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INCOME MAINTENANCE, LIFE CHANGES, AND PSYCHOLOGICAL DISTRESS: IMPLICATIONS FOR LIFE EVENTS THEORY

Socioeconomic Research Center

Research Memorandum 66
July 1979

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LIFE EVENTS THEORY** 11

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SRI Projects URD 8750/1190

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The research reported herein was performed pursuant to contracts with the states of Washington and Colorado, prime contractors for the Department of Health, Education, and Welfare under contract numbers SRS-70-53 and HEW-100-78-0004 respectively. The opinions expressed in the paper are those of the authors and should not be construed as representing the opinions or policies of the states of Washington or Colorado or any agency of the United States Government.

ABSTRACT

INCOME MAINTENANCE, LIFE CHANGES, AND PSYCHOLOGICAL DISTRESS; IMPLICATIONS FOR LIFE EVENTS THEORY

Previous work has indicated that a positive change in financial circumstances due to participation in an income maintenance experiment raises the psychological distress scores of some experimental subjects relative to controls. This study reexamines the effect of an income maintenance program controlling for intervening stressful life events that might have been caused by the experiment. The subjects are 5,596 low-income adult heads of household living in Denver and Seattle. Psychological distress is measured with a version of the MacMillan Health Opinion Survey index. The experiment is shown to have a significant distressing effect upon some groups (particularly blacks) independent of the effects of intervening life events. In most groups, however, income maintenance has no significant effect upon distress. This result is consistent with the "undesirability hypothesis" in the life events literature.

The results of this study cast doubt upon several generalizations in the life events literature. First, this study indicates that the inclusion of health problems in life events scales may account for the strong relationship often shown between events and distress. Second, contrary to the findings of some previous researchers, undesirable change appears to predict distress better than total amount of change. Third, despite the implication in the literature that events have similar impacts on all individuals, life events in this study are shown to vary in their effects by sex, race-ethnicity, and marital status. Additional critiques of the life events literature are made and directions for future research are suggested.

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I INTRODUCTION

In a previous paper Thoits and Hannan (1978) reported the effects of a large-scale income maintenance experiment upon the psychological distress of low-income subjects in Seattle and Denver. The authors found that in most sex, marital status, and ethnic subgroups the experimental treatments had no significant effects upon distress. However, in subgroups for which the experiment did have significant effects, the effects were always positive, raising subjects' psychological distress levels relative to controls. It appears that a positive change in a family's financial circumstances may increase psychological distress, at least in some groups. This paper explores these effects in more detail, using a "life events" approach. Of interest is whether the experiment affects distress directly or indirectly through its impact upon the occurrence of major life events such as divorce, unemployment, and migration. In the analysis the effects of these life events are controlled and experimental impacts upon distress are reexamined. The results of this analysis not only clarify the experimental effects but contribute to the development of life events theory in general.

Theoretical Approach

A life events researcher typically examines the relationship between the occurrence of major life events and some indicator of physical or psychological distress. In this paper attention will be confined to psychological distress outcomes only. Life events refers to "objective events that disrupt or threaten to disrupt the individual's usual activities" (Dohrenwend and Dohrenwend, 1969). These events require the individual to readjust behavior substantially, and readjustments are assumed to cause stress in the individual. Intense or prolonged stress may eventuate in physical or psychological disorder.

Two variants of this theory guide much of current life events research. One which I shall term the "undesirability hypothesis," assumes that only undesirable events cause distress. According to this hypothesis, the experience of many undesirable events or one intensely threatening event overloads the capacity of the actor to adapt to or control anxiety, thus leading to psychological impairment. Favorable events, on the other hand, should have no effect or should cause a decrease in psychological distress. The second hypothesis, which I shall call the "total change hypothesis," assumes that the more life events the individual experiences, regardless of their desirability, the more likely coping abilities will be overtaxed. Psychiatric disturbance follows from the loss of these abilities. Undesirable or threatening events are considered stressful in the first approach, while events that require readjustment regardless of their desirability are deemed stressful in the other.

Empirical studies support both hypotheses. There is strong evidence that undesirable events are associated with distress and even serious psychopathology (Basowitz, Persky, Korchin, & Grinker, 1955; Birley & Brown, 1970; Bremer, 1951; Brown & Birley, 1968; Clayton, Halikas, & Maurice, 1972; Fried, 1963; Grinker & Spiegel, 1945; Hudgens, Morrison, & Barchha, 1967; Janis, 1951, 1954; Lindemann, 1944; Myers, Lindenthal, & Pepper, 1971; Paykel, 1974; Rogler & Hollingshead, 1965; Schmale, 1958; Star, 1959). For example, Brown and Birley (1968) found that 60% of a schizophrenic sample, as opposed to 19% of a control sample, experienced one or more undesirable life events in the three weeks before the onset of illness. Singular undesirable events such as forced relocation due to slum clearance (Fried, 1963), bereavement (Clayton et al., 1972; Parkes, 1974; Stein and Susser, 1970), and even President Kennedy's assassination (Sheatsley and Feldman, 1964), have been shown to affect physical and emotional health adversely. However, there is equally strong evidence that total amount of change, regardless of desirability, is associated with mental illness (Coates, Moyer, & Wellman, 1969; Dohrenwend and Dohrenwend, 1970; Fröberg, Karlsson, Levi, & Lidberg, 1971; Markush and Favero, 1974; Myers et al., 1971, 1972, 1974; Paykel, Myers, Dienelt, Klerman, Lindenthal, & Pepper, 1969; Paykel, Prusoff, & Myers, 1974;

Unlenhuth and Paykel, 1973). For example, in a community study Myers and his associates (1972) found that the more psychologically distressed the individual, the more likely was the individual to have experienced a high number of undesirable and desirable events, particularly events which require a high degree of readjustment.

Several researchers have directly compared the relationship between undesirable events and psychological distress to the relationship between amount of change and distress. Dohrenwend (1973) found a correlation of .35 between undesirable events and number of symptoms and a correlation of .40 between amount of change and symptoms. She concluded that "change rather than undesirability is the characteristic of life events that should be measured for the more accurate assessment of their stressfulness." Gersten and her associates (1974), however, came to an opposite conclusion. Their finding was that whether conceived in pure terms or as the net difference between undesirable and desirable events, undesirability was the more productive measure of distress. This conclusion carries some weight inasmuch as Gersten, Langner, Eisenberg, & Orzek (1974) studied these correlations with multiple measures of psychological disorder. Further support for Gersten's conclusion comes from other studies (Mueller, Edwards, & Yarvis, 1977; Myers et al., 1974; Paykel et al., 1969, 1974; Ross and Mirowsky, 1978; Selzer, 1975; Vinokur, 1975). Paykel and his co-workers assert that the total change model by itself is not adequate to explain the occurrence of depression. They found that desirable changes occurred with equal frequency in control and depressed patient samples, but that undesirable events were significantly more common among the patients. If amount of change had been the primary factor, both favorable and unfavorable events should have been more prevalent among patients. Myers et al. (1974), using a large community sample, concur with Paykel's findings. Thus, the desirability of life events may have a more important effect on mental health than amount of change per se, although amount of change seems also related to psychological outcome.

