may not increase our understanding of the influence of life events they may enhance our understanding of 'anxiety-depression'.

The finding that the number of friends available and the number of social interactions in the previous two months were related to 'anxiety-depression' echoed previous results (e.g. Surtees, 1978; Henderson et al, 1981; Miller et al, 1976).

Several explanations that account for these relationships are available and they have been detailed by Lin et al (1979) and Henderson (1981).

On the one hand, 'anxiety-depression' may lead to changes in the level of support, either perceived or actual. Feelings of anxiety, irritability or fatigue might lead to withdrawal from friends or a reduction in social contact. Depressed mood might equally lead to the perception and reporting of fewer friends or fewer social encounters. There is some empirical evidence to support this view. Paykel and Weissman (1973), in a longitudinal study of the social support characteristics of depressed female patients, argued that withdrawal from social leisure activities was perhaps due to the general reduction of activity that is a frequent concomitant of depression.

On the other hand, certain theorists, notably those of a behaviourist persuasion (e.g. Hersen et al, 1973; Lewinson et al, 1970; Eastman, 1970) have argued that the direction of causality is in the opposite direction. Essentially these theorists have argued that depression is a direct function of a reduced frequency of social reinforcement; a reduction in social encounters, for example, being related to increased levels of depression. Whitehead (1979), in a detailed

review of behavioural treatment of depression, argued that there is some limited evidence from controlled intervention studies to support the view that improving social ability and thereby increasing the number of social encounters, can have a beneficial effect on depressive symptoms.

Unfortunately, the influence of the two quantitative social adjustment scale items can be interpreted in the light of either of these two hypotheses. Using cross-sectional data it is impossible to distinguish between these two forms of explanation, and indeed, it may be the case that both mechanisms operate in different individuals.

The pattern of results obtained regarding the qualitative aspects of relationships was apparently paradoxical. Within the female subsample, neither the degree of confiding with spouse nor the degree of confiding with friends achieved statistical significance. The degree of confiding with relatives, however, produced the strongest effect of all the Social Adjustment Scale items, as judged by variance explained, because the variance explained in 'anxiety-depression' was almost doubled. No immediately obvious explanation for this pattern of results is apparent. To speculate, it may be the case that the differences in variance explained may reflect variability in quality of relationships with relatives. While an individual tends to choose his friends and spouse, his relatives are preordained. Examination of the respective standard deviations of these three measures revealed that there is in fact substantially greater variability in the quality of relationships with relatives than relationships with either spouse or friends.

In conclusion, it would appear that consideration of these particular additional variables has added little to our theoretical understanding of the influence of life events. Their evaluation may not have been totally futile, however, because the negative findings reduce the plausibility of certain explanations. While lacking the important interactive effect, certain of the variables aid the understanding of 'anxiety-depression' and assist in the prediction of which subjects will report elevated levels.

#### Implications for Treatment or Prevention

The third argument in support of the evaluation of additional variables was that they might have implications for treatment or prevention. Andrews and Tennent (1978) characterised the study of life events as a 'Doomsday' exercise because it was unlikely that identification of a causal effect would lead to the prevention of disorders.

Part of the intuitive appeal of the vulnerability hypothesis derives from the implication that certain subject characteristics may protect or immunise against the deleterious effects of life events. The results reported by Brown and Harris (1978a) might be interpreted as suggesting that if an individual's ability to confide were increased then his resistance to life events would be enhanced. The evidence collected in this study did not support the vulnerability hypothesis and, therefore, does not support the view that intervention aimed at altering subject characteristics or their social network would necessarily immunise them against stressful life events.

The observation that certain of the additional variables behave in accordance with the independent causes hypothesis reduces their interest in this context. While they may be amenable to intervention, their alteration would not influence the impact of life events. Therapeutic procedures for altering the characteristics that achieved statistical significance will not therefore, be discussed.

The identification of a plausible causal association between life events and 'anxiety-depression' in the female sub-sample together with the notable absence of 'protective' factors, requires that treatment approaches aimed at directly reducing the impact of life events should be considered. This current study did not entail any therapeutic intervention, therefore, any views must necessarily be speculative.

Susser (1981) noted that '... since psychological stressors of necessity are mediated by the psyche, we need to find the means to take into account the perception or denial of stress by the subject' (p3). This view was perhaps more succinctly expressed by the Stoic philosopher Epictetus, '...men are not disturbed by things, but by the view they take of them' (quoted, Beck et al, 1980, p8). Therapeutic strategies aimed at altering the subject's interpretations of life events might be effective. Beck et al (1980) have developed a therapeutic package of cognitive-behavioural techniques designed to alter patients' cognitions about themselves, their world and their future. There is some empirical evidence to support the efficacy of this approach with clinically depressed patients (e.g. Rush et al, 1977; Blackburn et al, 1981). The author is unaware of any empirical



work where these techniques are being used to alter subjects' perception of particular life events. In general clinical practice using these techniques therapy may be directed at improving the subject's evaluation of specific stressful events.

Silver and Wortman (1981) in their detailed review of reactions to life events, emphasised the importance of the provision of a setting to allow the ventilation of feelings. A therapist, therefore, might facilitate direct coping with life events by allowing and encouraging the expression of feelings and emotions.

Structured direction and guidance might also have its place in therapy. If a patient is considered to be susceptible to the effects of stressful life events then the therapist should attempt to identify and predict likely future events in an attempt to reduce the probability of their occurrence. This might be achieved by guidance or by direct intervention. There is some empirical evidence available that might lend support to this type of approach. Tennant et al (1981b) demonstrated that life events that neutralised the impact of early life events accounted for 31 per cent of the remission from symptoms observed in their community sample. To take a simple example, redundancy may have produced an increase in symptoms while the neutralising event of obtaining employment may have reduced symptoms. Therapeutic strategies directed towards increasing the client's ability to obtain employment might increase the frequency of such a neutralising event.

To conclude this third section of the discussion, it would appear that the consideration of additional variables has had certain, albeit limited, advantages. Firstly, the level of variance predicted in 'anxiety-depression' was increased by the consideration of particular additional variables.

Secondly, the finding that the respondents' sex interacted with total life stress suggested several explanations about the way in which life events produce distress. Further, while respondents' sex was the only 'host' characteristic to modify the impact of total life stress, other additional variables aided the prediction of 'anxiety-depression'. These additional variables, therefore, improve the prediction of which individual will experience significant levels of 'anxiety-depression' following stressful life events.

Thirdly, the effective absence of additional variables that acted to modify the impact of stressful life events forced the investigator to speculate that the main therapeutic endeavour should be directed either towards changing the individual's perception of the event or towards mobilising her resources to generate neutralising life events.

#### THE STUDY RESULTS AND THOSE OF GEORGE BROWN AND HIS COLLEAGUES

One of the principle aims of the study was to replicate the results obtained by George Brown and his colleagues, using different procedures, measures and samples. Replication of their results would enhance the external validity of their social model of depression.

The results of this study provided partial replication in that a simple association between general life stress and one syndrome of depression was observed. It was argued above, in detail, that this association possibly had some causal significance.

When the more complex additional variable models were considered, however, the results obtained in this study failed to replicate those obtained by Brown and his colleagues. This failure to replicate occurred irrespective of whether the full female sub-sample, the younger female sub-sample, or the sample of mothers with young children was considered.

Within the total female sub-sample, only the vulnerability index, a summation of Brown's four vulnerability factors, contributed significantly to the prediction of 'anxiety-depression'. Indeed, although this additional variable made a statistically significant contribution, the model containing it conformed with the independent causes hypothesis, and not with the vulnerability hypothesis.

Within the sub-sample of younger women, only the number of confidants available and the vulnerability index achieved statistical significance. Once again, however, the models containing these additional variables were consistent with the independent causes hypothesis and not the vulnerability hypothesis.

Finally, within the sub-sample of women with children under the age of fifteen years at home, the three variable models either failed to

achieve overall significance, or the additional variable failed to contribute to the prediction of 'anxiety depression'.

Before considering possible reasons for this dramatic failure to replicate the more subtle features of the findings of Brown and his colleagues, two points should be considered. Firstly, most of the previous studies, reviewed above, have also failed to conform with the vulnerability hypothesis. Secondly, the detailed examination above of the work of Brown and his colleagues revealed significant internal inconsistencies within their data base.

Given these two points, it is still valuable to examine possible explanations for the failure to replicate in this current study. There are a variety of technical explanation that may account for inconsistencies with the vulnerability hypothesis.

The sub-sample of females obtained ( $n = 230$ ) may have been too small to demonstrate the interactive effects that are a defining characteristic of the vulnerability hypothesis (Duncan-Jones, 1980; unpublished manuscript). This seems an unlikely explanation for two reasons. Firstly, this current study applied more powerful statistical techniques and, therefore, fewer subjects are required to demonstrate an interactive effect (Cohen and Cohen, 1975). Secondly, Brown et al (1975) demonstrated their most powerful vulnerability effects on a sample of 126 women with young children at home. Sample size, therefore, is unlikely to be of vital importance in this context.

While sample size may not be of great significance, sample composition may be of some importance. Brown and his colleagues demonstrated substantial differences across different socio-cultural settings by comparing their early results obtained in the inner city of Camberwell to those obtained in the rural Hebrides. Solomon and Bromet (1982), in attempting to explain their failure to replicate the results of Brown and Harris (1978a), argued that findings obtained for city populations do not generalise across to rural settings. This argument, however, cannot be used to explain the inconsistent results obtained in this present study because most of the respondents lived within the inner city areas of Glasgow.

Solomon and Bromet (1982), suggested that their failure to replicate earlier findings may also be attributed to their use of a homogeneous group of subjects (i.e. 435 mothers). Unfortunately, this argument cannot be used with regard to this current study because heterogeneity was ensured by systematic sampling of the population of interest. Indeed, it should be noted that Solomon and Bromet's argument may be irrelevant within this context. Although Brown and Harris (1978a) applied their findings to all women under 65 years of age their earlier findings (Brown et al, 1975) were based on a homogeneous sample of women with children still living at home. The possibility that the vulnerability model only applied to specific sub-groups of women was examined by carrying out all the relevant analyses on three female sub-samples. The pattern of results obtained in each of these sub-samples was inconsistent with the vulnerability hypothesis. It can be argued, therefore, that the failure to replicate the earlier

findings cannot be convincingly attributed to either sample size or to these features of sample composition. While the samples used may not be central to the failure to replicate earlier studies, the measures used may be of primary importance.

With respect to the dependent variable, the manner in which the concept 'depression' is used and operationalised can vary considerably (Weckowitz, 1973). Within the life event literature, Miller and Ingham (1979) used the term to denote a symptom whereas Brown and Harris (1978a) used the term to denote a syndrome. The severity of the disorder described by Brown and Harris (1978a) is clearly greater. Miller and Ingham (1976) attributed their failure to replicate the results of Brown et al (1975) to this difference in severity of depression.

In this current study, the term 'anxiety-depression' was used to denote a syndrome. The procedure used in operationalising the concept, however, was very different from that used by Brown and Harris (1978a).

Firstly, the definition of the syndrome was based on the pattern of symptoms irrespective of the distribution of the respondents relative to the syndrome. It will be argued in detail below that the method used in defining a syndrome can critically affect the perceived association between life events and syndromes.

Secondly, the definition of a syndrome used was dimensional rather than categorical. It was argued above that an essentially arbitrary

division in terms of severity could lead to inconsistencies in the results obtained. The use of a dimensional measure may have caused this failure to replicate.

Thirdly, the composition of 'anxiety-depression', as the name implies, tended to be dominated by anxiety features. It is possible, therefore, that the results of Brown and Harris (1978a) referred to a purer syndrome of depression. This may be the case. Tennant and Bebbington (1978), however, have argued that at least 30 per cent of Brown and Harris's (1978a) 'depressed' cases were not suffering from depression but from some other recognizable neurotic syndrome.

The approach adopted in this study, of developing the syndromes of depression from the community sample is defended, however, because 'depression' as it exists in the community was of primary interest. Leighton (1979), in a detailed account of developments in psychiatric epidemiology argued that syndromes observed in the community may be very different from those observed in psychiatric settings. Leighton (1979) also indicated that syndromes observed in community settings may be more clearly linked to environmental factors than the grosser psychiatric syndromes. These differences in the dependent variable used in this study as compared to those used by Brown and Harris (1978a) may account for differences in the results obtained in the three variable model.

Differences in the independent variables may also be important in respect to the failure to replicate. This current study was concerned with discrete life events, whereas, the studies of Brown and his colleagues were concerned with discrete life events and long-term

difficulties. It is possible that the effects of long-term difficulties are substantially different from the effects of discrete life events. Re-analysis of the results of Brown et al (1977), carried out below for a different purpose, demonstrated that the associations between life events and different types of depression were more stable than the association between long-term difficulties and different types of depression. While these differences are suggestive they cannot be regarded as firm evidence about the differences in the effects of the two forms of provoking agents.

Although at an intuitive level, it is likely that long-term difficulties significantly affect the psychological well-being of respondents, there are substantial conceptual and operational difficulties in including them in a study. Tennant and Bebbington (1978) argued that long-term difficulties could be confounded by vulnerability factors. For example, marital disharmony, a long-term difficulty, might be affected by low intimacy, a vulnerability factor. Tennant and Bebbington (1978) argued further, that there may be significant operational difficulties in assigning temporal priority of long-term difficulties over vulnerability factors. They suggested that long-term difficulties, which by definition must have been present for more than two years, may have occurred long before the vulnerability factor. For example, having an alcoholic spouse, a long term difficulty, may have occurred for a long time before the respondent ceased employment outwith the home, a vulnerability factor. These differences in the definition and operationalisation of the provoking agents may have led to the failure to replicate the results



of Brown and his colleagues.

Finally, the measures used to operationalise the vulnerability factors or additional variables were very different from those used by Brown and his colleagues. The rationale underpinning the use of these particular measures was discussed in detail above. While the measures used were not identical to the earlier measures, they were conceptually related. Once again this approach is defended because, if the results achieved by Brown and Harris (1978a) are to be regarded as important, they should be robust to differences in operational form of the same construct. To the extent that failure to replicate can be attributed to this factor, the external validity of the findings of Brown and his colleagues is reduced.

In conclusion, therefore, it appears that a range of methodological factors could account for the current study's failure to replicate the more subtle features of Brown's social model of depression. Given the results of the literature review, however, it may be the case that the vulnerability model is untenable.

#### ADVANTAGES OF THE MATHEMATICAL MODEL

Two principle advantages accrue from the application of the mathematical model. Firstly, the model allows the distinction between the synergy and vulnerability hypothesis to be made. Tennant and Bebbington (1978) argued that '... there are difficulties in separating the vulnerability model from the synergy model using standard statistical techniques.' (p 572). The results obtained in

this study with respondents' sex, can be clearly interpreted as being consistent with the synergy hypothesis and not the vulnerability hypothesis.

Secondly, and perhaps more importantly, the mathematical model allows comparison of the magnitude of effect of the independent variables, in the presence or the absence of non-additivity. Measures of magnitude are important in this field particularly with reference to the vulnerability model. Brown and Harris (1978b) argued that their vulnerability hypothesis critically depends on the relative importance of the provoking agents being so much greater than the importance of the vulnerability factors. Unfortunately, Brown and Harris (1978a) do not use an appropriate measure of effect magnitude. Bebbington (1980) indicated that Brown and Harris (1978a) were in error in using the chi-squared statistic as a measure of the magnitude of association. The chi-squared statistic depends not only on the magnitude of the association but also upon sample size. Bebbington (1980) argued that comparisons of effect magnitudes across sub-samples of different sizes could lead to errors of interpretation.

The use of the mathematical model described above and the partial derivatives that are an integral part of this procedure, can provide clear estimates of the relative importance of the variables. These estimates of importance are independent of sample size.

The mathematical model was developed in this study because the modelling of vulnerability factors required that the technique should

be capable of modelling non-additive relationships. From the point of view of illustrating the value of this technique, it was unfortunate that the majority of the models conformed to the independent causes hypothesis. These models did not require the calculation of partial derivatives because the Beta coefficients of the regression model were equivalent. The advantages of the modelling technique, however, can be highlighted by examining the three variable model containing respondents' sex as the additional variable.

Evaluation of the relative importance of respondents' sex and T.L.S. was achieved through the comparison of partial derivatives. The standardised effect coefficient for T.L.S. was 0.22 and for sex was 0.19. Thus, despite the presence of a non-additive relationship, it was possible to demonstrate that each of these variables contributed essentially similar amounts to the prediction of 'anxiety-depression'.