There are three major difficulties with these life events studies that concern me here. First, amount of change and undesirability seem to be confounded on the typical life events scale. Most scales contain

far more negative than positive events (see for example Gersten et al., 1974; Homes and Rahe, 1967; Myers et al., 1974; Paykel, 1974a,b). Undesirable changes may contribute heavily to the relationships found between total change and psychological disturbance. Second, researchers have rarely attempted to analyze the separate contributions of desirable and undesirable events to variation in psychological outcomes, even though multivariate techniques would enable this. (A recent study by Ross and Mirowsky (1978) is a notable exception.) Third, most life event scales contain items which are symptoms or concomitants of psychological distress. These items, such as "change in eating habits," "change in sleeping habits," or "physical illness or injury," may confound cause and effect. The research reported in this paper attempts to avoid each of these problems. Roughly equal numbers of negative and positive experiences are included in the list of life events utilized here. Furthermore, desirable and undesirable experiences are entered in the analysis separately. Finally, health changes are controlled in the analysis both to assess their independent effect and to reduce variation in the dependent variable attributable to physical illness.

Life Events and Income Maintenance

An income maintenance experiment provides a family with an income guarantee. Monthly payments supplement the family's income whenever it falls below a specified level. The experiment represents a major improvement in financial circumstances in two ways. For all experimental subjects it assures a minimum income level regardless of changes in family, employment, or other circumstances. For those with preexperimental incomes below the guaranteed support level, it provides additional money resources.

From a life events perspective, an income maintenance experiment may have two major effects upon psychological distress. First, the experiment may influence distress directly as a life event in itself. Second, the experiment may have an indirect impact through its effect on the occurrence of other life events. Given an income guarantee, an individual may leave an unhappy marriage, stop working, get needed medical treatment, return to

school, or migrate to another city. These events in turn may have significant impacts upon psychological distress.

The undesirability and total change hypotheses make different predictions regarding the direct effect of an income maintenance experiment upon distress. Because the experiment represents a favorable change in financial circumstances, the undesirability hypothesis predicts either no effect on distress or somewhat lowered distress. The total change hypothesis, on the other hand, predicts an increase in distress, because regardless of its desirability, the experimental change places stress on individuals' coping abilities.

In previous analysis (Thoits and Hannan, 1978) I examined the total effects of the Seattle and Denver income maintenance experiments upon psychological distress. The coefficients reported in that paper (and reproduced in Table 1) represent the sum of all direct and indirect effects of the experimental treatments upon distress.* In general, I found few significant experimental effects upon distress two years after enrollment in the experiment. These results appear to support the undesirability prediction. However, the coefficients in Table 1 show that the program did have a significant effect on several subgroups and in each case the total impact of the experimental treatment was to raise psychological distress.

Because the coefficients are the sum of direct and indirect effects, we do not know whether these positive impacts are due to the effect of income maintenance per se or to stressful events which result from this favorable change in financial circumstances. It is very possible that the latter situation is the case. Previously it has been shown that income maintenance significantly increases the experimentals' rate of marital dissolution relative to controls (Hannan, Tuma, & Groeneveld, 1977), increases the experimentals' duration of unemployment (Robins and Tuma, 1977), and also increases their rates of geographic mobility (Keeley,

*The experimental sample and financial treatments will be described in more detail in the following section.

Table 1

EFFECTS OF THE THREE- AND FIVE-YEAR EXPERIMENTAL TREATMENTS
UPON PSYCHOLOGICAL DISTRESS^c at TIME 2[†]

	Married Men ^b	
	Denver	Seattle
3-year white experimental	.08	-.53
5-year white experimental	3.22***	-1.04
3-year black experimental	-.03	2.17**
5-year black experimental	.14	-1.12
3-year Chicano experimental	1.04	--
5-year Chicano experimental	.48	--
Payment (in thousands of dollars)	-.06	.36
<hr/>		
R ²	.06	.13
F-test for set of treatment variables (Degrees of freedom)	1.93* (7,1205)	2.39** (5,773)
Number of cases	1,228	793
<hr/>		
	Married Women ^b	
	Denver	Seattle
3-year white experimental	.63	1.19
5-year white experimental	2.42**	1.16
3-year black experimental	-.33	.05
5-year black experimental	2.91**	-1.03
3-year Chicano experimental	.95	--
5-year Chicano experimental	2.06*	--
Payment (in thousands of dollars)	-.37	.12
<hr/>		
R ²	.05	.08
F-test for set of treatment variables (Degrees of freedom)	2.02* (7,1228)	.87 (5,800)
Number of cases	1,251	820
<hr/>		
	Single Women ^b	
	Denver	Seattle
3-year white experimental	.20	-.79
5-year white experimental	-.51	1.51
3-year black experimental	2.28**	2.42*
5-year black experimental	2.69**	-.68
3-year Chicano experimental	-1.10	--
5-year Chicano experimental	.32	--
Payment	.08	.46
<hr/>		
R ²	.04	.05
F-test for set of treatment variables (Degrees of freedom)	1.15 (7,870)	2.27** (5,599)
Number of cases	889	615

*.10 ≥ p > .05 ** .05 ≥ p > .01 *** .01 > p

[†]Holding background variables (age, race-ethnicity, preexperimental income, education, parenthood, occupation at enrollment, working at enrollment, spouse's enrollment occupation, spouse working at enrollment) constant.

^aTime 2 occurs approximately 20 months after enrollment for males and 24 months after enrollment for females.

^b"Married" indicates married or cohabiting at enrollment. "Single" indicates divorced, separated, or widowed at enrollment.

^cA positive coefficient indicates increased distress. A negative coefficient indicates decreased distress. Unstandardized regression coefficients are reported.

1976) and fertility (Keeley, 1978). In this report, the effects of these and other intervening life events will be controlled. The net effect of a positive change in financial circumstances will be examined and the degree of support for the undesirability and total change predictions will be assessed.

II METHODOLOGY

Sample

A sample of approximately 7,500 adult heads of household participating in the Seattle and Denver income maintenance experiments (SIME/DIME) provides the basis for analysis in this study. Low income areas in each city were canvassed for eligible families. To be eligible, a family had to meet certain requirements. These requirements stratified the sample along several lines: family structure, race-ethnicity, and preexperimental family income. The details are explained below.

To be eligible to participate in the experiment, a family had to consist of a two-head household or a single-head household with one or more dependent children. Two-head households consisted of married couples with or without children and cohabiting couples with children. Single-head households were headed primarily by divorced or separated women. Heads were required to be between the ages of 18 and 58 at the time of enrollment in the experiment (1970-71 in Seattle and 1971-72 in Denver). Approximately 60% of the selected families had two heads of household. These requirements produced a sample consisting of three marital types: married men, married women, and divorced or separated women (hereafter termed "singles"). (Divorced and separated men are not analyzed in this study. There are too few of these individuals (N=63) for meaningful statistical comparisons.)

Three racial-ethnic groups were selected: blacks, whites, and Chicanos. In the Denver sample, the three groups are approximately equally represented. In Seattle, half of the sample is black, the other half white.

In addition to family structure and race-ethnicity, preexperimental family income was a criterion for selection. The earnings of a family could not exceed \$9,000 for a single-worker family of four and \$11,000

for a two-worker family of four. These limitations on income were adjusted by an index that depended on family size. Families were grouped into eight preexperimental income categories and were assigned to various experimental treatments.

Design of the Experiment

The design is experimental in that families were assigned randomly to treatments within the categories of family structure, race-ethnicity, preexperimental income, and site. Approximately 44% of the families were assigned to the control condition (in which case we merely interviewed them). The rest were assigned to one of a set of income maintenance treatments.

An income maintenance treatment is defined by a support level and a tax rate. The support level is the amount of the grant to a family with no other source of income. The tax rate is the function that diminishes the grant as family income increases. To control the tax rates of families assigned to an income maintenance treatment, effort is made to eliminate the influence of other tax and transfer programs. Therefore, positive nonexperimental taxes (i.e., federal and state taxes) are fully reimbursed and public transfers are fully taxed. Controls, of course, receive no payment. Controls may receive transfers from other programs such as welfare or Aid to Families with Dependent Children (AFDC). Controls also are subject to federal and state taxes.