This comparison is valuable. It is often difficult to assess how important a role life events have in producing depression and comparisons with established risk factors may be helpful. Extensive epidemiological and non-epidemiological evidence indicates that being female is a significant risk factor for depression (Weissman and Klerman, 1977; Hirschfeld and Cross 1982). The comparison of partial derivatives, provides the investigator with an intuitive 'feel' for the importance of life events. In this case the value of the partial derivatives were essentially equal suggesting that T.L.S. and respondents' sex are equally important factors with regard to 'anxiety-depression'.

The use of the non-additive model also amplifies the role of respondents' sex as a risk factor. The presence of a synergistic effect implied that the respondents' sex is not merely a simple risk factor, but rather, under adversity, females are at additional risk because life events produce a disproportionate increase in symptomatology. Under conditions of high life stress, women appear to be more susceptible to 'anxiety-depression' than men. This non-additive mathematical model facilitated the identification of this effect. The possible explanations for the effect were described above. To the extent that similar effects can be identified our understanding of the role of stressful life events is increased, and the value of the mathematical model is emphasised.

#### SPECIFICITY IN THE LIFE EVENT LITERATURE

The final issue to be considered is that of specificity of effects. The term specificity is used in the epidemiological literature to indicate the extent to which one construct can be used to predict another. Susser (1973) and Tennant et al (1981a) have argued that the presence of specificity tends to enhance the plausibility of a causal relationship.

The detection of specificity is critically linked to the construct validity of the independent variable, this in turn is critically linked to the manner in which the symptoms are clustered together. Initially, in life event studies, single heterogenous measures of dysfunction were used to measure psychological distress (e.g.

'caseness', Brown and Harris, 1978a; 'depressed patient status', Paykel et al, 1969; 'neurotic impairment', Tennant and Andrews, 1978). Rabkin and Struening (1976), in their detailed evaluation of the life event literature, sounded a warning regarding this approach. They stated that '... the continued use of one measure to represent an obviously complex domain of symptoms will frequently lead to limited and erroneous conclusions.' (p 1020).

In reducing the level of heterogeneity in the symptom domain it is necessary to test for convergence between groups of symptoms and divergence from other symptoms. Cook and Campbell (1979) indicated that some form of inter-correlational analysis is appropriate and traditionally factor analysis has been used for this task. Factor analysis can be applied to classify symptoms into separate syndromes, that is, into clusters of symptoms that are correlated amongst themselves, but relatively uncorrelated with other symptoms (Eysenck, 1953, 1970; Garside and Roth, 1978). Eysenck (1953) indicated that factor analysis systematically achieves what the clinician achieves intuitively, that is, it determines which signs and symptoms tend to occur together. Eysenck (1953) argued that the factorial stage is essential in any causal analysis as it refines the dependent variable and excludes epiphenomena. Eysenck (1953) and Royce (1966) regarded factor analysis as a tool for increasing construct validity, this is emphasised by Eysenck's (1953) statement that '... a factor is a hypothetical causal influence underlying and determining the observed relationships between a set of variables.' (p 108).

The ability to demonstrate specificity between the experience of life

events and particular independent variables is critically linked to the mode of classification used. There are two steps in the formulation of any classification system (Eysenck, 1970; Garside and Roth, 1978). The first is the classification of symptoms into syndromes as defined above. The second is the generation of a classification of subjects in relation to particular syndromes. This second step determines whether discrete diagnostic categories can be identified. Cattell et al (1966), Eysenck (1970) and Weckowitz (1973) noted that when several syndromes of psychological stress had been described it is possible that discrete diagnostic categories may be present or may be absent. The presence or absence of discrete diagnostic groups is open to empirical evaluation (Cooke, 1980a), unfortunately, in the life event literature the assumption of their existence has often been made a priori (Garmany, 1958; Leff et al, 1970; Brown and Harris, 1978; Thomson and Hendrie, 1972; Benjaminsen, 1982; Bebbington et al, 1982; Barratt, 1979). The assumption of discrete diagnostic categories is probably particularly inappropriate with regard to epidemiological studies of psychological distress (Ingham and Miller, 1976; Leighton, 1979). Bebbington et al (1981a) indicated that the low frequency and non-specific nature of symptoms found in the community makes the delineation of specific diagnostic groups particularly difficult. Given that the question regarding the presence or absence of diagnostic groups is to some degree amenable to empirical validation, it is unfortunate that the a priori assumption of their presence is common in the life event literature. The assumption can degrade the level of construct validity in the dependent variable and obscure the presence of specificity of effect.

This problem is well illustrated by a study of the life events experienced by depressed patients, an area where the presence of discrete categories is more readily accepted. Brown and Harris (1978a) examined the relationship between life-stress and the psychotic-neurotic dichotomy. Each of the 114 patients in their cohort was allocated to one of these diagnostic categories on the basis of clinical features alone. Brown and Harris (1978a) carefully evaluated the relationship between provoking agents and several discriminant functions which maximally discriminated between the two groups in terms of these clinical features.

Initially, Brown and Harris (1978a) examined the frequency of provoking agents across the two diagnostic groups. They found that they were not significantly different. Accepting that diagnostic unreliability could attenuate the relationship between diagnostic categories and provoking agents, they re-assigned patients to each diagnostic category on the basis of the patient's position upon a discriminant function.

Following the re-assignment significant differences emerged. Analysis of the data provided by Brown et al (1979) Table 1 using chi-square indicates that re-assignment of 10 per cent of cases results in cross category differences in event frequency emerging ( $p < 0.05$ ). Re-assignment of 20 per cent of cases resulted in cross category differences in long-term difficulties ( $p < 0.02$ ) (Table 41). The stability of this result, across different levels of re-assignment, may be adversely influenced by the adequacy of a discriminant function. The adequacy appears to be poor as the maximum amount of

TABLE 41 CHI SQUARED VALUES OF THE DIFFERENCES IN FREQUENCY OF PROVOKING AGENTS AMONG MEMBERS OF THE RECONSTITUTED PSYCHOTIC AND NEUROTIC GROUPS DERIVED FROM BROWN et al 1979

Percentage ReAllocation	Event and/or Difficulty	Event	Difficulty
0%	0.95	0.60	1.32
5%	2.52	2.93*	2.95*
10%	2.52	4.42**	1.78
15%	2.08	3.46*	3.38
20%	2.87*	5.07**	6.30***

\* P < 0.10 (Two Tailed)

\*\* P < 0.05 (Two Tailed)

\*\*\* P < 0.02 (Two Tailed)



variance explained by the diagnostic dichotomy, adjusted for chance effects, was only 24 per cent (corrected R Squared).

If provoking agents are differentially linked to the two syndromes then one might expect such a trend to be evident. Unfortunately, the magnitude of this link is obscured by the approach used by Brown and Harris (1978a). They noted that '... clinical differences between the 'endogenous' and 'reactive' groups were few indeed' (p 217) and further that "... 'typical' symptoms of either group can occur in the other" (p 217). Given this, it is likely that a unimodal distribution of the form of figure 3 is present. The bivariate distribution is probably not entirely symmetrical. The 'neurotic' sub-group is probably a more heterogenous group as a result of the diagnostic tendency, which Brown and Harris note, towards diagnosing 'neurotic' depression on the basis of a lack of psychotic features.

On Figure 3, subjects A and D will experience significant degrees of either psychotic or neurotic depression. Subject B will experience equally severe degrees of both psychotic and neurotic depression, whereas subject C will experience equally low degrees of both. If the patient (Pp) in the psychotic group is contrasted with a patient (Pn) in the neurotic group then it is clear that both patients (Pp and Pn) experience high levels of each syndrome and they differ merely in terms of which syndrome predominates. In contrasting such patients, the differential effect of life stress must be obscured as their levels of neurotic (reactive) depression are not substantially different. This is true for all the patients who lie near the cutting score of the discriminant function whether they are experiencing

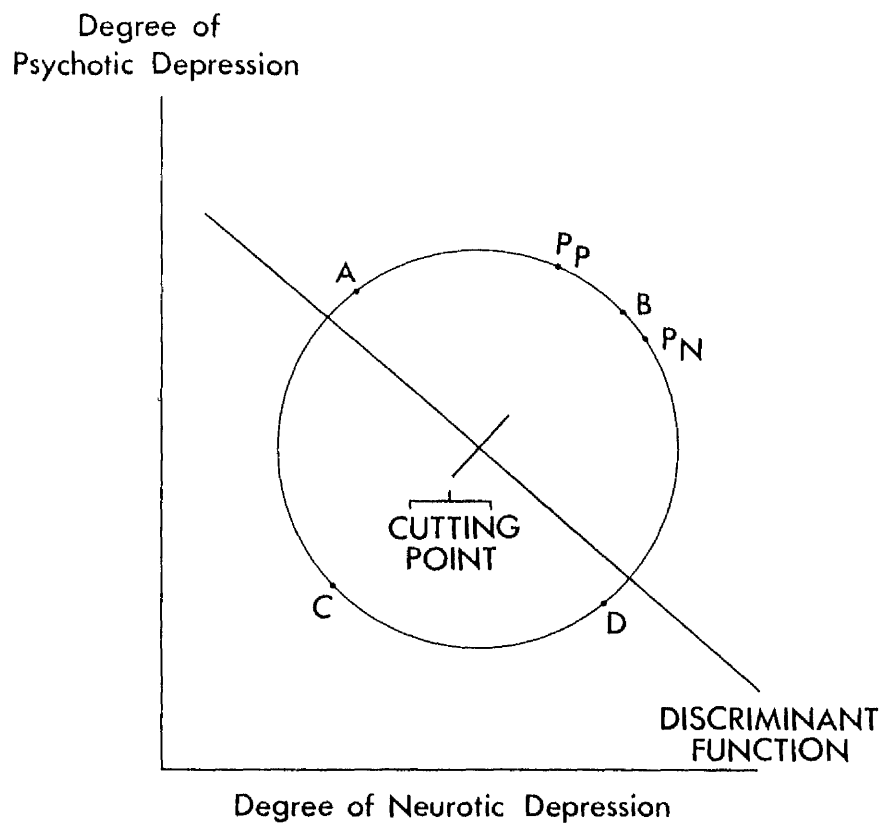


FIGURE 3. PATTERNS OF PATIENT SYMPTOMATOLOGY IN  
RELATION TO THEIR LOCATION ON A DISCRIMINANT FUNCTION.

equally high, moderate or low degrees of each syndrome.

The conclusion of Brown and Harris (1978a) that "... the psychiatric tradition has been misleading in its claim that there are, in any sense, two clearly distinct forms of depression and that the psychotic type is in general not 'reactive'" ( p 217) must be regarded as erroneous.

The a priori assumption of discrete diagnostic categories can thus lead to a lowering of specificity and, therefore, of construct validity.

The author is aware of two substantial studies in which life stress and symptoms were evaluated independently and a dimensional approach to the classification of symptoms was adopted. Ulenhuth and Paykel, (1973a, 1973 b), in a patient sample, found that the degree of life stress determined the intensity but not the configuration of symptoms. This lack of configural specificity may be due to the symptom factors used in specifying symptom configurations having been evolved to measure symptoms in neurotically anxious subjects (Williams, 1968). There may be no substantial differentiation of event-syndrome links within the domain of neurotic symptoms.

In a further study Paykel (1974) carried out a Principal Component Analysis of 28 symptom ratings for 185 depressed patients. The first component measured severity, while the second a bipolar component, contrasted the characteristic features of endogenous and reactive

depression. In an attempt to discriminate between endogenous and reactive depression, in terms of their antecedents, Paykel computed the correlations between two life stress scores and the patients' scores on the bipolar component. The correlation with one stress score achieved significance in the predicted direction while the other failed to do so. This tenuous link, however, vanished when the effects of age were partialled out. Paykel (1979) commenting on this result stated '... the relationship between stress and symptom pattern, although in the predicted direction, was so weak as to be trivial' (p76).

This conclusion is contentious as it is based on the interpretation of a bipolar component. Garside (1976) has argued that rotated factors '...indicate whether and to what extent the patient was psychotically depressed and to what extent he was neurotically depressed.' (p 63).

Bipolar components, however, are analogous to discriminating scales and in no sense are they descriptive. The criticism of Brown et al's (1979) approach noted above applies equally to this study. The bipolar component, being analogous to a discriminate function, does not provide a direct description of the degree of the reactive depression or the degree of endogenous depression experienced by patients. This may have resulted in the irrelevant correlation being '... so weak as to be trivial.' Correlation of the stress score with the rotated components may have indicated a greater specificity of effect.

These methodological issues may explain the apparent lack of specificity within the life event literature. The evidence of

Bebbington et al (1981) and Finlay-Jones and Brown (1981) together with the evidence presented in this thesis will tend to contradict the common view exemplified by the following quotation from Steele (1978), '... it would seem that life event stress is relevant to the timing of onset of illness, rather than nature of illness.' (p 313). Life events may not be a universal cause but rather a cause of certain circumscribed disorders.

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DEPRESSION SURVEY

INTERVIEWERS' MANUAL NO.1:  
GENERAL INTERVIEWING TECHNIQUES.\*

\* This manual was firmly based on the work of Atkinson  
(1971)

## APPROACH TO EMPLOY WITH THE PUBLIC

### A INITIAL CONTACT BETWEEN INTERVIEWER AND INFORMANT

The success of the work of the survey depends on the goodwill of the general public and on their willingness to co-operate in surveys voluntarily. No one is compelled to give an interview. The quality of the information we obtain from the public depends very much on how well you, the interviewer, have explained the purpose of the enquiry and the kind of atmosphere you have managed to create between yourself and the informant during the interview.

The person we wish to interview is unlikely to have prior knowledge of our organisation, or the survey in hand. In turn, you are a stranger to the informant. From this unpromising beginning it follows that you must work quickly to establish contact with the stranger if he is to be made to feel at ease in your company and willing to voice to you his true opinions or to confide facts relating to his pattern of living.

#### Necessary Introductory Points

Before beginning to interview an informant, there are some six points to be made to him:-

- (a) The name of our organisation
- (b) An explanation of how the person came to be selected for an interview
- (c) The purpose of the survey
- (d) The confidential nature of the enquiry. The confidence of those who give us information is respected under all circumstances; the identity of informants is not disclosed to anyone outside the Social Survey, nor to members of the public nor to the press. In particular reports and other analyses based on information supplied by members of the public are presented in such a way that the identity of our informants is not revealed.
- (e) An indication that co-operation is voluntary though this does not necessitate the use of the actual word on every doorstep. The point is covered by asking the informant either "May I ask you" or "Would you be willing to help us" or "Will you take part in the survey?"



- (f) Some indication of the length of the interview is required. Never mislead people into thinking it will take a few minutes if you know it will take half an hour or one hour. For long interviews it is sensible to quote the average time taken but indicate it could take longer if the person were particularly interested in the subject.

The initial contact is to be made by you in person. You must show your informant your authorisation card, usually on entering the house. This gives weight to points (a), (d) and (e) above.

All the above points must be covered before you begin to question a person but the order in which you make the points and the stage at which they should be mentioned is left to your discretion, bearing in mind the following.

People are not usually forewarned of your visit and it is your job to identify the person chosen and to explain the purpose of your visit in a way that will make sense to the chosen person(s).

In calling at an address you will find the demeanour of the person you will meet will help you decide how best to put the survey to them. You can judge from their facial expression where one argument is failing and a different line of appeal is necessary.

There are two stages to the initial contact at an address.

Stage 1 we will refer to as the doorstep contact.

Stage 2 is what happens when you get inside the house.

#### STAGE 1 OF CONTACT DOORSTEP CONTACT

KEEP YOUR DOORSTEP INTRODUCTION BRIEF. If the chosen person is immediately co-operative but shows no sign of asking you into the house, this may be because he has been interviewed by other callers who took only ten minutes. After establishing that a person can give sufficient time at that point, it is in no way impolite to suggest "May I come inside?". It is conducive to a good interview to have your informant at ease inside his own home. If you have had occasion to make point (f) above, then people will usually invite you in straightaway unless you have called at a busy time. It would be tactically correct to withdraw as soon as you realise the time is inopportune. If you try to persuade the informant to give an interview at what is a wrong time for him, you are asking for a refusal.

\* For this survey our provisional estimate of the time

required is 90 minutes.

Similarly, if a person is full of self-doubt, or in a bad mood and consequently disinclined to co-operate when you are discussing the purpose of your call and you fear he is on the point of saying no, it sometimes helps to give yourself the chance of returning on another day.

Some people profess to be busy but you may judge them to be evasive. To these people you must give a careful explanation of the value to us of their participation. With this minority of the population if you sense that to press for co-operation there and then will result in them taking fright and saying no, then leave saying, "If I may, I would like to call again some other day to see if you are then less busy". Providing they do not forbid you to call at the house the chances are you will gain co-operation on recall. People appreciate the importance you attach to the interview if you do bother to come back.

Some people's immediate reaction is to tell you they are not typical, they do not know anything about the subject and they genuinely feel that they would be of no use to us. It is your job to explain to them that providing they were the persons whose names came up by chance for interview, then they are the only persons you wish to see. No others can replace them. You can assure them that you know they will be able to help us in the way we wish, if only they will let you talk to them. They do not have to be any sort of expert on any subject to help you.

### Issues of Identification

You need to identify the person to be interviewed before you can put over the purpose of your visit. Correct identification is vital. It is worth discussing here the general procedure for contacting informants because the impression you make on the would-be informant, or his family, on first sight can make or mar one's chances of gaining co-operation.

### Method of approach when identifying an individual:

If we are concerned to know the public's attitudes on an issue then the type of sample drawn will usually consist of individuals. You will have been given a list of persons, both their christian names as well as surnames. Therefore, your first task on reaching a house, having knocked at the door, is to identify the selected person by using his full name the name which will be supplied to you from the electoral roll. If he or she is not at home, then indicate to whoever has come to the door that you are looking for the named person and ask at what time he or she may be in.

It is most important to try to get through to the person who has been selected for interview and to give your explanation of the reason for the visit to that chosen person. (This chosen person will be referred to as your "informant" throughout the rest of this handbook)

If it transpires that you are talking to a near relative, for example, wife or mother, then you may feel it advisable to produce your authorisation card and give a minimal explanation of your presence by saying "I am calling on behalf of the (name of organisation), having been asked to see your (husband/son). If I may come back at the time (or night) when you say he will be in, I can explain the purpose of my visit. It is not connected with anything that he knows about and I would like the chance to explain it to him in person."

Our experience of trying to explain surveys secondhand suggests that to do so lessens one's chances of co-operation. Garbled versions of the reason for your visit get through to the person concerned if they come from another member of the family. It is important that, whilst refraining from going into details of the purpose of your call, you in no way create the impression that you are holding back information. It is no help to create suspicion within the mind of one of your would-be informant's relations since to do so would only hinder your chances of co-operation from the named person.

The whole essence of contacting people in person to get information is based on the knowledge that the person-to-person situation affords you the chance of summing up the informant and providing an explanation of the object of your visit in terms that he will understand. Another member of his family may differ from the named person and the kind of explanation you have given to them, as to the reason for the survey, may, in fact, not appeal in any way to your informant. Or it may be that your explanation is perfectly adequate but that the intermediary is not sympathetic towards the aims of the survey, becomes prejudiced, and stops you getting through to see your chosen person.

Sometimes a named individual is available on first call but not for the interview because he is viewing you with timidity or else incomprehension. Your chances of success rest with finding out whether anyone else lives at the address (such as husband, or daughter) so that you can come back and explain the purpose of your visit in the other person's presence. This is not to say you want to interview any but the named individual, but if someone normally relies on his relative's judgement as to the rightness of an action, you will do best in such a case to win the support of the relative in the other's presence and so establish through him real contact with your informant.

Similarly if a would-be informant says he is willing to co-operate but his spouse or other relative does not believe in surveys, and can you interview quickly before the relative gets home, do not do this. If you sense, or are told of, family opposition in this way, suggest calling back to see the member of the family in person, to discuss our work, so your subsequent interview is by approval. We do not want to cause any family discord.

You will see from the above that a lot of care should go into establishing that you have the right informant and that you give yourself the best chance of putting the survey purpose over to him in person. The importance of this cannot be overestimated if you are to achieve a high response rate.

Once face-to-face with your informant, cover most of the points (a) to (f) above.

Special Points To Bear In Mind When Contacting Informant

(a) Appointments.

It is most important to keep any appointment that you make with the public, if you know you will be unable to keep an appointment either call earlier to cancel it, or else write to them if there is time. A semi-appointment should be binding and dealt with in this way on your part. On the other hand if the public break appointments you must accept it with good grace and try again on another day or evening.

(b) Minors.

Whatever the type of sample, if the person to be interviewed is a young person (under 18 years, living at home), permission of his parents or guardian must be gained before the interview takes place.

If on any such survey the parent or guardian wishes to be present at the interview the research officer concerned will tell you whether or not it is to be allowed or whether he prefers not to have an interview in such circumstances.

(c) Employees.

If your informant is an employee living in the employer's own home eg a maid, gardener or housekeeper, then the employer's permission must be sought. In turn the employee must be told he is free to decide whether or not he wishes to participate. He does not have to do so simply because his employer has agreed to his being seen by you.

B STAGE 2 OF CONTACT

After the "doorstep" contact, once inside the house you may well be asked by the informant to say what you have really come about. This does not mean, necessarily, that your doorstep explanation has been inadequate. It is probable that most people only half listen initially because they are preoccupied with thoughts of whatever was happening before the doorbell rang or they are making a judgement on whether or not to let you into the house.

Once inside the house repeat what you have said on the doorstep, expanding on the topic of the survey and explaining more fully about sampling procedure or our organisation's bona fide nature, to an extent that will satisfy the informant. Say this much to him once inside the house, irrespective of whether or not you are asked to do so. So not to

be lulled into foregoing explanations because a type of informant says "come in" straight away and appears to trust you, you should cover yourself by making sure he knows where you are from.

### Avoiding Misconceptions

All sorts of misconceptions may arise from an inadequate explanation. Sometimes the informant connects your visit with some circumstances in his private life of which you are totally unaware. For example, the informant may have been in hospital recently, or have applied for a pension or a house, and immediately assume that you have called because of this. Be on the alert for misunderstandings of this kind, as it is important to make the informant understand that there is no connection between any two events. Such an initial misconception might make the informant apparently more willing to give the interview, thinking that it has some connection with his own affairs. But he is likely to be justifiably annoyed when he finds out during the course of the interview that it is not so. Also the feeling that the interview is likely to have some bearing on a personal matter might direct him to answer the questions in a particular way which he feels would be to his advantage.

### Authorisation Card

When in the house always show the informant the authorisation card. In addition hand to them a copy of the letter which explains

- (a) The confidentiality of the survey.\*
- (b) The voluntary nature of the survey.
- (c) The purpose of the survey.

It is useful at this stage to say, for example, "As you will see from the letter anything said to us on the survey is treated as confidential. It also says that it is up to the public whether or not they want to take part", but having said this, you must add at once. "The value of our survey lies in getting everybody's participation and we are grateful, as in your case, that you are helping us".

Do remember to take back the card at once as it is an important document offering proof of the bona fides of your visit. The card must be shown to each informant.

Though we advise showing the card in the house, exceptionally it can be shown usefully on the doorstep if a timid person, perhaps a person living alone, is hesitant about letting you enter.

\* Although it is important to stress confidentiality, it is equally important not to overdo this - many are not the least bit concerned, but may become so if it is stressed too strongly.

### Putting Over the Purpose of the Survey

You need to set the right tone for the interview by neither blinding informants with science, talking down to them nor making unsubstantiated claims as to a study's purpose or outcome.

Remember, no interviewer is expected or should pretend to be, an expert on the subject matter of the survey. If the informant assumes you have specialist knowledge, point out that this is not the case and that we are a research unit and deal with many projects. You are basically a reporter. In fact you will find that the wording of the questions and instructions for the survey cover any technicalities of the subject matter which you may need to know.

You are meant to study your instructions and take notes at briefings when you are being told the background to the survey. Then in the interview, you will be able to give an account of the purpose of the survey.

### Interviewer Individuality in Survey Explanation

It may seem from the above that one is suggesting that you adopt a highly stereotyped approach to the public. This is so in respect of stating fully to an informant why he is being approached by us, but in order to get the understanding of all types of informants you will need to be extremely flexible in your choice of words and length of explanations.

For each survey, before working on it, you will need to think out several ways of introducing it to the public. You can emphasise a different facet of the survey purpose to suit different types of people. Adolescents, housewives, professional and retired persons would not all respond equally well to exactly the same introduction.

If people react rudely, which is rare, on no account react to the rudeness, but try to find out whether it hides fear or guilt; once you know to what a person objects or has concern about, you have a good chance of winning him over.

### Overcoming Non-Co-operation

For our survey to be wholly representative of the population we need to achieve 100% response. In a sense your own part of the sample is deficit (that bit less representative) by each informant that you fail to interview.

Non response can be due to:

- (a) Death of your chosen informant; or demolition of the address at which we asked you to call.
- (b) Inability to find your informant at home because he is in hospital, goes out frequently, works long hours and in consequence has insufficient time to spare for an

interview.

(c) Outright refusal to participate in surveys.

These categories form a minority of the sample set but are worth mentioning to show the problems of random sampling and to illustrate which non-contact you cannot avoid and which to overcome to some extent with perseverance.

To (a) above, clearly no blame is attached to the interviewer!

To (b) above, the non-contact rate due to informants not being found at home can be reduced to hardly any, since a survey is usually in the field over three to four weeks. This means one has time to call back on different weeks to catch up with persons who were on holiday or in hospital on your first call. If you are prepared to put yourself out by calling many times on busy people and to fix appointments for a week or so ahead, in nearly all cases, ultimately, your informant will find time to give you an interview.

To (c) above, Very few people are entirely unwilling to co-operate on surveys, providing they are offered a good explanation as to the purpose of the survey, re-assured on how they came to be chosen for interview and on the confidential nature of their participation.

However, you must appreciate the public's freedom to refuse us an interview and not press anyone who states unequivocally that it is against his principles to take part in surveys: neither must you take advantage of a timid person's inability to put his fears and rejection of you into words, if you are aware that he cannot accept you.

It is important that you are convinced of the value of surveys before you ask for the public's co-operation otherwise you might imagine you see a refusal at a stage when an informant is showing only minor reluctance; perhaps not wishing to be bothered or by being diffident about his ability to help us.

Your tone with doubtful people needs to be matter of fact, pleasant, enthusiastic, reassuring and quietly persistent. It is only when you have said as much as you can to an informant on reasons for the study and reasons for bothering them, that you can be certain, if such is the case, that what we want is anathema to your informant. An unhappy "not today thank you" housewife, trying to shut the door, if left so remains unhappy and possibly more fearful of callers in future.

#### Refusal Effect on the Interviewer

Real refusals are few, but the effect of a refusal on you, unless you take steps to avoid it, can be to debilitate. If this does lower your morale it can have the result that your dejection affects other people as you call on them and gets you more refusals, which are not genuine, but which are caused by your own lack of confidence in what you are doing.

If you do have a refusal, afterwards think how you handled it.

- (a) How did you introduce yourself and the survey?
- (b) Was your informant distraught at the time?
- (c) Did you allow for his mood in what you said?
- (d) Did you call at a socially unconventional time for the district in which you were working; either lunchtime or late in the evening?
- (e) Was there anything unconventional in the way in which you were dressed on that occasion?

If the answer is yes to any of these, then learn for the future what to avoid and so overcome the problem.

### C CONDUCTING THE INTERVIEW

The foregoing sets the tone of your contact with any informant. However, your role in establishing a friendly atmosphere has only just begun. Whilst your careful introduction of the survey will mean your informant is prepared for some questioning, if he is to be kept interested and his attention directed to the subject of the survey for as long as is necessary, encouragement and attention from you are called for as the questioning proceeds.

#### Seating

Sit facing your informant whenever possible, so that he cannot read the schedule or closely watch the answers being recorded. If he becomes too aware of the form filling process the spontaneity may go from his replies. Take care to sit facing the light for deaf people so they may lip read if they want to. Get your informant sitting comfortably; an elderly person should be cajoled into sitting in his own chair rather than that you should take it yourself, if it is proffered. On the other hand if you have a busy housewife in the midst of a chore such as ironing, she might be happier if encouraged to get on with her work, if she prefers whilst you talk to her.

#### Documents

In general it is best not to get schedules out until you have entered the house, shown your card and had some sign that your informant has accepted your presence.

Remember many people are unused to paper work and may feel uneasy on sight of many papers. It is important to have all papers to hand (but in your folder) when you enter the house so that you can produce them in an unobtrusive way. Similarly, have the necessary pencils and any prompt cards to hand to avoid any hiatus on entry. If you have to search for materials this would, wrongly, give a diffident informant time to have qualms about his ability to help. People need leading gently into the questioning and your efficiency in starting the interview smoothly depends on your preparedness for it. Your interview does not begin with question 1 on the schedule and end with the last question on it, rather it runs from the moment you enter the house until your departure from it.



If you are au fait with the subject matter of the survey, the order and the special instructions for each question, then during the interview you will have time, correctly, to concentrate on your informant's reaction to what you are saying. In any normal conversational situation if one person never looked at the other the conversation would soon stop, and so it will in the interview situation if you fail to look expectantly at the informant for replies.

You need to deliver the questions distinctly, and additionally, if the person is at all deaf, loudly. Your tone of voice should be governed a little by your informant and the situation in which you are interviewing. If he has a soft voice, or if there are other people present, it helps to use a soft tone oneself to indicate you are talking confidentially to him, but remember to speak clearly throughout. Watch the informant and if he shows any sign of bewilderment or slight suspicion, give further explanation of the general object of the enquiry or, if the question is a factual one, give a reason for the particular question and its relevance to the subject matter of the survey.

### Preambles

Several topics are covered in an interview and in order to help the flow of the questioning you can give a preamble when you are introducing a new topic. Without the use of linking phrases from you the informant may begin to feel the questions are never ending, yet with their use he can be kept interested and sense that you are moving toward the end of the interview.

If you mention to him that you are changing the subject a little (and why) it helps to avoid any upset in your relationship which could occur if the informant noticed himself that the nature of the questions was changing and he felt you had moved on to something unrelated to whatever you had said the survey was about.

A particular example of the type of topic it is necessary to introduce is one that occurs on all surveys under the heading of classification, where we are asking for background facts about the informant. You will have introduced your survey as being about mental health in the community. The informant has agreed to talk about this subject. However, unless he is familiar with survey techniques, he may not see the relevance of your asking him for details of who lives in his house, his occupation or his age last birthday, all of which you will need to ask at some stage in the interview. We need to have these data because in our surveys we aim to reproduce in miniature the characteristics of the whole population. We must relate our findings to many factors in the lives of our informants in order to discover which factors affect their opinions on the subject under enquiry: this much you must tell him.

### Pace and Tone of Questioning

You need to keep an interview progressing at a pace to suit your informant. The speed at which you ask questions must be governed by the speed at which your informant talks and thinks out his answers, rather

than by your own natural speed. Learn the schedule layout and partly memorise the actual wording of the questions before starting the interview. In this way you can faithfully ask the correct questions, but phrased conversationally. If questions are read in a natural tone, rather than read parrot-like, they will be better understood by your informant, and your task of creating a natural relationship will be easier.

Continue speaking clearly for the whole of the interview and take care not to speed up your questions when you are towards the end of your list of addresses. An informant is always hearing the questions for the first time so allow him plenty of time in which to answer, but do not allow the interview to drag. Remember not to talk too much yourself (other than in putting questions) and correctly avoid saying anything that could influence the informant's answers in any way. A few moment's silence whilst the informant thinks out an answer is often desirable. On the other hand, avoid long pauses which might irritate or embarrass him. A slight pause while you are writing down the answer will seem natural and must occur sometimes, since ALL THE ANSWERS MUST BE RECORDED AT THE TIME OF THE INTERVIEW.

Though the informant will see you are writing it is best not to draw his attention to the extent to which you are recording his responses. If, wrongly, you read out what he has said, he might start behaving differently by limiting replies because he is self-conscious or by reeling off soap-box statements because his ego prompts him to take the limelight.

Do not allow too much digression on the part of the informant between questions, but sense when it is very necessary to allow him to wax confiding and say something out of context of the interview. If all informants are kept too rigidly in the path of the interview, in some situations you will lose out on data because an informant may be bursting to describe some other experience to you. It may be only if he is allowed to tell you what he feels he must, that in turn your relationship will be deep enough to get the best from him for the rest of the questioning.

### Role of the Interviewer

So far the emphasis has been given throughout this manual to the possible reaction of the informant to the interview. What of your role as interviewer?

Consider the situation. You are sent by us to meet people unknown to you. But when you meet them you are facing a situation common to you throughout your life. You have coped with endless encounters with strangers in trains, at work, at parties and you have entered into conversation with them.