The support levels for an experimental family of four (in constant 1971 dollars) are \$3,800, \$4,800, and \$5,600 per year. The support levels are adjusted for family size and for price changes (by the Bureau of Labor Statistics cost-of-living index for each city). There are 50% and 70% constant experimental tax rates and two declining experimental tax rates. The declining tax rates begin at either 70% or 80% and decline 2.5% for each thousand dollars of taxable income. The least generous plan, the low support-70% constant tax, is just sufficient to bring families up to the official poverty level and to exceed slightly the support available from existing welfare programs, particularly AFDC.

The duration of the experiment also varies from family to family. In designing the experiment it was anticipated that individuals who viewed the experiment as transitory would adjust their behavior differently from those who viewed the program as permanent. The experiment had to be long enough so that once an initial period of adjustment had elapsed, sufficient time would remain for individuals to be expected to make long-term behavior adjustments. Although it was expected that a 3-year period would be sufficient, 25% of the families were enrolled for a 5-year period to ensure that long-term adjustments would in fact be observed.*

An important aspect of the income maintenance program is its guarantee. It was noted above that the least generous plan differs very little in financial terms from the combination of AFDC and food stamps. But, unlike AFDC, a family can receive a grant even if the male family head is employed. Furthermore, unlike AFDC, the income guarantee exists for all members of the family after a family unit breaks up. When an enrolled couple dissolves their marriage, each remains on income maintenance, but each person's grant is recalculated to take into account changes in family size and financial conditions. When single adults on the program marry, the new spouse and his/her children become eligible for income maintenance transfers. The eligibility is immediate if the marriage is official; if the union is common-law, eligibility begins after a 3-month waiting period.

Families are interviewed every four months both during the experiment and for a 2-year period following disenrollment. Families are followed anywhere in the continental United States. The interviews provide a continuous record (with dated changes) of family composition, marital status, and labor supply. They also provide historical information and attitudinal data. Data collection procedures are identical for experimentals and controls.

* See Kurz and Conlisk (1972) and Kurz and Spiegelman (1972) for detailed descriptions of the assignment process and other aspects of the experimental design.

Measurement of Psychological Distress

The psychological distress index used in the Denver and Seattle income maintenance experiments is a close variant of the MacMillan Health Opinion Survey index (MacMillan, 1957). The instrument consists of a series of psychophysiological symptom items. The MacMillan index has been used in several prevalence studies (DHEW, 1970; Gurin, Veroff, & Feld, 1960; Jackson, 1962, 1965; Leighton, Harding, Macklin, MacMillan, & Leighton, 1963; Myers et al., 1971; Srole, Langner, Michael, Opler, & Rennie, 1962). Validity studies have shown that this instrument, and variants of it developed by Gurin et al. (1960) and Langner (1962), significantly discriminate between psychiatric patients and nonpatient community residents (Leighton et al., 1963; MacMillan, 1957; Brawer, Hunt, & Kercher, 1963; Spiro, Siassi, & Crocetti, 1972; Tousignant, Denis, & Lachapelle, 1974). However, the instrument primarily taps neurotic symptomatology; it is less sensitive to psychotic symptoms (Schwartz, Myers, & Astrachan, 1973; Leighton et al., 1963). Those cases identified by the index are likely to be individuals whose psychological state impairs their everyday functioning to some degree, not individuals who are actively psychotic.

The MacMillan index and its variants have been severely criticized in recent reviews (Seiler, 1973; Tousignant et al., 1974; Spiro et al., 1972). The three most serious difficulties are that the scale lacks diagnostic specificity, a social desirability bias may be present, and physical illness may also bias responses. However, none of these problems mitigates against the use of the index in this report. First, my purpose is to assess general distress in the sample and not the prevalence of particular psychiatric disorders, so lack of diagnostic specificity is not a handicap. Second, although it is possible that subjects may respond with socially desirable answers to the symptom items, this bias simply lessens variance in the scores and forces one to draw more conservative conclusions. Desirability may vary by sex, social class, or race-ethnicity, but these variables are controlled in the analysis. Finally, because the items of the index are primarily physiological in nature, it is possible that the distress scores reflect variations in

physical rather than psychological health. However, the index has repeatedly been shown to discriminate successfully between psychiatric patients and nonpatient controls. Because the index has strong criterion validity, it seems reasonable to employ it as a measure of psychological distress. To minimize the possibility that physical and not psychological distress is measured, I control in the analysis for the occurrence of physical health problems. This procedure not only reduces variance in the index attributable to physical illness but allows examination of the effects of other life events upon distress independent of the effects of health changes. In no previous study have researchers controlled for the effects of physical health changes.

In summary, none of the problems with the MacMillan index seems to be serious. The index has been shown to be positively and significantly associated with stressful conditions (Seiler, 1973). I concur with several authors (Myers et al., 1971; Seiler, 1973; Spiro et al., 1972; Tousignant et al., 1974) who conclude that the instrument is indeed interpretable as a measure of mild psychological impairment, or psychological distress.

The items on the distress index are shown in Table 2. Symptomatic responses are scored as indicated and summed. The total score ranges from 18 (little or no distress) to 71 (high distress). Cronbach's (1951) Alpha, a measure of internal consistency for the scale, is satisfactorily high, ranging from .80 to .85 for the groups analyzed.

The psychological distress index was administered four times during the experiment. Data from only the first two administrations are available now. For male heads, the first measure (Time 1) was taken approximately 4 months after enrollment in the experiment, and the second (Time 2) approximately 20 months into the experiment. For female heads in general, the Time 1 measure was taken 8 months after enrollment, and the Time 2 measure 24 months after enrollment. (The administrations of the index were staggered by sex to preclude contamination from spouses' responses.) For reasons explained below, only the Time 2 measures are used in this paper, and only those individuals who responded to the distress index at Time 2 are analyzed.

Table 2

PSYCHOLOGICAL DISTRESS INDEX

	<u>Never</u>	<u>Seldom</u>	<u>Some- times</u>	<u>Often</u>	
*1. How often do your hands tremble enough to bother you?	1	2	3	4	
*2. How often do you smoke?	1	2	3	4	
*3. How often do your hands or feet sweat so that they feel damp and clammy?	1	2	3	4	
*4. How often are you bothered by your heart beating hard?	1	3	4	4	
*5. How often do you have cold sweats?	1	2	3	4	
*6. How often do you feel that you have several different ailments in different parts of your body?	1	2	3	4	
*7. How often do you lose your appetite?	1	2	3	4	
*8. How often has ill health affected the amount of work you do?	1	2	3	4	
*9. How often do you have weak spells?	1	2	3	4	
*10. How often do you have spells of dizziness?	1	2	3	4	
*11. How often do you tend to lost weight when important things are bothering you?	1	2	3	4	
*12. How often are you bothered by nervousness?	1	2	3	4	
*13. How often have you been bothered by shortness of breath when you were not exercising?	1	2	3	4	
*14. How often do you tend to feel tired in the mornings?	1	2	3	4	
*15. How often do you have trouble getting to sleep and staying asleep?	1	2	3	4	
		<u>Not Very Much</u>	<u>Pretty Often</u>	<u>Nearly All the Time</u>	
*16. How often are you bothered by having an upset stomach?	<u>Never</u>	1	2	3	4
		<u>Never</u>	<u>A Few Times</u>	<u>Many Times</u>	
*17. How often have you been bothered by nightmares or dreams which frighten or upset you?	1	2	3		
		<u>Very Good Spirits</u>	<u>Good Spirits</u>	<u>Low Spirits</u>	<u>Very Low Spirits</u>
18. In general, would you say that most of the time you are in:	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	

* Item is similar to MacMillan Health Opinion Survey item (MacMillan, 1957).