Some conversations have no purpose other than to pass time pleasantly. Yet participants in many conversations aim to make them purposeful, they decide there are specific things they will find out of the other person. Other than when one is with friends the titles of the participants (eg doctor and patient) often indicate how purposeful the conversation is

going to be and whether it is turning into an interview rather than an interchange of ideas.

When you meet your informant you make contact as one person to another. It is your subsequent words that begin to define the roles you wish to adopt with this stranger, so that he begins to see and accept his role as "informant".

You know that you are with the informant in a business situation to collect data and not to tell him much about yourself. Were you to talk about yourself it could have an effect on him; you might come to be seen as a special person and then his responses would differ, he might start giving special replies in order to please, gratify, or have some other effect on you. He might decide your roles were changing and you would find he had turned it into a discussion, or general conversation, so that you were led away from the specific line of questioning or, one could say, you have lost "control" of the interview. The guide lines given earlier are meant to help you establish your role with the informant.

We want you to build up an informant's assurance during and, as soon as possible in the interview. If he is at ease with you and satisfied as to the purpose of questions it should help his comprehension and result in greater accuracy of response.

We need you to show appreciation and interest in what your informant is saying in order to get his involvement at sufficient depth. Yet you have the task of not getting personally involved with the informant because you must keep control of the line of questioning.

Good rapport is probably the most crucial element in our type of survey, together with an ability to encourage detailed disclosure of pertinent information whilst eliminating inventiveness wherever possible.

The use of what has been termed "antecedents" will help achieve this - the interviewers should wherever possible, phrase the questions in terms of what they already know e.g. "you mentioned your mother's illness....", "you said your daughter married last year....". This can be especially useful if the respondent has earlier denied something which the interviewer wishes to probe further.

Pauses are often important and interviewers should not hurry the subject by trying to reduce them. In general, pausing depends on eventualities, but the interviewer should always remain attentive and interested. Skill in diverting the subject from irrelevancies can be learnt!

## Summary of Points to Observe When Asking Questions

### 1 Learn layout of schedule before beginning field work

Learn to scan questions so you are never unsure of sequences and can judge if pace of interview is correct. You need to judge whether you are spending a reasonable proportion of time on any one question according to its relative importance to the subject of the survey.

2 Ask the question in sequence and in the exact words in which they are printed on the schedule in the first instance, and use a conversational but deliberate tone so that the informant has the best chance of hearing the question. Guard against any lapse into your own words and listen carefully to your informant throughout.

### 3 Watch out for people mishearing or misunderstanding questions

A likely source of error is that they are still thinking about the answer they have given to a previous question. It is not necessary for them to ask you to repeat a question again before you may do so. Remember the informant will not know and cannot be expected to know survey definitions. It is your task to decide how to classify the facts and not the informant's.

4 Use preambles or linking comments when passing from one group of questions to the next in order to prepare the informant for the change of subject.

5 Use only standard probes on opinion questions but use them fully. Remember they are only effective if used efficiently; think whether the need is for more or for clarifying data when deciding which probe to use. Always bear the question fully in mind when you are deciding whether to probe further.

6 On probing for facts constantly watch that familiarity with a schedule and with similar kinds of replies does not lead you into the habit of half assuming responses. The use of inverted and leading questions is a sign that this may be happening.

7 Do not read back to informants what they have said to you unless you must quote back one or two words in order to show them what you want to know more about.

8 If you need to repeat a question or if you consider that the informant has misunderstood, generally adopt the line that you may have failed to make something clear which is why you want to go back over it.

## D SPECIAL CIRCUMSTANCES AFFECTING THE INTERVIEW

### When to Interview the Informant Alone

Earlier, in discussing initial contact, we mentioned the fact you might meet other members of the family. It follows that often they will be present in the house whilst you are interviewing an individual. There are some cases in which you should try to arrange to conduct an interview alone with the informant. You may be questioning him on matters that he does not discuss with other members of the household, or you may be wanting his opinion and his alone, uninfluenced by anyone else. However, since a considerable proportion of the population have only one living room and many interviews occur in the evening, it is difficult to arrange to see people alone. You can mention to informants that it might be best if you could see them alone. If you have explained that you want their views or to discuss some subject which they know they do not normally discuss with their family members, then they may go out of their way to let you sit, perhaps in the kitchen, where you will be uninterrupted for the necessary hour or so.

### When to Interview with Other People

If the information being collected is purely factual, such as data on housing, ages of family members or other classificatory items there is no harm in a third person being present, so long as he is in a close enough relationship to the informant for the informant not to mind his hearing what you have to say. On factual studies it may even be an advantage to have someone else present, where the other person knows the facts rather better than the informant and can help in providing data. Note you can only enlist the help of someone sitting in if your informant permits it. In no case can you take factual information without the informant's permission and/or when he is absent.

There are certain cases in opinion surveys where an informant needs to have a family member present for reassurance before he will have enough courage to say what he thinks. A drawback of having anyone else present during an opinion interview is that it is so difficult for onlookers to keep from "helping" your informant by putting words into his mouth or bursting out with some view of their own to which he voices agreement. Ideally, any question designed to find out the informant's opinion must be answered by the informant only.

If the informant is not alone when interviewed on an opinion survey, you must try to ensure that the informant's answers are influenced as little as possible by anyone else who may be present. If a third party interrupts at all, you should explain as tactfully as possible, if it is the case, that at the moment you want only the views of the person being interviewed, but will be very interested to hear what the other person has to say afterwards. If this fails to prevent further interruptions, you must take care to record only those answers from the informant which were unprompted by a third party. It is fairly easy to exclude opinions expressed by the onlooker to which your informant does not react but it is difficult in the case where the suggestion leads the informant to say

"I agree" or "I think that too". If this happens you will have to record the intervention and your informant's subsequent response to it since it acted as a stimulus to him. In such cases you need to make a note beside the answer saying whether, in your judgement, the informant's response was a lip service one, or a genuine opinion awakened by the prompting.

It is always important to have good public relations established between the interviewer and people surrounding the informant. Members of the family who sit in on an interview cannot be wholly ignored by you and anyone coming in during the interview should get some explanation from you as to your presence, and why you are interviewing the informant rather than them. Unless you give people a role in this way they may adopt one which is not conducive to a good interview. If you appeal to them to let their relative answer alone (the appeal to be made very sweetly) it is often obeyed, especially if you explain the chance by which their relative come into the sample.

### Odd Reactions to Overcome

#### Exaggeration

Occasionally an informant may want to present a false image to you. If it is natural for him to present a front in this way when he meets strangers then it will be natural for him to want to defend himself in the same way when you, as a stranger approach him. For example, someone may hint at the wonderful job he has, and it can be cruel and hard for you to press for truth on the matter later in the interview without making him lose face.

Since you know the full range of your questioning when you meet, and he does not, you need to stem an informant's natural defence, before he goes far in fantasy. Get over to him as opportunities occur from the doorstep onwards, that you want his help, but also that you are not over impressed by any of his words. Take his response in a matter of fact way and show that what will impress you is to know exactly what he thinks and precise facts about some aspects of his life. Indicate it does not matter what kind of view he holds, so long as it is truly his own. We are in no position to censure informants.

#### Untruth

If you are told something which you sense may be untrue; in that the tone or speed at which it is said conveys an informant's fear of guilt in some way, try asking the question again using a linking phrase "Can I just check, I am uncertain whether I made clear what I meant to ask you" (and then repeat the question carefully). Often this will give the informant the opportunity to overcome any doubt about stating the truth. Your tone will help show that you do not take it to be a question of any special significance, and the inference of your own mishandling of the question can be used by the informant to excuse himself from not having been clear on what you wanted. A question which pressurises an informant in some way should prompt you to reiterate, just as you would be asked if a given reply were acceptable, that if what he has said reflects truly what he thinks, or if it is a fact so far as he knows it,

then it is the answer we want.

### Facetiousness

Another tone which may be adopted by a minority of those you wish to interview is one where, whilst the informant nominally agrees to the interview, he is answering tongue in cheek. In fact your manner whilst explaining the survey purpose, and the way you probe for exact replies during the interview, will often serve to school an informant to the degree of responsiveness required, but if it fails and the informant continues in the same vein try one of two lines: to a facetious response adopt a facetious tone yourself to show you appreciate his sense of humour but probe to sort out whether his response is genuine in spite of the tone or not. If you have an extreme case where you both know this, then you may find it necessary to tell the informant that if, really, he is not willing to take part, we would rather he said so than that both parties waste time. Such cases are rare, but worth a mention here to illustrate to you that we are not interested in nominal, only meaningful, responses. It is not necessary for your informants to respond in an utterly earnest fashion before one can say they are taking the matter seriously. Never expect informants to treat any subject matter in exactly the same degree of caution, frivolity or seriousness to match your own. On the other hand, do not let yourself be hoodwinked and left feeling you are wasting time on someone who just won't use the word no to you, although he had no intention of co-operating.

### Ending the Interview

Always thank the informant at the end of an interview. The public do not have to take part in surveys. Our enquiries are on worthwhile matters but the public are still doing us a favour when they agree to give us their time and express their views fully to us. Our aim is to leave them happy about the whole reason for the survey. Make sure everyone has the opportunity to ask further questions of you about the background of the survey, should they want to, before you leave the house. If they do not ask anything then you may find you can incorporate a little more about the value of the study and their participation as you give them you thanks.

Always leave the informant with permission to return to check any item, in case you discover an omission once you have left the house. Your conduct throughout the interview should be aimed at making the interview as pleasant an experience as possible for your informant, so that he is left feeling willing to co-operate in the future on any other survey.

### Confidentiality of Schedules

If on any survey an informant asks to see the schedule, either to read it during, or it could be before, the interview, or if he asks you to leave it for him to fill in, explain you want an opportunity to talk with him and you have notes for your own guidance but no forms as such for him to complete. DO NOT SHOW A SCHEDULE TO INFORMANTS AND NEVER LEAVE THEM IN A HOUSE AFTER AN INTERVIEW.

If you were to let the informants read schedules before answering the same it would make nonsense of some of our schemes of analysis; if the

informant is unaware of the form the questioning will take it is unlikely he will consider his replies, other than in the terms of reference of each specific question and in the light of foregoing replies. He has no knowledge of what is yet to come and it is unlikely there can be any fabrication that would not reveal itself before the end of the interview. But were he to know the question sequence beforehand, it is possible that the knowledge of the questions would condition, if not educate him, to devise a set of answers that more readily hung together. We would be getting considered answers to sequences of questions rather than spontaneous replies to individual items. We want all informants alike to be conditioned so far as possible, by nothing more than the questions that have been asked to date.

For anyone who asks you to show him a blank schedule, spelling out to an informant the reason for not doing so in the way above is not recommended. Simply indicate you have a check list to remind you what to talk about, but it is not a form of questions in the sense in which he means and go on to say you want the chance to talk with him and get his spontaneous replies.



## E SUMMARY ON METHOD

Throughout interviews you need to keep in mind that there are certain basic sources of error that can occur in communication by word of mouth to upset the collection of accurate data.

- (a) All of us when talking to others spend some energy or devote some of our attention to finding out the motives of the other person, and we try to see how these fit in with our own needs (which can include a desire on our part to create an impression on the other person). There is an evaluation and classification of what is being said on both sides, and so unconsciously, we often hear only what we wish to hear.
- (b) As adults we are experienced in communicating with others, and so we guess in advance what is being said to us and fail to hear what is actually said; possibly we guess incorrectly.
- (c) There may be a psychological barrier between the informant and the information we want. He may have little knowledge on the subject, he may have forgotten what we want to know about. If the question put to an informant touches on a subject painful to him to discuss, it may be that he will either not answer at all, saying or thinking he does not remember, or be evasive, or even if pressed give false information. There is no uniform guide to memory faults.
- (d) Language. One may meet foreign speaking people, people who speak heavy dialect or groups of the public who have some difficulty in self-expression, when you are wanting them to put their experience or reasons for action into words.

Some of the sources of error listed above apply to you as well as to the informant, item (b) in particular.

Error in communication can be cut by careful planning of the interviews, by using as we do, tested questions which have proved most meaningful to the majority of people.

This schedule of questions is a tool to help you collect accurate data. As with all tools it is important to know how to use the schedule. Practice until you can use it so well that you do not have to consider its use, but can concentrate on the object on which you work. Your object is your informant.

In order to have time to concentrate on your informant's reaction to questions, you must learn to scan a schedule before field work begins: consider the scope of each question, study where particular series of questions occur and which topic leads on from another. If you do this, and memorise in which circumstances questions do and do not apply, you will know enough about the form of the interview to conduct it smoothly in the field. Some interviewers find reading a schedule out loud is one way of learning its form.

Realise there is a plan to the sequence of topics introduced into an

interview. Questions that tax the informant tend to be left until a point in the interview by which we assume a good relationship will have been built up between you and the informant.

Some topics may be re-introduced in a different way at a later stage in an interview to help us gauge the informant's opinion more accurately. Methodical handling of the questions, progressing by number is essential, but it must never seem wooden to your informant. Know your schedule until the questions come over as spontaneous speech.

#### Question order and circumstances in which it may/may not be changed

Often an informant may mention an area you want to question him about later in the interview. If he does this, indicate that what he is telling you is something in which you are interested and about which you would like to know more later on. This will often enable you to get on with current questioning. When you then reach the appropriate questions you can preface them with the comment "I think you mentioned something about this earlier" and then go on to the actual questions. So long as acknowledgement of past comments is made in this way informants will not feel you are disregarding anything they say.

When you do come to the actual question on no account put down what the informant formerly said; to do so would be to forward code replies and the cases where precise questions are answered before asked are few and far between. When your actual question is put the informant may wish to make a different point.

If earlier in the interview you failed to stop the informant from talking about the later issue and if he expressed himself at length on it because it was something about which he felt strongly, you should note in the margin where this happened (quoting what he said). Then, at the appropriate place in the interview try the gambit,

"You did tell me something about this. I was going  
ask (the question)"

Alternatively one could try,

"What was it you did say about (the question)"

If the question produces only a sparse reply which you cannot get him to clarify because he remembers having detailed it to you already, one needs to say something on the lines.

"You did mention something about this before of  
course, now what was it you said?"

as though you cannot remember it at all. If he will not or cannot be expected to repeat all he said, you must make a note in the margin to this effect and give the earlier question number so that coders can refer back to his response.

If the data to carry forward is factual and if, for example, the informant has told you during the interview, he is married or has children, you should still use the check form of question.

"I believe you told me, but may I check (question)"

This form of checking is necessary because the informant may be answering on different premises. Someone may refer to his children and include those no longer at home whereas we may be wanting to know only the number living at this address.

Circumstances in which responses to earlier questions may/may not be changed

You do not go back and change or query the answer already given to an OPINION question should the informant contradict himself on a later question - not even if asked to do so by the informant. At some stage of the interview he may want to change his view as a result of the line of questioning you have pursued since the time of asking him the earlier question. Note where, and how, he asks for a reversal of response, if he does, but leave the original reply intact.

On the other hand you must check for consistency in FACTUAL questions. You need a retentive mind throughout the interview. As you come to ask subsequent questions, remember what has gone before, both in answer to your factual questioning and from incidental conversation with the informant. If you feel there is a contradiction in his factual responses, it is your job to reconcile the answers.

Do this by suggesting that you (the interviewer) may not have made a point clear and ask if you can go back over it. The informant may answer in a different way this time because he had genuinely misunderstood the question the first time. Or if, at the earlier stage of the interview, he had felt disinclined to give you precise data (without indicating this was the case) your reference back to the question, in terms of there being an error on your part, will give him the opportunity to amend an answer without loss of face.

DEPRESSION SURVEY

INTERVIEWERS' MANUAL NO. 2:  
SPECIFIC INTERVIEWING TECHNIQUES\*

\* This manual was based on the work of Atkinson (1971) and several other authors who are acknowledged in the method section.

## INTRODUCTION

This manual defines the approach which must be taken with each of the sections present in the overall interview schedule. Several of the sections are structured: that is, their format is strictly defined and the questions must not be altered either in terms of their order or in terms of their form. Other sections are semi-structured: that is, they involve questioning about defined areas of the subject's life in a systematic free-flowing fashion. The defining characteristics of the relevant areas and the nature of allowable prompt questions will be outlined in this manual.

Before examining the individual sections of the interview schedule in detail, it should be noted that the order in which they are discussed represents one of the two orders which will be used in the survey. Alternative orderings are being used to allow the determination of interviewer and interviewee order response biases\*. The different versions of the schedule will be utilized with samples selected using the same process and, therefore, they represent replicated samples. Although the ordering of the sections vary this should not alter the approach to be used.

### 1 CALL RECORD

This is the front page of the interview and is essentially a summary of the status of the interview. This should be filled in after each contact or attempted contact with the selected person.

SAMPLE NUMBER \_\_\_\_\_

Each person is defined by a five digit number which will be regarded as their sample number. Column I specifies the number of the interviewer

- 1 C. KNUSSSEN
- 2 S. HAMILTON
- 3 B. KELLY
- 4 M. O'DONNELL
- 5 F. PHIPPS
- 6 J. BOSWELL

\* Procedure discontinued after pilot phase.  
Column II specifies which ordering of the questions to be used i.e.

0 = the "depression" questions appear before  
"life events" questions

1 = the "life events" questions appear before  
the "depression"

Column III, IV, V, specify the number of the person in the particular sub-sample of a particular interviewer i.e. each interviewer will have two sub-samples (one for each ordering) with people numbered 1 to n.

Each call record allows space for information pertaining to six calls. These six calls will be regarded as the minimum number of attempts which must be made before this particular person is designated as non-contactable.

#### Day of the Week

The appropriate number used for designating the day of contact should be entered in the first row i.e.

MONDAY	1
TUESDAY	2
WEDNESDAY	3
THURSDAY	4
FRIDAY	5

#### Date of Contact

This is of considerable importance to any epidemiological examination of mood. The date and the number of the month should be entered in the conventional fashion.

#### Time of Call

The time of call must be entered as this allows predictions about the best time for call backs to be made.

#### Start/Finish

This information should be provided as accurately as possible to allow the estimation of the average length of interview. This information is necessary to allow determination of the appropriate magnitude of the final sample. The time period involved should include the time required for completion of the post-interview schedules.

#### Interviewer's Initials

The interviewer must sign the call record after each call.

#### Results

In this row the appropriate RESULTS CODE should be placed. The key defines the codes which should be entered i.e.

- 01 Completed interview.
- 02 Respondent absent i.e. someone at home but not respondent.

- 03 Refusal.
- 04 House vacant i.e. not lived in.
- 05 House demolished.
- 06 Respondent permanently moved away.
- 07 Respondent temporarily moved away i.e. employment elsewhere, student, armed forces, etc.
- 08 No one at home.
- 09 Incomplete interview.
- 10 Other: no interview for some reason other than above.

### Final Status Code

The same codes as above should be used to clarify the final status of the interview schedule. If a successful interview is completed after the first call then the RESULTS CODE and the FINAL STATUS CODE will be identical. However, in a situation where the successful interview does not take place to the third call, the codes will not be identical until that third call. In this case columns 1 and 2 of the FINAL STATUS CODE will be blank.

## 2 DEMOGRAPHIC DATA

### Sex

Hopefully, this should be self evident.

### Age

For age a precise figure is required and you must ask this in the form "What age were you last birthday?" Without the use of this definition throughout, elderly peoples' ages would invariably be given inaccurately.

### Household

We need to lay down rules as to which people should be included as members of any household.

A household is a group of people who live regularly at the address given on the sample list, and who are all catered for, for at least one meal a day, by the same person.

Any other individual or group of individuals in the same dwelling who has different catering arrangements forms a separate household.

### Points to Remember

The two important facts to be established are:-

1. that all persons in a household are catered for by the same person.

BEING CATERED FOR is defined as:

- (a) having at least one meal a day, when in residence.

2. that all persons live there regularly.

LIVING THERE REGULARLY means:-

- (b) for relatives and other persons, they are included if they spend at least 4 nights a week in this household - even if they are regularly away from it for the remaining 3 or less nights. It is four nights every week which count for household membership and not an average of four nights per week over a period of time.
- (c) for married persons, they are included if they do return to their spouse at this household at least one night a week. This covers spouses who work away from home and can only return home week-ends.

Clearly you will meet cases which the above criteria do not satisfy. You must know what to do with other people who are mentioned as absentees or visitors. You must decide from the following whether or not to include them.

In addition to people who satisfy conditions (a) and (b) or (a) and (c) you must count as members, i.e.

#### INCLUDE IN THE HOUSEHOLD:

- (d) people on holiday, away on a rare business trip or in hospital at time of interview who are normally in the household {satisfying points (a) and (b) or (a) and (c)}, unless they have been away for more than six months (if it is precisely 6 months on the day of interview include them).
- (e) fishermen and any merchant seamen whose only shore address this is and who normally spend up to and including, but not more than, six weeks at sea.
- (f) children under 16 away at boarding school or other schools. (This is an exception to point (g) below).
- (g) members of the family of 16 years and over who live away from home and who only come home for holidays (this will cover persons away at school, or college as well as those working away from home).



- (h) members of the Forces (and Merchant Navy) stationed permanently away from home. (Of course if they were stationed permanently at home then they would be included in the household).
- (i) temporary members of the household. Relatives who do not normally live there, and persons home on leave from abroad etc. They would only be included in the household if they had been there for more than six months prior to the date of interview.

(NOTE: This six months rule applies only to temporary members of the household. Anyone who has joined the household within the last six months as a regular member, that is someone with no intention of leaving the household is no longer considered a temporary member of the household).

When asking for HOUSEHOLD COMPOSITION, it is simplest to ask: "How many people live here regularly, who are catered for by the same person as yourself?"

Both the concepts of residence and having meals must be mentioned. Even with the use of this exact question further probing may be necessary when the informant says, for example, she has boarders or relatives staying there.

Never confuse our definition of the terms boarder and lodger.

- (a) Boarders. These are members of the household, who are not related by blood or marriage to any other members of the household. They receive accommodation for at least four nights a week, and, when they are in residence, at least one meal per day from the housewife, in return for payment. These are called boarders. So that our definitions are consistent, they are included in the landlord's household ~~unless~~ they are married and return home to their spouse at least one night every week. (see point (c) by which we would count them, theoretically in a household elsewhere).
- (b) Lodgers. By a lodger we mean someone who caters for himself and is therefore not a member of this household because he forms a separate household.

You will see from the above we are establishing the number of people living in a private household.

NOTE:

- (a) A household can consist of only one person.
- (b) Members of the household need not be related by blood or marriage.

If you follow the above definition you will have excluded those persons who form the floating population by living in hotels, hostels or are of no fixed abode. If your informant is the proprietor or belongs to the proprietor's household of an hotel or commercial boarding house or hostel, the guests are not included in the proprietor's (i.e. informant's) household. We define a commercial boarding house as one which caters for AT LEAST FOUR BOARDERS at the time of the interview. In smaller establishments (i.e. any household with three or less boarders) the boarders are included in the landlord's household.

Marital Status

We require to know whether our subject is married, widowed, single or divorced. Divorced persons and those separated from their spouses for over two years, are coded as divorced. When asking for marital status it is necessary to mention all the alternatives included on the schedule. This should be done in the form of a running prompt, i.e. "Are you married, single, widowed or divorced?"

Employment Status

In this section we are concerned with whether our informant is employed or otherwise and the nature of their employment. The following definitions should be applied:

WORKING

By working we mean gainful employment, that is to say paid employment. Gainful employment results either in

- (a) wages or salary in return for working for an employer for more than ten hours a week at the time of interview. (Anyone working for ten hours or less a week is counted as not working.)
- (b) income, as a result of being self-employed for more than ten hours a week. A self-employed person is one whose main responsibility for his work is to himself.

A distinction is drawn between those people who are not working (i.e. retired people, housewives, full-time students) and those who are unemployed.

UNEMPLOYED is defined as not falling into categories (a) and (b) above, but actively seeking work. One can actively seek work by registering at a Labour Exchange or other employment agencies, or by answering advertisements or advertising for jobs.

NOT WORKING A person who is not employed and does not intend to apply for work is counted as not working.

CODING In order to code a person who is gainfully employed (i.e. in paid work) either Full or Part-Time you must establish whether he works as follows:

FULL time is over 30 hours per week

PART time is over 10 hours per week and up to and including 30 hours.

For persons receiving full time education, i.e. children and students, code not working, and disregard any part time (holiday and paper round) jobs which they may have.

Note that any person serving an apprenticeship who may be receiving part time education (day release or sandwich course) is to be coded WORKING if he is paid a wage for his apprenticeship.

THE LENGTH OF THE WORKING WEEK (over thirty hours or over ten and up to and including thirty hours) is decided on the number of hours as follows:

- (a) A basic working week agreed between employer and the employee.
- (b) The usual working week for a self-employed person.
- (c) The number of hours worked in the seven days prior to the day of the interview for a casual worker.
- (d) If a person has two or more jobs, the foregoing criteria about hours should be applied to each job. The total number of hours spent on all the jobs should be taken as the length of that person's working week.
- (e) A person on holiday or on strike, or not actually employed for any reason beyond his control whilst under agreement to work counts as working.
- (f) Persons on sick leave who have a job kept open for them, and to which they can return when they have recovered, would be coded as working full time or part time according to which category they are in when they are fit.

The NOT WORKING code is used not only for persons who have no gainful employment and are not seeking any, but also for those

- (a) persons who work up to and including 10 hours per week.
- (b) any person who says he is sick but has to seek a new job or re-employment with his former firm (not simply return to job kept open for him) when he recovers.

When asking whether the informant is WORKING or not, it is necessary to explain our definition of working:

"Are you in paid employment at present?"

If the answer is yes then ask:

"Is that full time i.e. over 30 hours per week or not?"

If not ask:

"Is it part time, i.e. over 10 and up to and including 30 hours?"

Remember it is paid employment that we are interested in, and only current employment is to be included. It is incorrect to ask only

"Are you working?"

Since housewives consider they work in the home and voluntary workers may consider that they work for charity.

We want to have details of the informant's occupation and the industry, trade or profession with which he is associated. We also want similar data for the Head of the Household. This provides us with the socio-economic or social class grouping to which the informant and the H.O.H. belong. Do not use the phrase socio-economic group or social class to the informant since it could lead to some misunderstanding on the part of the informant.

We need to know it as one indication of the person's economic and social standing. Naturally an occupation must be related to education, income and other environmental factors of the person concerned, and this is why you will be asking other classificatory details besides occupation.

Explain why you want to know the informant's or H.O.H.'s occupations in general terms expressing the need for background data.

#### Occupation

In order to classify occupations we use the Registrar General's Classification of Occupations as used on the National Census of the Population. In this classification there are some two hundred main unit groups of types of occupation, and further breakdown indicating the socio-economic groups to which each occupation has been assigned, and in most cases the degree of skill and/or job qualifications required to do them. It also shows for each job whether the work involved is mainly of a manual type or not.

In order to allow for such factors the classification consists in all of some thirty thousand specific job descriptions which are obviously far too many for you to learn off by heart. For example there are some three hundred different codes from which to choose if a person tells you he is "a cleaner" ranging from whether he cleans airgates on drainage, or copper in a brewery, or trays in a bakery, to pick out but three of the three hundred.

There are further distinctions to be made according to whether persons are self employed, or above works' foreman level or management or foreman of labourer, or labourer with a degree of skill.

In the space provided, record the informant's name for his job and a clear description of the kind of work he does; the nature of the operation performed unless it is self evident from the job title, e.g. dentist, bricklayer or carpenter. It is not possible to devise any one question that will fit in all cases and the interviewer should adapt her approach to suit the situation.

If the informant's description of his/her occupation is too vague or too technical,

"What kind of work is that?"

or

"What do you actually do in that job?"

are the most useful probes; but such questions are clearly absurd when the occupation is self explanatory, e.g. dentist or where the technical term for the job is well known, e.g. lino-type operator. On the other hand the interviewer should realise that certain terms that may present no difficulty to her because she is familiar with local industries may be incomprehensible to the staff at H.Q. and should therefore be explained. There are also certain occupations which, though they may appear to be self explanatory, are not precise enough to permit accurate classification. See example below.

Two other types of information are relevant

- (1) Is the person employed or self employed and whether he is a manager/superintendent or self employed.
- (2) The number of people employed at his establishment.

If the person does not fall into the above categories, the default code 0 should be entered in the space provided for 'number in the establishment'.

- (1) Whether the person is self employed, i.e. working on own account or as an employee, is straightforward since most persons know which kind of National Insurance contribution they have to pay (i.e. either all or part of the stamp). Not all directors of firms are employees, i.e.

when a person turns his private business into a Company he becomes an employee within it and should be coded as such. Note the codes tell us whether or not a person is self employed. If he is self employed we still need a clear job description since a self employed person can be anything from a working craftsman to a professional man or an administrator of office and works' staff.

- (2) This is an item we want asked for in this particular form, again in order to code occupation as defined by the Registrar General. For any person who has indicated that he is in a managerial position, providing that he is self employed and/or a manager or superintendent, i.e. above foreman level, ask  
"The number of people employed, in all capacities, in the establishment where you work".

#### Head of Household

These items are also required for the H.O.H. where our respondent is not the H.O.H. The latter is defined for our purposes in the following manner:

The Head of the Household must be a member of the household. The Head of the Household is, in order of precedence, the husband of the person, or the person who either

- (a) Owns the household accommodation
- (b) Is legally responsible for the rent or the accommodation
- (c) Has the household accommodation as an emolument of prerequisite
- (d) Has the household accommodation by virtue of some relationship to the owner in cases where the owner or lessee is not a member of the household.

#### Points to Remember

Throughout the schedules the term Head of Household will be referred to as H.O.H.

- (e) The important fact to establish is in whose name the property is owned or rented. To obtain this information you should normally ask "In whose name is this house/flat owned or rented?" DO NOT ask "Who is responsible for paying the rent?" since the person who pays out the money may not be responsible for the house in name

If your informant is living in only part of the house, i.e. if there is more than one household at the address, you must make the point of the question clear by saying "For the part of the house in which you live (with your husband and your mother-in-law, etc.) may I know in whose name it is owned or rented?"

(f) When the accomodation is in the name of a person who is not a member of the household by our definition, you must establish another H.O.H. from within the household, taking the person within it who stands responsible for the house in the other person's absence. For example if you are told the house is in the name of a husband who is stationed away from home, he is not a member of the household and in this case you can take as the H.O.H. his wife who is living there.

(g) So long as the husband is resident he takes precedence over the wife in being H.O.H. This means if you have a married couple living together, even if the wife owns the property or has her name on the rent book, you count on her husband as the H.O.H.

Where the household consists only of mother, father and children under 18 years, no questions as to who is the H.O.H. need be asked, since, by the above rule, you take the father as the H.O.H. In all cases where there is any other adult (except boarders) living in the household you must ask "in whose name etc" since the house could be in the name of one of the other adults.

(h) When two persons of different sex have an equal claim to be H.O.H., i.e. if you are told ownership is joint, then take the male of the two to be the H.O.H.

(i) When two persons of the same sex have equal claim to be H.O.H., i.e. if you are told ownership is joint, then you take the elder of the two as H.O.H.

#### JOB DETAILS

It is essential to get a good description of the H.O.H.'s occupation as well as the informant's. A wife may be vague on the name for her husband's (H.O.H.'s) job, or quote an ambiguous title, yet she can usually give you a graphic description of what is involved in his job if you ask for it.

- (1) An informant may say he is a Civil Servant. The first question to ask in this case is "What is your grade?" No further details are necessary for 'occupation' unless the informant cannot tell you the grade, in which case "What do you actually do?" should be asked.
- (2) It is not enough for somebody to answer 'Engineer' as this can cover anything from a fully qualified professional employee with either a degree or an equivalent qualification to a semi-skilled machine operator. When an informant answers in vague terms like this, we want to know his full title, and whether he is qualified professionally, e.g. "Professionally qualified - civil engineer (or electrical engineer)". This means that the term 'Engineer' should never appear on its own unless the informant cannot answer about another person's occupation any more exactly than this.

(3) Other vague job descriptions are 'Machinist', 'Technician', 'Miner', 'Collector' and 'Laboratory Assistant'.

- (a) Machinist: There are at least 1,000 different types of machinist and the socio-economic grouping and/or social class depends on the type of machinist. There are 'Machine Grinders', 'Machine Cutters', 'Machine Castors', 'Machine Drillers' etc and obviously we want to know which type of machinist the informant is.
- (b) Technician: There term technician also has a wide range of usage and can be applied over almost the whole range of the socio-economic groups and social class. Here we want to know whether the informant is e.g. 'Surgical Technician' or a 'Radar Technician', a 'Cine Technician' or any other of the approximately forty different types of technical worker ranging from the semi-professional type of worker down to a semi-skilled non-manual type of worker such as an electrical fitter or a generating station attendant.
- (c) Miners: There are several different types of miners. Some work at the coal face, underground and some are surface workers and the term miner can include 'Coal Cutters', 'Trimmers' (coal or coal mine) 'Haulage Hand' etc. We always want to know his full job description and whether or not he works at the coal face.
- (d) Collectors: This term covers nearly 80 different types of workers. For example there are 'Debt', 'Rent', 'Meter', 'Rate', and 'Salvage Collectors' or 'Collectorsof Customs and Excise', etc, etc to name just a few.
- (e) Laboratory Assistant: Here again this occupation is used to describe someone who merely washes the utensils and instruments and cleans the laboratory generally. Whenever this occupation is given, describe fully the duties of the informant and any technical qualifications the informant may possess for this work.