Measurement of Life Events

Life events obtained from the SIME/DIME data base are shown in Table 3. For parsimony the life events in Table 3 are summed within the categories shown. Five variables result: family gains, family losses, occupational gains, occupational losses, and health problems. Each of these variables is the sum of all such events reported by the individual. Only events that occurred between enrollment and the Time 2 measure of distress are summed. Because I lack subjects' own assessments of these experiences, I categorize events as gains or losses on the basis of their general social desirability. As mentioned previously, this classification allows examination of the independent effects of desirable and undesirable life changes upon distress. Note that family and occupational changes are examined as separate variables. Prior work with this data has shown that family and occupational events have inconsistent effects upon women (Thoits, 1978). As discussed above, the health problems variable is included to reduce the variance in psychological distress attributable to physical illness. It also enables examination of the effects of other life events upon distress independent of the effects of health changes.

An important feature of these data must be mentioned. Previous life event studies have been strongly criticized for their reliance upon subjects' ability to recall events which occurred during the past 1 or 2 years (Paykel, 1974a,b; Hudgens, 1974, 1970). The reliability of recall over such long periods is certainly questionable. It is also possible that recall is distorted or biased toward negative events, especially if subjects seek to explain an illness in terms of past events (Brown, 1974). The advantage of using SIME/DIME data is that information was gathered about particular events (marital and family changes, job and educational changes, changes of income) at every interview (i.e., every 4 months). Thus, the period of recall for subjects is relatively short. Further, the data about these objective changes were gathered for administrative reasons rather than for the explanation of changes in mental health, so this source of potential bias in recall was avoided.

Table 3

LIFE EVENTS EMPLOYED IN THE ANALYSIS

Family Gain Events

Reconciliation/remarriage
Pregnancy (or spouse pregnant)
Birth of child
Child or children arrive
Other family member arrives

Family Loss Events

Separation/divorce
Child or children leave
Other family member leaves
Major residential move
Death of spouse
Death of child or other family member
Spouse suffers health problems

Occupational Gain Events

Begin employment
Begin school/training
Increase in occupational status
Spouse begins employment
Increase in spouse's occupational status
Family income increases 50%

Occupational Loss Events

Employment ends
End school/training
Decrease in occupational status
Spouse's employment ends
Decrease in spouse's occupational status
Family income decreases 50%
Retirement
Institutionalization

Health Problem Events

Chronic illness or disability
Major illness or accident
Other illnesses or accidents
 requiring hospitalization
Sickness during past 6 months

Analysis Strategy

To estimate experimental effects I adopt a simple analysis strategy. I regress levels of distress on experimental treatment variables, life events, variables that determine assignment to experimental treatments,* and other variables known to affect distress (age, education, occupation, etc.). This strategy relies heavily on the experimental nature of the study. When the assignment variables are held constant, the control group and the experimental groups should not differ systematically on initial levels of distress. If they differ significantly later in the experiment, I can infer that the experimental treatment(s) caused a change in the psychological state of the experimental subjects.

This study represents an improvement over prior work in the life events area. Most previous investigators have compared the life experiences of psychiatric patients to those of nonpatient controls. In these studies, comparison groups have been selected on the basis of the dependent variable. This selection procedure can produce biased estimates of the effects of life events upon psychological state. A proper test of life events theory contrasts the distress levels of two samples that are comparable in all respects but one: one sample has experienced a major life change and the other sample has not. If the distress levels of the two groups differ significantly at some later time, the investigator can infer that a major life change does produce a change in psychological state. The present study follows this latter design. Subjects who have received an income guarantee are compared to subjects who have not. As a further refinement, the effects of other intervening life events are controlled in order to estimate the direct effect of this financial change upon distress.

* Because assignment, or stratification, variables covary with experimental treatment, they must be controlled in the analysis. The assignment variables are: marital status, race-ethnicity, preexperimental family income, and site.

The general form of the model to be estimated is:

$$\begin{aligned} \text{Distress} = & \alpha_0 + \underline{\alpha_1} (\underline{\text{Background Variables}}) \\ & + \underline{\alpha_2} (\underline{\text{Treatment Variables}}) + \\ & \underline{\alpha_3} (\underline{\text{Life Events}}) + \mu \quad , \end{aligned} \quad (1)$$

where underlining indicates vectors of coefficients or variables, and μ is a random disturbance term. The direct effects of the experimental treatments are measured by $\underline{\alpha_2}$. The effects of intervening life events are given by $\underline{\alpha_3}$.

Each regression contains the same set of nonexperimental variables that I refer to in Equation 1 as the "background variables." The background variables are variables that were used to assign the families to experimental treatments as well as several other variables that the literature shows to be related to distress. The latter are included to reduce the sampling variability of estimated experimental effects. The background function includes:

Assignment Variables

1. Denver (0,1)
2. Black (0,1)
3. Chicano (0,1)
4. Marital status of head (0 = single, 1 = married)
5. Normal preexperimental family income: Expected income of the family in the year before the experiment, assuming normal family circumstances and adjusted for family size. It includes all money and in-kind earnings from paid work and family business, but omits transfer payments. There are seven categories and a residual category for families not assigned to an income level. This set of categorical variables is used in the analysis instead of actual earnings because it was used in the assignment of families to experimental treatments. Each income category appears as a dummy variable (0,1) except the last two categories (\$9,000+), which serve as the omitted comparison group. [For single women, the last three categories serve as the comparison group (\$7,000+).]

Other Variables

6. Age at enrollment (in years)
7. Education at enrollment (years of formal schooling)
8. Parenthood: Family has one or more children (0,1)
9. Occupational status at enrollment: 0 = not employed; 1 = service and private household workers; 2 = laborers, farm laborers, farm owners and managers; 3 = operatives; 4 = craftspersons; 5 = sales and clerical workers; 6 = managers; 7 = professional and technical workers
10. Spouse's occupational status at enrollment: As above (for those married at enrollment)
11. Working at enrollment (0,1)
12. Spouse working at enrollment (0,1) (for those married at enrollment).

I represent the income maintenance treatment in the following ways. In preliminary analysis (not reported here), I found that length of treatment (3 or 5 years) interacted with the effect of the financial treatment and with race-ethnicity. Consequently, I represent the experimental treatments as follows:

- a. F3*White White 3-year experimental subject (0,1)
- b. F5*White White 5-year experimental subject (0,1)
- c. F3*Black Black 3-year experimental subject (0,1)
- d. F5*Black Black 5-year experimental subject (0,1)
- e. F3*Chicano Chicano 3-year experimental subject (0,1)
- f. F5*Chicano Chicano 5-year experimental subject (0,1)

With the inclusion of these six interaction terms, differences between the racial-ethnic groups become nonsignificant and the samples can be pooled. I also add a variable that estimates the yearly income maintenance payment a family would receive if family members did not change their preenrollment behavior (including labor supply and family composition). I refer to this variable as "payment." Payment measures the magnitude of the change in disposable income a family experiences upon being

assigned to an income maintenance treatment. The six interaction terms and payment constitute the set of experimental treatment variables.*

As stated previously, the model in Equation 1 is estimated with ordinary least-squares regression. The dependent variable approximates the distribution appropriate for the assumptions underlying the usual regression sampling theory (normal distribution). Moreover, because assignment to experimental treatment is random, conditional on the background variables included in the model, the seven experimental treatment variables should be independent of the disturbance. Under these conditions, ordinary least squares gives the best linear unbiased estimates of experimental effects.

Because significant site, sex, and marital status interactions were found in previous work (Thoits and Hannan, 1978), I analyze married men, married women, and single women separately by site. Individuals who are totally and permanently disabled did not answer the psychological distress index and therefore are omitted from the analysis.

* Analysis not reported here indicates that replacing these treatment variables with a larger set of support level and tax-rate dummies does not improve the fit of the model.

III RESULTS

Because the time to the first observation of distress is short and relatively few life events occur in this period, the analysis which follows is based on the period from enrollment to the second observation of distress, an interval of 20 and 24 months for males and females, respectively. Table 4 reports the mean number of events which occurred during this period for each analysis group.

In Table 4, husbands and wives experience roughly similar numbers of life events. Single women experience fewer family losses, occupational gains, occupational losses, and total events overall than married individuals. This is probably due to the absence of a spouse and changes associated with a spouse. Interestingly, single women report the highest number of physical health problems. This finding is consistent with the literature, which shows that formerly married individuals have higher rates of physical illness as well as higher rates of psychological disorder (see Gove, 1972, for a review). The means of total life events are somewhat higher in Seattle groups than in Denver groups. This appears to be because of the slightly greater numbers of occupational gains and losses

Table 4

MEAN NUMBER OF LIFE EVENTS EXPERIENCED
FROM ENROLLMENT TO TIME 2

	<u>Married Men</u>		<u>Married Women</u>		<u>Single Women</u>	
	<u>Denver</u>	<u>Seattle</u>	<u>Denver</u>	<u>Seattle</u>	<u>Denver</u>	<u>Seattle</u>
Family gains	.46	.45	.53	.50	.53	.52
Family losses	.96	.87	1.03	1.02	.57	.44
Occupational gains	2.33	2.72	2.23	2.45	1.71	1.95
Occupational losses	1.42	1.66	1.81	1.78	1.07	1.21
Health problems	.68	.68	.70	.65	.88	.91
Total events	5.85	6.38	6.30	6.40	4.75	5.03
Sample size	1,228	793	1,251	820	889	615

experienced by the Seattle groups. Perhaps these figures reflect the uncertain employment situation in Seattle during the first two years of the experiment (1970-71). During that time the total unemployment rate in Seattle rose from 9.7% to 12.4% then fell the following year to 10.8% (Manpower Report of the President, 1978).

When the incidence of life events in each analysis group in Table 4 is broken down by control and experimental group status, no striking differences between the samples appear. In general, there is a tendency for the experimental groups to have experienced somewhat higher numbers of life events than the control groups. This is consistent with the higher divorce, unemployment, migration, and fertility rates for experimental subjects reported in previous SIME/DIME studies.

Experimental Effects

To determine the effect of the income maintenance experiment upon distress net of the effects of intervening life events, I estimate Equation 1 in two steps. In Step 1, I regress distress at Time 2 on background variables, treatment variables, and the life event measures for family and occupational changes. In Step 2, I add health problems to the equation. For each analysis group in Table 5, the results of each step are shown in Columns 2 and 3, respectively. For comparative purposes, Column 1 shows the total experimental treatment effects found in my previous work (from Table 1).

Comparing the Column 1 coefficients to the Column 2 coefficients in Table 5, we see that control for intervening family and occupational events does not alter the pattern of significant experimental effects at all. Each group that responded to the experiment with significantly increased distress continues to exhibit this response when family and occupational changes are controlled. Thus, the experiment appears to have a direct, positive impact upon distress in these particular groups. However, when physical health problems are controlled (Column 3), three of the eight originally significant coefficients become nonsignificant. In these three groups (white wives, Chicana wives, and black single women

Table 5
 EXPERIMENTAL TREATMENT EFFECTS UPON PSYCHOLOGICAL DISTRESS^a AT TIME 2^b,
 LIFE EVENTS AND HEALTH PROBLEMS CONTROLLED[†]

Treatment Variables	Married Men [#]					
	Denver			Seattle		
	(1)	(2)	(3)	(1)	(2)	(3)
F3 * White	.08	.06	.21	-.53	-.57	-.28
F5 * White	3.22***	3.06***	2.41**	-1.04	-1.09	-.37
F3 * Black	-.03	-.07	-.14	2.17**	2.20**	2.37**
F5 * Black	.14	.10	-.14	-1.12	-1.05	-.23
F3 * Chicano	1.04	.95	.90			
F5 * Chicano	.48	.45	.22			
Payment (in thousands of dollars)	-.06	-.04	-.12	.36	.35	.18
Life Events						
Family Gains		.04	-.02		-.06	.05
Family Losses		.32*	.23		.31	.14
Occupational Gains		-.07	-.10		-.09	-.09
Occupational Losses		.29	.28		.14	.11
Health Problems			2.24***			2.32***
R ²	.06	.06	.14	.13	.13	.21
F-test for gain and loss events		1.63			.49	
F-test for health problems (Degrees of freedom)		(4,1201)	(1,1200)		(4,769)	(1,768)
Number of cases	1,228	1,228	1,228	793	793	793
Married Women [#]						
Treatment Variables	Denver			Seattle		
	(1)	(2)	(3)	(1)	(2)	(3)
	F3 * White	.63	.68	.66	1.19	1.09
F5 * White	2.42**	2.15*	1.34	1.16	1.00	1.14
F3 * Black	-.33	-.32	-.35	.05	-.09	.18
F5 * Black	2.91**	2.90**	2.83**	-1.03	-1.02	-.64
F3 * Chicano	.95	.96	.45			
F5 * Chicano	2.06*	1.97*	1.68			
Payment (in thousands of dollars)	-.37	-.41	-.26	.12	.13	-.002
Life Events						
Family Gains		.29	.24		.52	.25
Family Losses		.66***	.52**		.59**	.49**
Occupational Gains		.06	-.07		-.13	-.10
Occupational Losses		.08	.19		.07	.06
Health Problems			2.36***			2.70**
R ²	.05	.06	.13	.08	.09	.18
F-test for gain and loss events		3.40***			2.48**	
F-test for health problems (Degrees of freedom)		(4,1224)	(1,1223)		(4,796)	(1,795)
Number of cases	1,251	1,251	1,251	820	820	820
Single Women [#]						
Treatment Variables	Denver			Seattle		
	(1)	(2)	(3)	(1)	(2)	(3)
	F3 * White	.20	.29	.96	-.79	-.67
F5 * White	-.51	-.51	1.01	1.51	1.52	1.49
F3 * Black	2.28**	2.35**	2.54**	2.42*	2.57**	2.79**
F5 * Black	2.69**	2.73**	1.45	-.68	-.56	.11
F3 * Chicano	-1.10	-.97	-.84			
F5 * Chicano	.32	.49	.12			
Payment (in thousands of dollars)	.08	.10	.14	.46	.47	.44
Life Events						
Family Gains		.38	.51		.31	.35
Family Losses		-.11	-.06		.66	.41
Occupational Gains		-.02	-.24		-.53**	-.46*
Occupational Losses		.30	.33		.54	.56*
Health Problems			3.68***			2.51***
R ²	.04	.05	.24	.05	.07	.19
F-test for gain and loss events		.41			2.03*	
F-test for health problems (Degrees of freedom)		(4,866)	(1,865)		(4,595)	(1,594)
Number of cases	889	889	889	615	615	615

*.10 ≥ p > .05 **.05 ≥ p > .01 ***.01 ≥ p

[#]"Married" indicates married or cohabiting individuals. "Single" refers to divorced, separated, and widowed persons.

[†]Background variables are controlled in each equation.

^aA positive regression coefficient indicates increased distress. A negative coefficient indicates decreased distress.

^bTime 2 occurs roughly 20 months after enrollment for males, 24 months after enrollment for females.

enrolled in the 5-year treatment in Denver), the effect of the experiment is apparently indirect through its impact upon physical health problems. The experiment continues to have a direct impact upon distress in five of the eight original groups: white 5-year husbands in Denver, black 3-year husbands in Seattle, black 5-year wives in Denver, and black 3-year singles in both Denver and Seattle. The experimental effect is positive in each case. In the other groups in Table 5 neither the experimental treatments nor payment has significant net effects.