As you can see these general headings like 'machinist' or 'miner' etc are not sufficient in themselves and the fullest description should be given when such occupations are mentioned by the informant. You should consider whether what you have elicited gives you and coders a clear idea of how the informant spends a typical day, not minute by minute, but in terms of the skills he is using both mental and physical. Never suggest the type of job to the informant, always ask them to describe it in their own words.



## JOB DETAILS continued

### Two or more jobs

If the informant and/or H.O.H. has two or more jobs, details of the most remunerative should be recorded. (This differs from rule on no. of hours worked where in order to arrive at total no. of hours worked both jobs are taken into consideration).

### Unemployed persons

If a person is unemployed at the time of interview, i.e. if he is actively seeking work, record under occupation the number of months he has been unemployed followed by full details of his last occupation and industry.

### Retired persons

Anyone beyond the normal retirement age who is no longer in paid employment will have been coded as Not Working and in the space reserved for details of paid occupation give the status of the person you have interviewed, i.e. 'retired' together with a general description of his last main paid job. The "main" job is the one which the informant would consider his "career" job or in some cases whole work life-time job. Beside job details indicate person is retired (to explain the not working code which you will have entered).

### Other persons

Other person coded Not Working (i.e. no paid work or only up to 10 hours) but not of retirement age should havea their "status" recorded across occupation space. e.g. "housewife" or "housewife with only 5 hours private dress-making per week" or "unoccupied - private means - never worked".

## TYPES OF DWELLING

We require to know something about the dwelling in which our informant lives. The following categories should be used.

Whole house - detached	1
- semi-detached (inc. prefab)	2
- terrace	3
Flat/Maisonette - self contained	4
Rooms	5
Caravan	6
Other types of dwelling (specify)	7

A house which is the end house of a terrace is to be coded as a terrace house. The distinction between flat, self-contained and rooms is that the former has all its rooms contained behind one door, whereas a dwelling classed as rooms does not.

### Religion

People do not have to be regular attenders at church/chapel/synagogue to be regarded as being a religious affiliate. Please record as stated below.

Protestant	1	No affiliate	4
Catholic	2	Other	5
Jewish	3		

### General Points

It is bad to use any phrase or tone of voice that suggests you are apologetic about asking for data in an interview, as this suggests you doubt the informant's willingness to answer. (Too many "may I ask you's" may bring a retort that you may not). However, there is a use for polite phrases from time to time, not voiced diffidently, but in order to show that you are not taking it wholly for granted that this is a straight question and answer routine. Remember throughout the need to keep the informant at ease whilst he is answering classificatory questions. If he is clear about the necessity for us to have a picture of the person who has expressed views on a subject then you will find him willing and helpful in giving facts about himself.

Just as on any other question on a schedule if an informant says he prefers not to answer a classification question there can be no compulsion for him to do so. Do explain if he asks whether he must give a reply that he is not forced to but go on to explain why the question is relevant. Most often this will lead to the informant giving you the answer. Remember that an interview on which your informant refused to complete the classification section wholly would be useless to us since we would not be able to compare his responses on the subject matter of the survey with anyone else's responses since we would not know whether we were comparing like with like person or not.

#### SUMMARY OF POINTS FROM THE CLASSIFICATION SECTION

(a) CLASSIFICATION IS BACKGROUND DATA ON THE INFORMANT; factors which it is held generally influence one's outlook on life and one's way of life. All surveys aim to discover which types of people behave or think in which ways and the researcher wants to be able to see how the different sections of the community are affected.

EXPLAIN the purpose of classification questions in simple words to the informant. Tell him of our need to relate given views or facts on the subject matter of the survey to the kinds of people who have been interviewed. Names do not help us on this. We do not use names in our report. It is sensible for us to take into account the ages, size of family and like factors which describe to us the person who has been interviewed.

(b) AVOID use of JARGON, i.e. if our terms for items are used without explanation they will be misconstrued, since we have attached uncommon, specific definitions to them.

(c) LEARN definitions and then APPLY them clearly to each interview.

### 3 SELF-ADMINISTRATION SECTION

The self-administration section is separate from the main interview schedule. On the front cover the person code number must be entered immediately after interview or at an appropriate time after its completion. Any comments regarding reading difficulties should be stated. The principal difference between self-completion and the rest of your questioning routine is that for self-completion you must get closer to your informant, if possible at a table. Ensure he has his reading glasses on if he uses them. Often you need to read out the items for them to be understood. Do this in as neutral a manner as possible. Revert to your normal interviewing position of sitting face to face, once the self-completion section has been dealt with.

Two versions of the self-administration section will be available. The one which is to be used with female respondents incorporates a scale looking at menopausal symptoms.

The titles used below for the scales should not be used with the interviewee. They are used only to inform the interviewer about the value of the scales.

#### Depressive Symptoms

The subject is required to circle the number attached which gives the appropriate indication of the frequency of the depressive symptoms. The instructions to be used are: "Below is a list of the ways you might have felt or behaved. Please tell me how often you have felt this way during the past week." The point to emphasise is that we are interested in the previous seven days, no other period. Try to keep the pace reasonably fast. If the subject is spending too long in deliberation and states that he has difficulty choosing among the alternatives, make it clear that he should choose the one which comes nearest to what happened. The non-response codes to be used are:

DON'T KNOW	7
INAPPLICABLE	8
REFUSE TO ANSWER	9

#### Anxiety Symptoms

There are eight symptoms which are laid out in the same format as the depression questions. The same general principles apply as above - transference from one set of symptoms to another should be fairly straight-forward given the similarity of form. However, the point should be made that we are interested in whether they occurred without good reason. e.g. physical reasons.

### Adjective Checklist

This is a measure of mood at a particular time. The instructions you should give are: "Below you will find words which describe different kinds of moods and feelings. Circle the words which describe how you feel now today. Some of the words may sound alike, but we want to know all the words which describe your feelings. Work rapidly and circle all of the words which describe how you feel today." The points underlined should be emphasized. If your subject is unsure of the meanings of a few words you may explain these to him briefly, in as neutral a fashion as possible. If severe difficulty in comprehension is encountered this section should be discontinued.

RESPONSE CODES:	UNCIRCLED	0	DOES NOT COMPREHEND	6
	CIRCLED	1	DO NOT KNOW	7

### Eysenck Personality Questionnaire

The instructions to be used in this scale are: "Please answer each question by putting a circle around the 'YES' or the 'NO' following the question. There are no right or wrong answers, and no trick questions. Work quickly and do not think too long about the exact meaning of the questions."

The points underlined should be emphasized. If the interviewee attempts to get your interpretation of the question, resist firmly but politely.

RESPONSE CODES:	YES	0	DOES NOT COMPREHEND	6
	NO	1	DO NOT KNOW	7
			INAPPLICABLE	8
			REFUSE TO ANSWER	9

### Locus of Control Scale

The structure of this scale and the instructions used are similar to those used in the Eysenck scale. The principle difference is that this scale involves statements which have to be answered as 'TRUE' or 'FALSE' rather than questions. The instructions to be used are: "Below is a list of statements. Please say what you feel by putting a circle round the 'TRUE' or the 'FALSE' following the question. Work quickly and do not think too long about the exact meaning of the statement."

You may find that the respondent will endeavour to obtain your opinion on the statements. This should be firmly but politely resisted, e.g. "We are really interested to find out what you feel about this."

### Change of Life Schedule

This questionnaire must be given to all female subjects. The instructions should be read out if the person has difficulty in comprehending them. If there are queries about the relevance from women who are not of that age range usually thought to be susceptible

to the menopause, briefly indicate that it is important for us to know how common the symptoms are in non-menopausal women so that we can determine the effect of the menopause more accurately.

The self-administration questionnaire should be collected in immediately after completion.

## LIFE EVENTS SCHEDULE

### General Points

The life events section of the survey is the least structured. Your aim should be to create as free flowing a situation as possible while minimising irrelevancies. In a free flowing interview which involves the previous twelve months informants may have difficulty in remembering required facts. Frequently subjects will state that they "don't know" initially, but careful probing will enable you to elicit accurate responses.

We know from Bartlett's research on 'Effort after meaning' that people are not out to falsify data, but they do have their own biases in the recollection of events, tending to remember only things they want to remember or deem it important to remember. Unless we adopt a specific procedure when there are memory difficulties probing could very easily become prompting, i.e. suggesting. If you suggest to your informant a possible date of a life event, in what to him is a stress situation, (i.e. if he is trying to remember but cannot) he may grasp at your suggestion and accept it, because trying to remember has become a strain and he has to stop thinking about it. One way of dealing with the tendency to "grasp at your suggestion" is always to pose equally balanced alternatives when suggesting possible dates, e.g. if the respondent says "early in the year," the interviewer must not say "January?" but "Before or after Easter?" etc.

The interviewer should not necessarily accept the reply he gets. He should use the tone of the reply as a cue for further questioning. If there is difficulty in dating events the interviewer should ask directly whether there is anything the respondent can think of that may help date the events he mentions.

### Introduction of Life Events Section

You should introduce this section by stating that our concern is with things which have happened to the subject, family and friends in the last twelve months.

It is usually necessary to repeat these points, at least during the early stages of questioning about events.

"The last 12 months," preferably stating the month (e.g. the 12 months since last July), is better than "the last year" as this can be confused with the calendar year. However, the list of family, friends etc., for whom the events may be relevant should not be too rigidly conveyed as the categories of "other persons who can be relevant" vary for different events. It is worth letting subjects be over-inclusive in talking about events and not cutting them off too hastily.

After the introduction the interviewer should introduce each category of life events e.g. health, work, education, etc., by stating e.g. "I would like to ask you about things which have been happening at work."

Every event must be enquired for unless clearly inapplicable. e.g. "education" events for people not involved in any educational programme. Enquire for the occurrence of the life events in the previous twelve months. If an event has occurred, elucidate the circumstances surrounding its occurrence and its impact on the subjects. Continue probing for information until the full nature of the event is clear.

Points Concerning the Dating of Events (this section is also relevant to the dating of onset)

(1) Relevance of Event

The interviewer should first establish whether an event is suitable for inclusion before trying to fix its exact date of occurrence so that unnecessary questioning can be avoided.

(2) Anchor Dates

If possible the interviewer should establish "anchor" dates from which events can be dated e.g. "Did so and so come before or after so and so?" The simplest way to do this is to use something like a Bank Holiday, a birthday or a firm's outing. If nothing like this seems relevant, already established dates (such as moving house) should be used. In any case it is good practice to cross check on the dates of the events obtained e.g. "Your son's accident did come after your daughter's engagement?" etc. Once such before-and-after training has been established, the interviewer should go on to try and date an event more securely. "About how long after....?" etc. It is essential not to "suggest" a date in such questioning. It is often helpful to establish first of all the day of the week on which the event occurred; it is usually remembered, and the date can then be narrowed down. The interviewer must be ready to recognise that respondents often cannot recall a date - there is a great deal of difference between helping recall a date (by suggesting associations and encouraging him to talk about the events) and forcing him to choose between dates about which he is unsure, e.g. "Was it two or four weeks afterwards?" Giving such alternative dates can be useful but it should be used carefully; it is advisable to check how sure the respondent is about his "choice." Most people will readily accept that for the purposes of the research the interviewer wishes to get an accurate date, but not at the cost of distraction. If a good deal of questioning about date is necessary, it is a good idea to make this point; in this way, it should prove relatively easy to get most respondent's on "your side" in trying to establish a difficult date.



### Coding of Life Events

The occurrence of each life event is completely coded in 11 rows:-

-----	1st
-----	2nd
-----	3rd
-----	4th
-----	5th
-----	6th
-----	7th
-----	8th
-----	9th
-----	10th
-----	11th

#### Row 1 and 2 Life Event Number

In the first and second row of the set the rater records the identifying number of the life event that has occurred.

#### Row 3,4,5, Time of Occurrence of Life Event

In the 3rd row the rater records whether the events occurred before the onset of the subject's, symptoms or after the onset.

If the event occurred and there has been no onset of symptoms, code 0.

If the event occurred before symptoms onset, code 1 in the 3rd row.

If the event occurred after the symptoms onset, code 2 in the 3rd row.

In the 4th and 5th row, the rater records the month in which the life event occurred. The event is dated by counting in blocks of one calendar month from the date of interview.

Number of months is as follows:

12	11	10	09	08	07	06	05	04	03	02	01	00
12 months						six						interview
before						months						
interview						before						
						interview						

Row 6 Independence or dependence of an event

In the 6th row the rater records the "independence" or "dependence" of the event.

By "Independence" is meant the likelihood that the event was not a consequence or potential consequence of illness.

By "dependence" is meant the likelihood that the event was a consequence or a potential consequence of illness.

The independence/dependence of an event is rated on the following 5 point scale and coded in the 6th row of the 11 row set.

If the event was almost certainly independent of the illness	Code 1 in the 6th row
--	-----------------------

If the event was <u>probably independent</u> of the illness	Code 2 in the 6th row
---	-----------------------

If the rater is <u>uncertain</u> as to the independence or dependence of an event	Code 3 in the 6th row
---	-----------------------

If the event is <u>probably dependent</u> on the illness	Code 4 in the 6th row
--	-----------------------

If the event is <u>almost certainly dependent</u> on the illness	Code 5 in the 6th row
--	-----------------------

Row 7 Control Over the Event

In the 7th row the rater assesses the degree to which the patient has choice or control over the initiation of the event.

Complete control	Code 1 in the 7th row
Considerable control	Code 2 in the 7th row
Some control	Code 3 in the 7th row
A little control	Code 4 in the 7th row
No control	Code 5 in the 7th row

Row 8 and 9 Subjective Impact of an Event

In the 8th and 9th row rater records the subjective impact of the event on the patient.

The subjective impact of an event is the degree of impact that the event had on the patient as rated from the patient's report of his feelings about the event and other circumstances indicating patient's reaction to the event.

The subjective impact of an event may be negative or positive or mixed. Therefore negative and positive impact should be assessed separately.

Subjective negative impact is assessed on the basis of a patient's report of upset, tension, worry, anger or disappointment and any other circumstance which indicate negative feelings. Subjective negative impact is recorded on the following 5 point scale.

Severe negative impact	Code 1 in the 8th row
Marked negative impact	Code 2 in the 8th row
Moderate negative impact	Code 3 in the 8th row
Mild negative impact	Code 4 in the 8th row
No negative impact	Code 5 in the 8th row

Subjective positive impact is assessed on the basis of a patient's report of happiness, pleasure, relaxation or lessening of stress and any other circumstances which indicate positive or pleasurable feelings. Subjective positive impact is recorded on the following 5 point scale.

Intense positive impact	Code 1 in the 9th row
Marked positive impact	Code 2 in the 9th row
Moderate positive impact	Code 3 in the 9th row
Mild positive impact	Code 4 in the 9th row
No positive impact	Code 5 in the 9th row

#### Row 10 and 11 Objective Impact of an Event

The objective impact of an event is the degree of impact the event would be expected to have on someone when its full nature and particular circumstances are taken into account. The measure is independent of the patient's subjective report of the impact of the event. However, the measure takes into account the particular circumstances of the patient and of the event which modify the objective impact of the event. These might include many factors such as previous experience of the event, its desirability, expectedness, support received after its occurrence as well as particular circumstances of the patient's life which might be expected to modify the event's impact and consequences.

Objective negative impact is recorded in the 10th row, using the same scale as for the "subjective negative impact" rating.

Objective positive impact is recorded in the 11th row, using the same scale as for the "subjective positive impact" rating.

## 5 QUESTIONS ABOUT EARLY LOSS

This small group of questions can be easily linked to the preceding section of life events. You can say to your informant that up to now you've been interested in events which occurred in the last twelve months, but now you would like to consider a few things which happened before this.

The record sheet provides an account of all the questions to be asked and the information required. We are interested in the period up to twelve months before the interview, i.e. the period not covered by the "life events" section. The principles relating to probing and dating outlined above should be borne in mind.

### LIFE EVENTS SCHEDULE

#### WORK

01. Change to a different line of work, e.g. begin work for first time after a substantial time lapse or change to a different line of work with a new employer.
02. Substantial change in work conditions, e.g. change to similar work in a new firm or another department, new boss, new colleagues, big reorganisation, or lateral change in duties and/or responsibilities. (Includes housewives who cease permanent employment and return to housekeeping as well as married women and students who begin a temporary job).
03. Substantial changes in work hours. (Includes taking a second job).
04. Onset of troubles or disagreement with boss, supervisor or co-workers.
05. Promotion.
06. Demotion.
07. Fired.
08. Retirement.
09. Unemployed for one month or more (includes those laid off, fired or who quit and are unable to find another job. Do not code if job was intended to be temporary).
10. Failure of business (code only for owners and management).

#### EDUCATION

11. Begin full time or half time education (including training programme - but does not include correspondence course, or light night school course).

12. Change in schools.
13. Cease full time education, e.g. graduate or drop out.
14. Important academic failure.
15. Prepare for or take an important exam (also include important thesis). (Do not include routine or semester exams unless:-
  - a) it is first set of college exams;
  - b) continuing at school in jeopardy, in the case of failure).

#### FINANCIAL

16. Moderate financial difficulties. (include significant new difficulties not sufficient to be called major, in addition to chronic difficulties which have worsened somewhat, e.g. increased expenses). Trouble from bill collectors or need to take a second job is indication of this.
17. Major financial difficulties - much worse off than usual, e.g. bankruptcy, very heavy debts or expenses.
18. Substantial improvement in finances (go off welfare, spouse of patient takes additional job, inheritance etc).

#### HEALTH

19. Major personal physical illness or injury. (hospitalisation, surgery, or illness requiring one month's absence from work).
20. Major physical or emotional illness of close family not leading to death.
21. Wanted pregnancy (code discovery of).
22. Unwanted pregnancy (code discovery of).
23. Miscarriage, stillbirth, or abortion.
24. Birth or live child.
25. Menopause (code only for time of onset of major menstrual change - irregularities, infrequent etc).

#### BEREAVEMENT

26. Death of a close friend or significant relative (e.g. favourite aunt, grandmother).
27. Death of a close family member (parent, sibling or other close person, e.g. fiancé).
28. Death of a child.
29. Death of a spouse/partner.

30. Loss or robbery of personally valuable object (e.g. wedding ring, jewellery, sentimental object, money).

#### RELOCATION

31. Move within same city.
32. Move to another city (for student, record only initial or terminal move, not vacation).
33. Move to another country.

#### DATING

34. Become engaged, or begin serious relationship.
35. Break engagement.
36. Cease steady dating of three months or more (include termination of established homosexual relationship).
37. Increase in arguments or difficulties with fiancée, long term steady date, or homosexual partner. (Include major sexual conflicts).

#### LEGAL

38. Minor violations not leading to court appearance, e.g. parking ticket, speeding etc. (Include students who have discipline difficulties with school authorities).
39. More important violation leading to court appearance (Includes losing driver's licence).
40. Jail sentence.
41. Lawsuit with legal action.
42. Legal problems of close family member.

#### FAMILY AND SOCIAL

43. Birth of child (code for father) or adoption of child.
44. New person other than infant moves into household (e.g. oldster, relative, lodger). (Includes children returning from foster homes).
45. Child engaged.
46. Child married (with approval).
47. Child married (without approval).
48. Child leaves home for other reasons (e.g. college or institution).

49. Son enlists or is drafted into Armed Forces.
50. Increase in arguments or problems with resident family member. (Includes behavioural problems with children. Difficulties not to be coded if they are typical of usual parent-child strife, e.g. mildly rebellious teenager). (Code under emotional illness if hospitalisation or institutionalisation results).
51. Serious argument with non-resident, close family member, in-laws, neighbour or friend.
52. Marked improvement in relationship with resident or non-resident family member.
53. Separation from significant person (e.g. close friend moves away; therapist, social worker leaves).
54. Marital problems of close family member (e.g. child separated from spouse, parents' divorce).

#### MARITAL

55. Marriage.
56. Increase in arguments with spouse. (Difficult to assess - can often be pinpointed by especially serious or violent argument).
57. Marital separation of one month or more not due to argument.
58. Marital separation due to argument. (do not code increase in arguments as well).
59. Extramarital affair of partner.
60. Begin extramarital affair.
61. Marked improvement in relationship with spouse.
62. Marital reconciliation.
63. Divorce (actual decree).
64. Other. Include any event which cannot be classified as life event under any of the above categories but may be considered a life event because:
  - 1) It is clearly an event of major importance to patient
  - and 2) It resulted in major changes in patient's work, social or family circles, living conditions, health or status.

## 6 DATING OF SYMPTOM ONSET

The general principles outlined above in regard to the dating of events apply equally to the dating of symptom onset. Given the techniques we are applying, you may be confronted by informants who fall into one of three broad categories.

1. People who are not depressed at the time of the interview and who have not been depressed in the past twelve months.
2. People who are not depressed at the time of interview but who have been depressed at some time during the twelve months prior to the interview.
3. People who are depressed at the time of interview.

You must determine which of these broad categories your informant falls into and then adopt different techniques to determine the onset of symptoms. Dating involves the use of 'Change Points'. Change Points are defined as clear changes in depressive symptomatology, either from "normality" or from an already established pattern of symptoms that had been present for longer than the past twelve months. The establishment of these change points depends entirely upon the judgement of the interviewer. They should be dated as accurately as possible. The probing techniques outlined above in relation to the dating of life events should be applied. Up to four Change Points can be used to describe a depressive phase. If, for example, an informant falls within category 2 above, that is, he is not depressed at the time of interview but has suffered a depressive phase at some time during the past twelve months, his depressive phase might be characterised by four change points.



Figure 1 illustrates a situation in which C1 (First Change Point) marks an increase in depressive symptomatology, C2 represents a levelling off point, C3 marks the state of a recovery phase and C4 defines the point at which the subject returned to the pre-depressive phase level. Change points should be numbered chronologically from 1 upwards. Three types of change points will be distinguished, they are listed below.

1. Point of increase in symptomatology.
2. Point of levelling of symptomatology.
3. Point of decrease of symptomatology.

In specifying Change Points three types of data are required.

1. Number of change points.
2. The date of occurrence.
3. Type of Change Point as above.

These data should be coded in the following manner.

<u>Occurrence</u>		<u>Type of Change Point</u>		<u>Date of Change Point</u>
<u>Yes</u>	<u>No</u>			
C1	1 0	1	2	3
C2	1 0	1	2	3
C3	1 0	1	2	3
C4	1 0	1	2	3

---

The date of the change point should be specified in the same manner as that used to date the life events.

The dating of the Change Points should follow the completion of the self administration schedule. You should briefly examine the subject's protocol to determine whether he has endorsed any of the depressive symptoms. It should be noted that some of the symptoms are rated the "wrong way", that is, to control for positive response biases, items such as "I still enjoy sex" receive a '3' if depression is not indicated, whereas items such as "I felt down-hearted and sad" receive a '3' in a situation where the depressive affect is likely to be strongest.

If the subject admits to the presence of symptoms you should gently state e.g. "I see that you have not been terribly happy in the last week," or "I see that you have been rather low in spirits in the last week." You should proceed to determine the Change Points in the manner outlined above.

If the subject does not admit to any symptoms and does not appear to be depressed then you should ask whether they have experienced "feeling low", "being disheartened", "being bored" etc. at any time during the last twelve months. Two points ought to be borne in mind at this stage in the proceedings. Firstly, do not use the word "depressed" but rather refer to specific symptoms; people often misunderstand the term or confuse it with anxiety. Secondly, you should not accept the informant's first reply but rather probe gently until you are convinced that further probing will not yield any more data. In a situation where an informant has not suffered from any depression in the previous twelve months all 'Change Points' occurrences should be scored "No".

The Social Adjustment Scale should be used following the questions on Early Loss. The free flowing format of the previous section should be maintained. Below is a full list of questions followed by detailed relevance criteria.

## 7 SOCIAL ADJUSTMENT SCALE

### Introduction

"I'd like to ask you some questions about you work, your leisure time and your family life. There are no right or wrong answers to these questions. We want to know the answer that best describes how you have been getting on. If any question does not make sense to you let me know Please try to answer all the questions for the last two months - that would be from (date) to today. Do you have any questions before we begin?"

"The first set of questions have to do with your work."

Male Employment status will have been determined earlier.

Female "Do you work full time or part time outside your home?"

### 1. TIME LOST

For worker ask:

"Have you missed any time from work in the last two months?"

For housewife ask:

"Were there any days in the last two months when you did not do any housework?"

-----  
If yes ask:

"How many days did you miss?"

1 = none to 2 days

2 = 3 to 5 days

3 = 6 to 10 days

4 = 11 to 20 days

5 = 20+ days  
-----

If no:

Record 0

## 2. DIMINISHED CONTACTS

"How many close friends do you have?"

"By close friend I mean people you have regularly seen or telephoned during the last few months?"

"Are you including couples? Are you close to both the people or just one?"

---

1 = 9 or more

2 = 5 to 8

3 = 2 to 4

4 = 1

5 = no close friends

---

## 3. RETICENCE DO NOT ASK IF SUBJECT DOES NOT HAVE CLOSE FRIENDS

"Have you been able to talk about your feelings openly with your friends?"

"What types of things do you discuss?"

"What type of things do you hold back?"

---

Use examples of problems known to be bothering subject.

1 = reasonably open with at least one person.

2 = mildly reticent

3 = moderately reticent or occasionally able to discuss

4 = usually able to discuss feelings

5 = unable to discuss feelings at any time

---

## 4. DIMISHED SOCIAL INTERACTIONS

"How many times have you done something socially with friends in the last two months?"

"What kind of things have you been doing socially?"

---

In number of interactions

1 = 16 or more

2 = 8 to 15

3 = 4 to 7

4 = 2 to 3

5 = 0 to 1

---

## 5. LONELINESS

"Have you felt lonely and wished for companionship these last two months?"

"Have you felt this way when you were around people too?"

---

RATE SUBJECTS FEELINGS

1 = has not felt isolated

2 = feels a little isolated or isolated occasionally

3 = feels moderately isolated or isolated often (i.e. every weekend)

4 = feels a great need for people

5 = feels totally alone or feels lonely every day

---

"The next questions are about your outside family, your relatives, not your husband/wife, children (if child in parents home up to 21 years), parents or brothers/sisters etc."

"How have you been getting along with your relatives?"

"Let's start with your parents."

#### 6. RETICENT

"During the last two months, have you been able to talk about your feelings and problems openly with any of these relatives?"

"What types of things do you discuss?"

"What types of things do you hold back?"

-----  
1 = reasonably open with at least one person

2 = mildly reticent

3 = moderately reticent or occasionally unable to discuss

4 = usually able to discuss feelings

5 = unable to discuss feelings at any time  
-----

#### 7. DEPENDENCY

"Do you depend on your family for help or advice for baby sitting or for financial help?"

"When you go visiting or go out, is it usually with the family or with friends?"

-----  
Include dependence on family for friendship

1 = quite independent

2 = a few dependent relationships

3 = mostly dependent but has other resources

4 = almost totally dependent

5 = completely dependent  
-----

#### 8. REBELLIOUS

"Did you do things just to make your family angry or annoyed or just to go against their wishes?"

"Did you want to make them angry but didn't do it?"

-----  
1 = feels no urge to defy family

2 = a little inhibited by need to defy family

3 = some decisions and values determined solely by need to defy

4 = many important decisions and values determined solely to defy

5 = goes out of way to defy family continuously  
-----

#### 9. RESENTMENT

"In the last two months have you been feeling that your relatives have let you down at any time?"

"How did they let you down?"

"Have you felt bitter?"

- 
- 1 = reasonably satisfied with family
  - 2 = appreciative but some grievances
  - 3 = disappointment but some appreciation
  - 4 = mostly bitter or disillusioned
  - 5 = consumed by bitterness or resentment
- 

10. RETICENCE

"Have you been able to talk about your feelings and problems with your husband/wife these last two months?"

"Could you tell me what kinds of things you talk about?"

- 
- 1 = confides freely
  - 2 = keeps back only a little
  - 3 = moderate disability in communication
  - 4 = marked disability
  - 5 = completely unable to express themselves
- 

11. DOMINEERING BEHAVIOUR

"Who has been making most of the decisions at home in the last two months?"

"What decisions have you been making?"

"Do you take your spouse's wishes into consideration? Even when he's not there?"

- 
- 1 = non-domineering
  - 2 = mildly domineering
  - 3 = moderately domineering
  - 4 = little consideration given to spouse's wishes
  - 5 = tyrannical
- 

12. SUBMISSIVENESS

"If you and your husband/wife have a disagreement on something, who usually gets his or her own way? Who usually goes along?"

"Have you been pressured or bullied by your spouse during the last two months? Could you give an example?"

- 
- 1 = can be firm when necessary
  - 2 = firm enough except on unimportant issues
  - 3 = cannot assert self against spouse's firm decision
  - 4 = cannot assert self against spouse's minor oppositions
  - 5 = cannot assert opinion even if invited to do so
- 

13. DEPENDENCY

"During the last two months have you had to depend on your husband/wife to help you?"

"What kind of things have you needed help with?"

"Do you lean on him for emotional support when you are upset?"

- 
- 1 = reasonably independent
  - 2 = dependent in some ways
  - 3 = moderately dependent
  - 4 = markedly dependent
  - 5 = depend on spouse in least things, cannot care for self

PARENT

"Do you have any children? step-children? foster children?"

"These questions are about just your children living at home with you?"

"What are your children's names and ages?"

Name .....	Age .....
.....	.....
.....	.....
.....	.....
.....	.....

14. LACK OF INVOLVEMENT

"What kinds of things have you been doing with the children during the last two months? Let's start with (name)."

- 
- 1 = active involvement in children's lives
  - 2 = good interest, knows children's lives well
  - 3 = moderate interest
  - 4 = little interest
  - 5 = disinterested, totally uninvolved
- 

15. IMPAIRED COMMUNICATION

"Have you been able to talk to your children during the last two months?"

"Starting with (name)."

"Does he/she come to you with problems?"

"Could you give me an example from the last two months?"

- 
- 1 = communicates easily
  - 2 = most times can communicate
  - 3 = fair communication
  - 4 = rarely able to talk
  - 5 = never able to talk
- 

16. ECONOMIC INADEQUACY

"This last question has to do with your finances. In the last two months have you had enough money for your basic needs?"

"Have you had to go into your savings?"

"Have you had to put off important things?"

"Could you tell me, are you receiving social security?"

- 
- 1 = income adequate for needs (not necessarily adequate for wants)
  - 2 = income and reserves adequate with minor problems

- 3 = income and reserves inadequate leading to major problems and/or small loans
- 4 = income and reserves inadequate necessitating supplements from outside resources. Subject is having major problems.
- 5 = severe financial problems, totally dependent with no income or reserves, on social security

#### SOCIAL ADJUSTMENT SCALE - SCORING CRITERIA

##### 1. TIME LOST

Include physical illness, psychological difficulties.  
Exclude days laid off, days unemployed and holidays.

##### 2. DIMINISHED CONTACTS

Exclude as friend's, siblings, parents, children, close aunts and uncles, spouse's siblings and parents; exclude co-workers, unless there is contact outside of work; exclude friends of the opposite sex if dating; exclude old friends with no recent contact. Include as friends other relatives such as cousins or spouse's siblings.

Regular contact is defined as contact about once a week during the two month period.

Exclude second member of a couple unless subject is close to both.

##### 3. RETICENCE

The subject should not be asked to determine the degree of which the communication was appropriate.

The rater should not confuse satisfaction and level of communication (e.g. what is quite satisfying to one person may not be open communication and vice versa).

Reasonably open includes subjects that withhold appropriately private matters (i.e. sex life with spouse).

##### 4. DIMINISHED SOCIAL INTERACTIONS

Includes entertaining or visiting friends (not family excluded as close friends, see no. 2); going out in company of others (not just spouse, date or family) including cinema, sports events, restaurants, shopping with friends, etc.

Include weddings, parties, club meetings attended.

Include attending church if subject socializes with people at the service.

Ask subject to recall activities, asking specifically about each type of situation. The subject will not necessarily know which activities the rater wants to include.

If subject went alone to an activity, rate only if he was an active participant rather than audience.

#### 5. LONELINESS

Do not include fear of being alone, loneliness when with others or cosmic loneliness ("I'm all alone in the world").

Loneliness must be a desire for human companionship.

#### 6. RETICENT

The subject should not be asked to determine the degree to which the communication was appropriate.