The undesirability hypothesis predicted that the experiment would have no direct effect on distress or would reduce distress somewhat, while the total change hypothesis implied an increase in distress. The results in Table 5 overwhelmingly support the undesirability prediction. In the large majority of groups no significant experimental effects were found. It is puzzling, then, that the experiment significantly raises the distress levels of five groups (four of which are black) despite control for intervening life events. These results suggest that culturally desirable events may not always have desirable consequences. This issue will be pursued in the following sections.

Effects of Other Life Events

Although family and occupational events did not explain the positive experimental effects found in the particular groups above, their effects on distress warrant further consideration. From examination of the results in Table 5 several conclusions may be drawn for life events theory in general. In brief, these conclusions are:

- Family and occupational life events add little to explained variance; health problems account for much more variance in distress.
- The undesirability hypothesis better predicts distress than the total change hypothesis.
- Life events vary in their impact by sex and marital status.

Each of these generalizations will be discussed in turn.

Examine the F-ratios for the set of family and occupational events in Column 2 for each group in Table 5. This set of life events does not

add significantly to explained variance in distress for husbands or for Denver single women. The events do contribute significantly to explained variance for wives and for Seattle singles. However, a comparison of the R^2 values in Columns 1 and 2 for these women indicates that at most 2% is added to explained variance because of these events. This contribution is hardly impressive. Note that the F statistics for health problems is significant in all groups. This variable doubles the R^2 values, adding 7% to 19% to explained variance in distress. Were it not for the problem of a possible health bias in the dependent variable, one would have to conclude that illnesses and injuries are far more important determinants of psychological distress than other types of events. However, because many items on the distress index employed in this study are physiological in nature, I cannot rule out the possibility that the index is simply reflecting physical rather than psychological illness. From these results I concur with B. P. Dohrenwend (1974) that it is vitally important to keep health problems events analytically distinct when examining the relationship between events and distress. (It seems imperative that researchers in this area develop measures of psychological disturbance which are free of potential health bias.) In sum, family and occupational events do not add significantly to explained variance in distress for males and add only small amounts of explained variance for females. Serious health problems contribute heavily to variance in distress. Whether this effect is a life change effect or is simply due to a health bias in the dependent variable has yet to be determined.

The results in Table 5 help resolve the general question of whether undesirable change or total amount of change best predicts distress. Confining attention to Column 3 in Table 5, for married men we see that gain events are near-zero in effect while loss coefficients are somewhat larger and positive. When health problems are controlled none of these event types are significantly related to distress. The pattern of effects does support the undesirability hypothesis, however.

For married women we have somewhat different results. Family losses and gains increase psychological distress, although only family loss coefficients are significant. Occupational gains and losses are near-zero

in effect, but their signs are generally those predicted by the undesirability hypothesis. Because only loss events significantly increase distress, these results support the undesirability hypothesis. (However, these patterns of family and occupational effects warrant further attention. I return to them below.)

Like married women, single women respond with increased distress to family gains. Family losses are near-zero in effect for Denver singles but are distressing to Seattle singles. (These patterns will be discussed further below.) Occupational gains clearly reduce distress for singles in both cities while occupational losses augment it. Occupational coefficients are significant for singles in Seattle. The undesirability hypothesis is supported once again.

Overall, the undesirability hypothesis better predicts psychological state than the total change hypothesis. Loss events produce distress while gain events typically either relieve distress or have no significant effect. Only in the case of income maintenance do gain events significantly increase distress, as the total change hypothesis predicts.

From the patterns of family and occupational effects it appears that life events affect women somewhat differently from men. These life changes add significantly to explained variance in distress for women but do not for men. Family gain events appear to distress women, in contrast to men who are relieved or show no effect at all. There also appear to be differences between married and single women; occupational events clearly influence singles but have near-zero effects on wives.

What are the causes of these apparent differences by sex and marital status? For exploratory purposes I represented life events differently in Equation 1. I created a dummy variable for each life event listed in Table 3. (I excluded, however, death of spouse, death of child, retirement, and institutionalization. These events are so rare that estimates of their effects are highly unstable.) A value of 1 indicates one or more experiences of the particular event and a value of 0 indicates no experience of the event. I replaced family and occupational variables in Equation 1 with this larger set of event dummies. This substitution

allows me to investigate the effects of particular life changes in each group. The results of the analysis are shown in Table 6.

It would be misleading to discuss the meaning of specific nonsignificant coefficients in Table 6; the standard errors of these variables tend to be large.* I restrict attention here to the significant coefficients.

In Table 6 six experimental treatment groups show significantly raised distress. Five of these groups are those that had significant positive responses in Table 5 (white husbands in Denver and the four black groups). Chicana wives enrolled in the 5-year treatment in Denver also show significantly increased distress when events are individually controlled. (Chicana wives were among the eight groups originally found to respond positively to the experiment.)

F-tests for the difference between equations shows that for husbands and singles the life event dummies do not improve the fit of the previous model (in Column 3 of Table 5). For wives, controlling for particular events significantly improves the fit of the model.

Disaggregation of gain and loss variables reveals that only one or two events significantly affect husbands and singles.† For wives, several events have significant effects upon distress. Thus, married women appear to be more responsive to life change than husbands or single women. In most cases, significant effects are those predicted by the undesirability hypothesis. But disaggregation discloses that certain culturally desirable events have unpleasant consequences for wives. Remarriage or reconciliation (the majority of these events are reconciliations) is significantly disturbing to wives in both Denver and Seattle. For Seattle wives, the

*The spouse coefficients for single women are particularly unstable, as so few women single at enrollment remarry by Time 2 (N=68 in Denver, N=81 in Seattle).

†A 50% increase in family income significantly raises the distress level of Seattle husbands. The effect of this gain is nonsignificant but positive for all other groups but married women. As with income maintenance, a positive change in financial circumstances tends to be distressing to some groups.

Table 6
EFFECTS OF PARTICULAR LIFE EVENTS
UPON PSYCHOLOGICAL DISTRESS AT TIME 2[†]

Treatment Variables	Married Men ^a		Married Women ^a		Single Women ^a	
	Denver	Seattle	Denver	Seattle	Denver	Seattle
F3 * White	.34	-.26	.62	.50	.78	-.75
F5 * White	2.52**	-.49	1.47	.98	.66	1.10
F3 * Black	-.12	2.51**	-.29	.17	2.28**	2.57**
F5 * Black	-.003	.03	2.77**	-.98	1.27	-.15
F3 * Chicano	1.00		.70		-1.02	
F5 * Chicano	.45		2.08*		.15	
Payment (in thousands of dollars)	-.16	.03	-.25	.22	.20	.23
Family Gain Events						
Reconciliation/remarriage	.54	-.02	3.35**	3.63**	1.51	.37
Pregnancy (spouse pregnant)	-1.09	.43	-.78	1.62	1.45	-3.50
Birth of child	.36	.64	.92	-1.90*	-.74	2.50
Child or children return home	-.93	-.02	-1.03	.43	.71	1.07
Other family member arrives	-2.36	-4.75	.53	5.05*	.69	-.82
Family Loss Events						
Separation/divorce	-.47	.57	-.01	.91	-.23	1.18
Child or children leave home	.50	-.02	2.01**	.43	-.30	.62
Other family member leaves	1.63*	-1.26	.89	-.58	1.63*	.06
Major residential move	-.81	6.31	-1.38	2.40	-2.18	--
Spouse suffers health problems	.24	.33	.11	.39	-1.13	2.08*
Occupational Gain Events						
Begin employment	-.39	-.13	.80	-1.24*	-.43	-.60
Begin school, training	-.88	.48	.12	-.56	-.04	-.99
Increase in occupational status	-.05	-1.62**	-1.39*	.45	-1.28	-3.59***
Spouse begins employment	-.17	.66	-.21	1.05	-.35	-1.53
Increase in spouse's occupational status	1.04	.94	.13	-.31	-3.14	3.91
Family income increases 50%	.50	1.58**	-.31	-.83	.19	.97
Occupational Loss Events						
Employment ends	.68	.59	.34	.37	.38	.75
End school, training	.54	-.83	.27	-.92	-1.13	-.18
Decrease in occupational status	.46	-.11	.62	1.88	1.55*	.63
Spouse's employment ends	.40	.03	-.19	.59	2.90	-2.82
Decrease in spouse's occupational status	-1.29	.17	-.59	-.59	-2.29	3.80
Family income decreases 50%	-.71	-.29	2.39***	-.30	.21	.88
Health Problems						
	2.29***	2.26***	2.37***	2.66***	3.64***	2.50***
<hr/>						
R ²	.15	.24	.15	.21	.25	.21
F-test for set of life event dummies	.66	1.25	1.73**	2.25***	1.08	.98
(Degrees of freedom)	(18,1182)	(18,750)	(18,1205)	(18,786)	(18,847)	(17,580)
Number of cases	1,228	793	1,251	829	889	618

* .10 ≥ p > .05

** .05 ≥ p > .01

*** .01 ≥ p

All background variables are controlled in the equation.

^aAt enrollment.

arrival of another family member is also highly distressing. These effects help explain why the family gain coefficient in Table 5 is positive for married women. Summation of events by presumed desirability obscured these strong distressing impacts.

IV DISCUSSION AND CONCLUSIONS

Why do some groups respond to income maintenance with increased distress while most do not? Why do wives appear to be more responsive to life changes than husbands or singles? Why do family gain events stress wives and, to some extent, singles? Why do occupational events affect single women more than wives or husbands (see Table 5)? It is possible that the social situation of the individual partially determines the effect of life events. For example, there is some evidence that sharing whatever one has with relatives and friends is a norm in the lower class (Hannerz, 1969; Lewis, 1959, 1961, 1965; Rogler and Hollingshead, 1965). This appears to be especially true of black and Spanish-speaking groups. If such a norm operates, then it may be that individuals in such groups who are enrolled in income maintenance programs experience increased demands upon their financial resources from relatives and friends, frustrating their hopes of getting ahead. The finding that black groups more often respond to the experiment with significantly increased distress is consistent with this possibility. The individual's social situation may indeed mediate the effect of a particular event.

Sex and marital status differences in response to events might also be explained by social situational factors. Wives' special vulnerability is a case in point. As Gove (1972) points out, husbands are typically involved in two major social networks, one at home and one at work. Wives have only one major network and source of gratification, the family. If a man finds one of his roles unsatisfactory, he can reinvest himself in the other. But the woman often has no other major alternative source of gratification. Furthermore, the housewife's role is characterized by financial dependence upon the spouse, repetitive and unrewarding tasks, and isolation from meaningful adult interaction; i.e., the housewife's role is low in power, prestige, and reward. Even if the wife works she is at a disadvantage, because more often than not she is underpaid. Demands on her time and energy also increase because the working wife

typically performs most of the household chores as well as her job (Bahr, 1974; Gove, 1972). These circumstances may leave the wife far more vulnerable to life changes, regardless of their desirability.

The single woman is in a position somewhat different from that of the wife. She is the sole head of household and provider for her family, and it seems reasonable that occupational changes will have significant effects upon her distress level. Family gain events can only add to her burden; family losses may have conflicting effects, because they may lessen the demands upon her time and energy but also isolate her further.

Much previous life events research has focused upon the properties of events which cause distress. Investigators have examined the desirability of events (B. S. Dohrenwend, 1973; Gersten et al., 1974; Pearlin and Lieberman, 1977), their severity or intensity (Holmes and Masuda, 1974), and the degree to which individuals anticipated or had control over the occurrence of the event (Dohrenwend, 1970). Implicit in this research is the assumption that life events affect all individuals in the same way. Only recently have investigators begun to realize that the effects of events may also vary with the social and psychological characteristics of individuals. A few researchers have begun to examine the social conditions which mediate the effects of life events. A factor which is receiving special attention is the individual's degree of social support, or social integration (Dean and Lin, 1977; Kaplan, Cassel, & Gore, 1977; Myers et al., 1975; Nuckolls, Cassel, & Kaplan, 1972; Thoits, 1978; Wells, House, Michael, & Kaplan, 1976). The findings in this report underscore the need to identify not only the characteristics of events but the characteristics of social situations that make life changes more or less distressing.

To summarize, I have found that life events add very little to explained variance in distress for husbands in this sample. Life events add significantly to explained variance for wives and single women, but the increment (2%) is hardly a substantial one. In contrast, serious health problems account for a large amount of variance in psychological distress in all groups. These results cast some doubt upon the strong

positive relationship between events and psychological disturbance reported in the literature. The relationship may be due to a health bias in measures of distress, or it may reflect the possibility that health problems are the primary determinants of distress. In either case, these results suggest that physical illnesses and injuries should be kept analytically distinct from other life changes in future research.

The preceding analysis also indicates that gains and losses should be analyzed separately, as in general they have opposite (though often nonsignificant) effects on distress. This finding supports the undesirability hypothesis over the total change hypothesis. However, gain and loss events do not always have effects predicted by the undesirability hypothesis. For example, some gain events (such as an increase in family income due to income maintenance, a reconciliation with spouse, the addition of a family member) appear to cause distress rather than alleviate it. Unexpected effects are particularly evident for married and single women. Effects that differ by sex and marital status, coupled with the fact that life events explain more variance for women than men, suggest that the social situation of the individual may mediate the effect of life changes. Future research must identify not only stressful properties of life events but the social conditions under which particular events will be distressing.

Other Problems for Investigation

Several additional problems require the attention of life event researchers. I mention them here only briefly. These include the problems of event selection, event clustering, multiple contradictory effects, and dynamic effects. Each will be discussed in turn.

Life event researchers have failed to justify theoretically the selection of particular life events for inclusion on scales. From a universe of life happenings, experiences as diverse as divorce, taking on a mortgage, and the death of a pet have been selected as indicators of life

change, apparently on face validity alone.* Clearly, the readjustment required by divorce differs in important ways from the readjustment required by, for instance, a major accident or an increase in income. Distinctions between types of change must be made and indicator events must be selected on theoretical grounds. Theoretically delineated changes may then be tested for their predictive utility. Only when research is guided by theory will we obtain an improved understanding of the relationship between events and distress.

Researchers in this area have typically regarded life events as independent, unrelated experiences. But a major life change may cause other changes. For example, for a male head of household, divorce or separation typically entails a residential move, loss of contact with his children, isolation from former friends, and even a decrease in income if his wife is employed. A major illness or accident may cause a decrease in income, an increase in debts, and the development of other physical complications. Prolonged ill health may eventuate in the loss of a job. Thus, a major life change may have a "ripple effect," causing additional events to occur within a short period.