The rater should not confuse satisfaction and level of communication. Reasonably open includes subjects that withhold appropriately private matters.

#### 8. REBELLIOUS

Focus on whether the subject could make more effective decisions if he did not need to defy the family.

Rate effect on actions and behaviour.

#### 9. RESENTMENT

Include absent and deceased family members.

Include feelings experienced in the period about events or relationships which occurred anytime in the past.

RATE SUBJECTS FEELINGS.

#### 11. DOMINEERING BEHAVIOUR

#### 12. SUBMISSIVENESS

The next two questions are used to evaluate the dominant-submissive balance in the marriage and are therefore asked at the same time. It is unusual, although not impossible, for a subject to rate a high number on both items for the two month period.



### 13. DEPENDENCY

Ask about specific types and instances of assistance as appropriate by sex. For women, driving, shopping, monetary decisions, helping with children, helping with housework, minor repairs (i.e. changing a light bulb or fuse etc.) emotional support. For men, monetary decisions, driving, upkeep of house, emotional support.  
DO NOT RATE AFFECTION AS SUPPORT

### 14. LACK OF INVOLVEMENT

Ask about each child separately and average for rating.

For pre-school children inquire about involvement with day to day care, child's play activities, pre-school learning.

For older children inquire about school progress, child's interests friends and dates, special problems, chores, etc.  
Do not rate feelings, rate behaviour.

### 15. IMPAIRED COMMUNICATIONS

Rate for each child and average.

Consider what is appropriate communication for the child's age.

Do not rate for children under two years of age.

By communication is meant discussion of feelings and problems and other overt forms of communication, not just the relating to activities.  
DO NOT RATE FEELINGS

### 16. ECONOMIC INADEQUACY

This question covers the situation of the immediate family unit.

Do not rate whether a husband gives his spouse sufficient money but whether the family unit has sufficient income and resources to meet their basic needs.

Do not rate a mortgage to buy a house as difficulties leading to a loan.

7 DRUG QUESTIONS.

The following list of questions should be asked of the informant at the end of the interview. You should make the point that we are asking these questions because the use of medication might affect the responses we get during the interview. The instructions are as follows.

"I WILL READ YOU A LIST OF REASONS FOR WHICH YOU MIGHT HAVE TAKEN SOME MEDICINE OR DRUG. TELL ME WHETHER OR NOT IT WAS TRUE FOR YOU DURING THE PAST WEEK.

DID YOU TAKE ANY MEDICINE FOR.....? (READ CHOICES)"

IF YES, ASKS "HOW MANY DAYS DID YOU TAKE IT?"

1. FOR PAIN?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

2. TO LIFT UP YOUR SPIRITS, TO CHEER UP?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

3. TO CALM DOWN?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

4. TO PEP UP?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

5. TO HELP YOU SLEEP?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

6. TO HELP YOU STAY AWAKE?

0 NO 1 YES \_\_\_\_ NUMBER OF DAYS \_\_\_\_

## 9 POST INTERVIEW SCHEDULES

The post-interview questionnaire should be completed at the earliest opportunity following the interview. The "Post Interview" schedule consists of four rating scales.

- (a) Social skills ratings
- (b) Global Social Adjustment Ratings
- (c) General Ratings
- (d) Psychiatric Manifestations

### (a) Social Skills Ratings

With these behavioural ratings we are concerned with a range of behaviours which are thought to be important components of social skills. Your ratings should be made in such a way as to discount any uncertainty or hesitation in the initial phase of the interview.

The ratings to be used are as follows

### POST-INTERVIEW QUESTIONNAIRE A

- |                   |  |
|-------------------|--|
| 1. <u>Volume</u>  | <u>Voice Quality</u>   |
|                   | 0      Appropriate volume  |
|                   | 1      In between  |
|                   | 2(a)   Somewhat too loud   |
|                   | (b)   Somewhat too soft  |
|                   | 3      In between  |
|                   | 4(a)   Much too loud   |
|                   | (b)   Much too soft  |
| 2. <u>Tone</u>    | 0      Well-modulated tone with appropriate expression                                       |
|                   | 1      In between  |
|                   | 2      Tone tending to be somewhat flat and expressionless                                   |
|                   | 3      In between  |
|                   | 4      Tone completely flat, no vocal expression, says everything in dull, monotonous voice. |
| 3. <u>Clarity</u> | 0      Speech appropriately clear and well-enunciated  |
|                   | 1      In between  |
|                   | 2(a)   Somewhat unclear, tendency to mumble  |
|                   | (b)   Tendency to over-enunciate   |
|                   | 3      In between  |
|                   | 4(a)   Markedly indistinct, slurred  |
|                   | (b)   Markedly over-precise, clipped, pedantic   |

4. <u>Pitch</u>	0	Appropriate moderate pitch
	1	In between
	2	Moderate pitch, but occasionally slipping into unpleasant high pitch, at beginning or end of utterances
	3	In between
	4	Unpleasant high pitch, squeaky

Comments on Any of the Above

		<u>Conversation</u>
5. <u>Interest in Other</u>	0	Shows appropriate interest in other person e.g. asking questions about themselves, bringing them into conversation, seeking their opinion encouraging them to talk about themselves
	1	In between
	2	Shows moderate interest in the other person e.g. asks occasional question about them or their opinion, but predominantly not paying a great deal of attention to them.
	3	In between
	4	Shows no or very little interest in other e.g. does not seek information about them or their opinions, or only very rarely; monopolises conversation and doesn't give them a chance to talk.
6. <u>Speech Errors</u>	0	None
	1	In between
	2	Occasional stuttering, repetitions, omissions, etc. (specify)

N.B. Speech errors include any of the following: "er", "ah" or "um" sentence incompleteness; tongue slip; intruding incoherent sound. Please underline type(s) of error.

Comments on any of Above

		<u>Physical Accompaniment of Speech</u>
7. <u>Facial Expression</u>	0	Predominantly relaxed, smiling, lively, open, interested.
	1	In between
	2	Predominantly expressionless, vacant, blank
	3	In between
	4	Predominantly hostile, aggressive, glaring, bad-tempered.

8. Posture
- 0 Appropriate posture for situation, poised, holds self well but not stiffly, sits naturally, decorously, not slouched.
  - 1 In between
  - 2(a) Somewhat inappropriate posture - a bit stiff and unnatural
  - (b) Somewhat inappropriate posture - a bit too relaxed in informal for situation
  - 3 In between
  - 4(a) Very inappropriate posture - extremely rigid and unrelaxed, stiff, upright
  - (b) Very inappropriate posture - slouched and inelegant, or much too informal
9. Gaze
- 0 Appropriate looking behaviour - e.g. looks at other at intervals when speaking or listening and at end of sentences, not staring, not avoiding looking at other.
  - 1 In between
  - 2 Somewhat inappropriate - e.g. tendency to avoid looking at other person, occasionally or to stare.
  - 3 In between
  - 4 Extremely inappropriate - little or no eye contact, looks at floor, hands, ceiling or elsewhere most of the time
10. Appearance
- 0 Appropriate appearance; indicating care has been taken (not necessarily expensively got up)
  - 1 In between
  - 2 Mediocre appearance; not dirty or unkempt but little attempt to look attractive
  - 3 In between
  - 4 Slovenly or dirty appearance, very unkempt, unwashed, neglect of self

(Please note identity being presented)

POST INTERVIEW SCHEDULE B  
GLOBALS - SOCIAL ADJUSTMENT RATINGS

Globals should be done immediately after the interview. Use all available information and own judgement in making these ratings. Unlike the items which must be based on the information supplied by the subject, the globals can take into account suspected denial. Also take into account behaviour that is maladaptive but not covered in the items (i.e. the compulsive housewife, the parent that lives through her child with no life of her own, the subject with a social life so hectic that it interferes with her health). Do not use the average of interview items!

Do not rate for areas for which none of the interview items were rated.

Work

- 1 = Excellent - no maladjustment, excellent adjustment and performance in main role, without deficiencies.
- 2 = Good - no maladjustment, adequate adjustment with steady effective function, but on account of minor deficiencies cannot be rated as excellent.
- 3 = Mild maladjustment - definite deficiencies, outside the range of adequate adjustment, but deficiencies limited in severity, pervasiveness, and proportion of time they manifest.
- 4 = Moderate maladjustment - deficiencies moderate and manifest about half the period, or work adjustment about half its potential.
- 5 = Marked maladjustment - poor functioning for most of the period but occasionally still displays adaptive behaviour.
- 6 = Severe maladjustment - very poor functioning most of the period
- 7 = Very severe maladjustment - complete inability to function in work role throughout period.

Social Leisure

- 1 = Excellent - no maladjustment, excellent adjustment with no deficiencies. Plentiful friendships and activities outside immediate family.
- 2 = Good - no maladjustments, adequate adjustment with friendships and activities appropriate to natural situation but not such as to warrant being described as plentiful.
- 3 = Mild maladjustment - definite deficiencies in friendships, social participation and leisure activities, but limited in severity, pervasiveness and proportion of time manifest.
- 4 = Moderate maladjustment - deficiencies moderate and manifest about half of the period or social adjustment about half its potential.
- 5 = Marked maladjustment - marked deficiencies in friendships, social participation and leisure activities but occasionally still displays adaptive behaviour.
- 6 = Severe maladjustment - very poor social functioning most of the period
- 7 = Very severe maladjustment - completely isolated and socially maladjusted throughout the period.

### Extended Family

- 1 = Excellent - no maladjustment, close and harmonious relationships with all living members of family of origin.
- 2 = Good - no maladjustment, harmonious relationships with parents and siblings with minor deficiencies or in so far as geography permits.
- 3 = Mild maladjustment - definite deficiencies outside the range of adequate relationships but limited in number of relatives involved and severity.
- 4 = Moderate maladjustment - may be discordant relationships with several relatives or coolness to family in general.
- 5 = Marked maladjustment - poor relationship with much of family, but some relationship retained.
- 6 = Severe maladjustment - discordant relationship with entire family or origin, with minimal contact.
- 7 = Very severe maladjustment - entirely out of social contact with family of origin for entire period due to severely discordant relationships or any interaction extremely limited and discordant.

### Marital

- 1 = Excellent - no maladjustment, very good relationship in all aspects, no deficiencies.
- 2 = Good - no maladjustment, stable and adequate marital relationship which on account of minor deficiencies, cannot be regarded as excellent.
- 3 = Mild maladjustment - definite deficiencies, outside the range of adequate adjustment, but deficiencies limited in severity, pervasiveness and proportion of time manifest.
- 4 = Moderate maladjustment - deficiencies moderate and manifest about half the period or marital adjustment about half its potential.
- 5 = Marked maladjustment - poor marital relationship with discord but some relationship remaining.
- 6 = Severe maladjustment - relationship very poor in most respects. Separation may be impending or actual at time of rating.
- 7 = Very severe maladjustment - separation actual at time of rating and serious divorce proceedings impending.

### Parental

- 1 = Excellent - no maladjustment, very effective parent with good parent-child relationship with all children, no deficiencies.
- 2 = Good - no maladjustment, adequate parent relationship which cannot be described as excellent on account of minor deficiencies.
- 3 = Mild maladjustment - definite deficiencies in some areas with respect to at least one child but limited in pervasiveness and time manifest.
- 4 = Moderate maladjustment - deficiencies involving more children and manifest more consistently.
- 5 = Marked maladjustment - consistent deficiencies involving most of parental function and relationships but with some assets.

- 6 = Severe maladjustments - very poor adjustment in most respects and manifest most of the period.
- 7 = Very severe maladjustment - total neglect and lack of affection throughout the period. Children may be in the care of relatives or foster parents at the time of rating.

Overall Adjustment      USE YOUR OWN JUDGEMENT IN MAKING THE RATING  
DO NOT USE THE AVERAGE OF THE GLOBALS SOLELY

- 1 = Excellent - no maladjustment, excellent rating in most areas. Handles problems well, good interpersonal relationships, happy with adjustment.
- 2 = Good - adequate adjustment but because of deficiencies cannot be called excellent.
- 3 = Mild maladjustment - definite areas of deficiencies but limited in severity, pervasiveness or time of manifestation.
- 4 = Moderate maladjustment - greater deficiencies (moderate maladjustment includes subjects with good adjustment in some areas but marked maladjustment in others).
- 5 = Marked maladjustment - relatively persistent pervasive and severe difficulties.
- 6 = Severe maladjustment - marked or severe maladjustment in most areas.
- 7 = Very severe maladjustment - marked or severe maladjustment in all areas.



POST INTERVIEW SCHEDULE C

GENERAL RATINGS

PHYSIQUE

This is easier to assess in men than in women. Only those respondents who are fairly obviously either lean, fat or muscular should be classified as such, others should be recorded as 'indeterminate'.

- 1 Lean (leptomorphic)
- 2 Fat (pyknomorphic)
- 3 Muscular (mesomorphic)
- 4 Indeterminate
- 5 Not known

RELIABILITY DATA

The interviewer is required to give a subjective rating of the degree to which he takes the informant's information to be reliable. The format to be used is as follows:-

Reliability: To what extent is the information reliable?

Not at all				Very
1	2	3	4	5

if unreliable (3 or less) give reasons

- 0 Multiple or other than listed below
- 1 Limited intelligence
- 2 Confused
- 3 Difficulty understanding questionnaire
- 4 Deliberately misleading
- 5 Uninterested
- 6 Distracted (attention wanders)
- 7 Don't know

### INTELLIGENCE RATINGS

In any large population about 50% will have 'average' intelligence, 20% will be 'above' and 20% 'below' average, 5% will be 'superior' and 5% 'dull'. It does not necessarily follow that the respondents you see will be distributed in this way as regards intelligence. Take everything into account in making your assessment, a child at grammar school, for example, is unlikely to be below average.

The ratings should be made in the following way:-

I.Q.	Superior	Above Average	Average	Below Average	Dull	N.K.
	1	2	3	4	5	6

## POST INTERVIEW SCHEDULE D

## PSYCHIATRIC MANIFESTATIONS

The aim here is to record symptoms or signs of a psychiatric nature. It is not the aim to make a psychiatric diagnosis (because for the most part, the interviewer is unlikely to have enough information to make a reasonably firm diagnosis). A "psychiatric sign" is present when the respondent displays an undue degree of anxiety, depression etc. If, for example you think the respondent is unduly suspicious, record this - whether or not you also consider that he has a paranoid personality.

### 1. Anxiety and Tension (0 - 4)

Many signs are included here, such as tension and difficulty in relaxing, irritability, ranging over trivial matters, apprehension and feelings of panic, fears, difficulty in concentrating, forgetfulness and feeling of appearing "jumpy".

### 2. Depression (0 - 4)

Depressed mood is not easy to assess. One looks for a gloomy attitude, pessimism about the future, feelings of hopelessness and a tendency to weep. As a guide, occasional weeping could count as 2, frequent weeping as 3 and severe symptoms allotted 4 points. When patients are severely depressed they may 'go beyond weeping'. It is generally believed that women weep more readily than men, but there is little evidence that this is true in the case of depressive illness. There is no reason to believe, at the moment, that an assessment of the frequency of weeping could be misleading when rating the intensity of depression in women.

### 3. Retardation (0 - 4)

Severe forms of this symptom are rare, and mild forms are difficult to perceive. A slight flattening of affect and fixity of expressions rate as 1, a monotonous voice, a delay in answering questions, a tendency to sit motionless count as 2. When retardation makes the interview extremely prolonged and almost impossible, it is rated 3, and 4 is only given when an interview is impossible. This rating should be strictly based on observation because although some patients may say their thinking is slowed or their emotional responsiveness has been diminished, questions about these manifestations usually produce misleading answers.

### 4. Agitation (0 - 4)

Severe agitation is extremely rare. Fidgetiness at interview rates as 1, obvious restlessness with pulling at hands and clothes should count as 2. If the patient has to get up during the interview he is given 3, and 4 points are only given if the interview has to be conducted 'on the run', with the subject pacing up and down, picking at his face and hair and tearing at his clothes. Although agitation and retardation may appear to be opposed forms of behaviour, in mild forms they can co-exist.

##### 5. Paranoid Symptoms (0 - 4)

These are uncommon. It is of no significance if the patient says that others talk about him, since this is usually true. What is important in the mild symptom is the subject's attitude of suspicion, and the malevolence imputed to others. Doubtful or trivial suspicion rates as 1, thoughts that others wish him harm as 2, delusions that others wish to harm or are trying to do so rates as 3, and hallucinations are given 4 points. Care should be taken not to confuse this symptom with that of guilt e.g. "people are saying I am wicked".