This possibility has several implications. The effects of one event may easily be confounded with the effects of events which are its consequences. Not only may an event have multiple consequences, but the effects of these consequences may be contradictory. For example, a divorce may free a woman from an unhappy marriage but leave her in dire financial straits. It may be difficult to disentangle these effects because of their collinearity. Researchers must be aware of this possibility and exercise caution in interpreting the effects of particular life events. It also seems reasonable to suggest that the number of events experienced in a particular period may be less important than the intervals between the events. Events which cluster together in time may have more distressing

* Many other seemingly stressful events have been omitted. For example, events such as being the victim of a felony, obtaining an abortion, or having a child accused of a crime have never appeared on life event scales. Omissions require justification as well.

effects than the same events occurring singly. This possibility deserves attention in future research.

The question of time raises one last issue. Most investigators have considered only effects of events upon equilibrium levels of distress. If the adjustment to a new equilibrium after an event is very rapid, then a comparative statics framework suffices. However, if adjustment requires relatively long periods, or if there are variations in adjustment over time, a dynamic approach must be taken. The literature offers very little information about adjustment periods in response to major life changes. Casual observation suggests that for most events there is an initial sharp increase in distress, followed by a decline to an equilibrium either above or below the individual's initial level of distress. Whether the equilibrium is above or below the initial state probably depends upon the desirability of the event. There is also some indication of an "anniversary effect" for a few events such as the death of a loved one. Distress increases markedly on the anniversary date of the event (Brown, 1970). The main implication of these considerations is that the average level of distress observed for a group may depend greatly upon the timing of the distress observations. For example, consider the situation depicted in Figure 1. It illustrates a plausible set of distress cycles for several persons who have experienced at three different times the same major event, marriage. For simplicity, I present a case in which individuals make one major adjustment and settle at a new, lower equilibrium. For simplicity I assume also that adjustment rates do not vary by race, sex, income, or other social characteristics (although clearly such variation is possible--a further complication to be considered). In this situation one would arrive at very different conclusions regarding the effect of marriage upon psychological distress by choosing A, B, or C for the observation of distress. At A, newly marrieds are all above singles, so there appears to be a positive relationship between marriage and distress. At B, the average levels of distress of newly marrieds do not differ from singles. By C, the newly marrieds are below singles. It is very possible that the effects of particular events (including financial changes due to income maintenance) vary with time. Further,

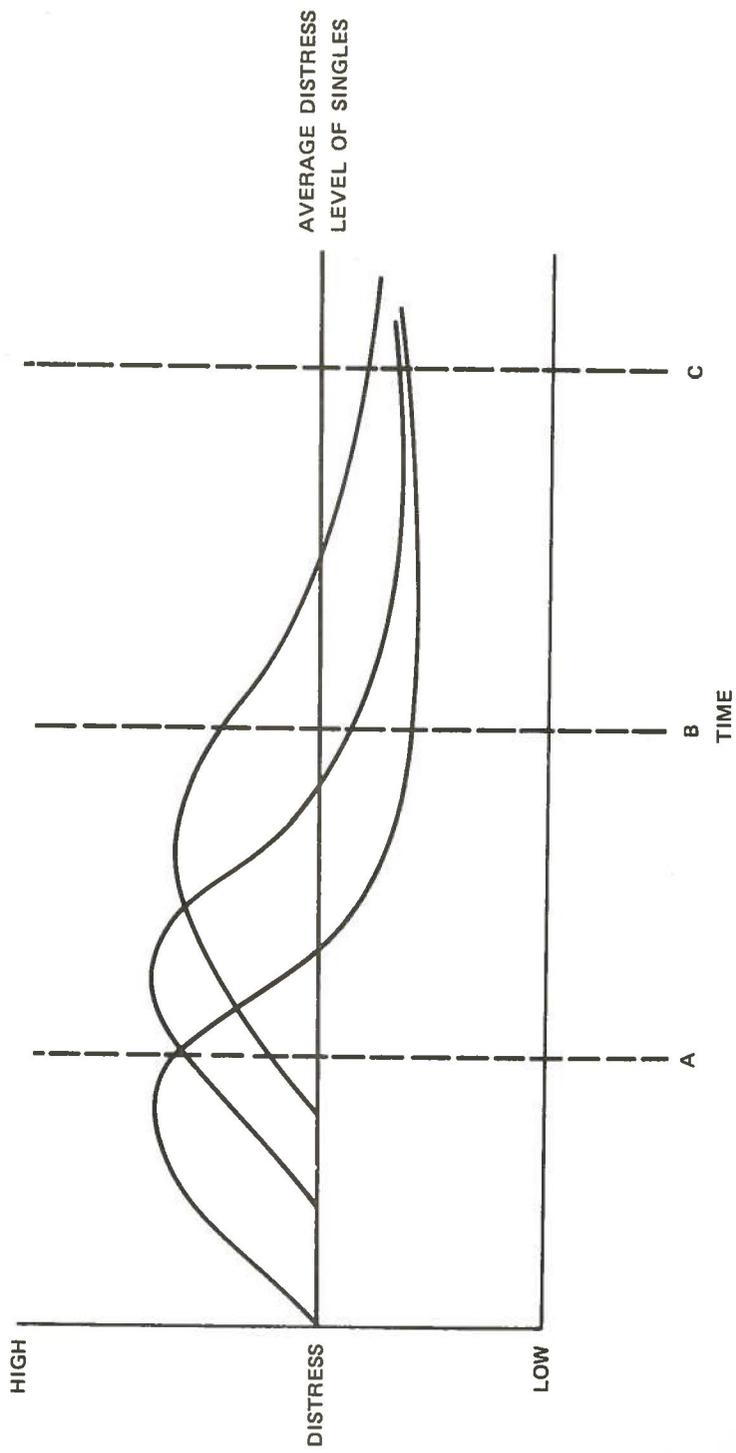


FIGURE 1

these variations may well depend upon other social characteristics (age, race, sex, etc). These possibilities lead one to expect very complex patterns of effects upon distress. To specify the effects of major life events upon psychological distress in future research accurately, it may be necessary to develop and test a number of such dynamic models.

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18	<i>The Design of the Seattle and Denver Income Maintenance Experiments</i> , M. Kurz and R. G. Spiegelman, May 1972.
19	<i>The Payment System for the Seattle and Denver Income Maintenance Experiments</i> , M. Kurz, R. G. Spiegelman, and J. A. Brewster, June 1973.
21	<i>The Experimental Horizon and the Rate of Time Preference for the Seattle and Denver Income Maintenance Experiments: A Preliminary Study</i> , M. Kurz, R. G. Spiegelman, and R. W. West, November 1973.
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23	<i>Measurement of Unobservable Variables Describing Families</i> . N.B. Tuma, R. Cronkite, D. K. Miller, and M. Hannan, May 1974.
24	<i>A Cross Sectional Estimation of Labor Supply for Families in Denver 1970</i> , M. Kurz, P. Robins, R. G. Spiegelman, R. W. West, and H. Halsey, November 1974.
25	<i>Job Search: An Empirical Analysis of the Search Behavior of Low Income Workers</i> , H. E. Felder, May 1975.
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27	<i>A Study of the Demand for Child Care by Working Mothers</i> , M. Kurz, P. Robins, and R. G. Spiegelman, August 1975.
28	<i>The Impact of Income Maintenance on the Making and Breaking of Marital Unions: Interim Report</i> , M. Hannan, N. B. Tuma, and L. P. Groeneveld, June 1976.
29	<i>The Estimation of Labor Supply Models Using Experimental Data: Evidence from the Seattle and Denver Income Maintenance Experiments</i> , M. C. Keeley, P. K. Robins, R. G. Spiegelman, and R. W. West, August 1976.

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- 30 *Determinants and Changes in Normative Preferences of Spouses*, R. C. Cronkite, May 1977.
- 31 *Homogamy, Normative Consensus, and Marital Adjustment*, R. C. Cronkite, May 1977.
- 32 *The Determinants of Participation of Single-Headed Families in the AFDC Program*, Arden Hall, May 1977.
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