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BELONG TO METRO, SUNY LAND, AND

Occupational Stress and Preemployment Measures of Depressive Symptoms: The Case of Teachers

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This paper describes difficulties involved in conducting cross-sectional and longitudinal research on the effects of occupational stressors on the mental and physical health of veteran workers. The difficulties include (1) the disappearance, prior to the beginning of the investigation, of the casualties of job stress (Kasl, 1983) and (2) selection bias. The paper advances the view that an alternative design, one that (1) follows new workers longitudinally, (2) includes preemployment measures of mental and/or physical health, and (3) controls for nonoccupational stressors, is warranted. The alternative design allows for (a) the assessment of the extent to which the findings are vulnerable to selection-based and event-proneness explanations (Dohrenwend and Dohrenwend, 1981) and (b) the estimation of relatively immediate effects of working conditions on health. The paper provides an example of such a design in a study of the effects of adverse working conditions on depressive symptoms in female teachers. Ordinary least squares and LISREL analyses suggest that adverse working conditions exert a relatively immediate effect on depressive symptoms in teachers, controlling for preemployment symptoms and other factors (age, race, social support, nonoccupational stressors, marital status, and social class of origin). Event-proneness and selection-based explanations were ruled out. Three criteria for the utility of collecting preemployment health data are outlined: differential selection, immediacy of effects, and convenience.

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All correspondence concerning this paper should be sent to the first author, EDFN, City College of New York, New York, NY 10031.

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The purpose of this paper is to examine the link between job conditions and depressive symptoms in newly appointed teachers. In examining this link, the utility of following new workers longitudinally will be described. The paper will show that measures of preemployment health can be particularly helpful in research on occupational health.

Most of the published literature on occupational stress involves samples of veteran workers. Kasl (1983) forcefully argued that research on veteran workers, including longitudinal studies that follow cohorts of veteran workers over time, is problematic. It is almost impossible to study the causal effects of workplace stressors in veteran workers, even with the help of longitudinal designs. Kasl (1983) wrote, "the casualties of inadequate adaptation may have disappeared from observation and the remainder have adapted 'successfully' (e.g., giving up on expecting work to be a meaningful human activity), but the costs of such 'successful' adaptation can no longer be reconstructed through the belated follow-up" (p. 90). Studies of veteran workers are also problematic because they are susceptible to selection-based explanations. Investigators often have little or no knowledge of the physical and mental health of veteran workers before they assumed their current jobs, making it difficult to determine if poor health in members of certain occupational groups is the result of exposure to adverse working conditions or if less healthy individuals selected themselves into the occupational groups under study.

Virtually all the published literature on teacher stress, the focal interest of the research presented here, involves samples of veteran teachers. The one paper we could locate in which an investigator examined newly appointed teachers was an unpublished final report for a grant (Kahn, 1982). Thus, in the area of sample selection, the teacher-stress literature is similar to the majority of studies in the occupational stress literature.

A Case Study

In research on psychopathology, case studies have often stimulated investigators to pursue links between possible causal variables and mental health outcomes. By itself, a case study can not establish causation; however, it can be helpful in elucidating aspects of the phenomena under study. The case study presented here highlights difficulties in establishing causal links between job conditions and (mental) health outcomes when studying veteran workers. The heretofore unpublished case material was collected in a study of coping behaviors employed by veteran teachers (Schonfeld, 1990a).

The case study (with identifying characteristics changed to preserve anonymity) of a veteran teacher apparently implicates adverse working conditions as a contributing cause of the teacher's depressive symptoms.

A woman with four years experience as a science teacher in a public school in an affluent suburb of Chicago obtained a position as biology teacher in a Bronx high school. She was an excellent biology student in college and her teaching earned satisfactory ratings for each of the four years she taught in suburban Chicago.

As a teacher with low seniority in her new school, she was assigned classes with students having very weak academic skills. It is no surprise that children who are weak academically also have many conduct difficulties (Kolvin, Miller, Fleeting, & Kolvin, 1988; Schonfeld, Shaffer, O'Connor, & Portnoy, 1988). She encountered children who by her standards were extremely unruly and given to fighting. She reported that the children frequently used vulgar language to express their contempt for her and, no less, for each other. With a great deal of energy, she tried to maintain order and a semblance of learning in her classes, but often she felt she was a failure. Her first year in the school was marked by episodes of depression that were severe enough to prompt her to seek help from a psychiatrist who would then treat her with an anti-depressant medication.

Although this case study appears to support the conclusion that her depressive symptoms resulted from difficult school-related circumstances, such a conclusion, for a number of reasons, would probably be unwarranted. First, the teacher had experienced the loss of her father the year before she started teaching in the Bronx. Second, she had recently moved to a new city. It is possible that both the loss of her father and the move could have led to a depressive episode, or that the combination of the loss, the move, and the daily interaction with unruly students brought about an episode. The teacher, however, had several depressive episodes prior to her father's death while she lived in the Chicago area and was prescribed anti-depressant medication to treat those episodes.

The issue of linking work-related stressors to psychological health is far from straightforward. The case study suggests that self-selection or selection via administrative pathways may mislead investigators into believing that situations commonly held to be aversive cause depressive symptoms. The case study illustrates the viability of what Dohrenwend and Dohrenwend (1981) described as an event proneness model. In the model, individuals with *preexisting psychopathology* are at increased risk for experiencing undesirable life events. Given the framework of the model, it is plausible that the teacher's classroom management skills were handicapped by preexisting psychopathology. Inadequate classroom management can contribute to the unruliness of students. The case study, thus, underlines the importance of controlling for preexisting health.

The case study also highlights the desirability of obtaining information on stressors occurring outside of occupational roles. The experience of a

loss and a change of domicile occurring within a year of the teacher's assuming a new job may have increased the amount of distress she experienced. Teacher stress researchers have rarely controlled for the occurrence of stressors outside of the workplace, thus, increasing the risk of drawing misleading conclusions about the influence of occupational stressors on health.

Veteran vs. Newly Appointed Teachers

A disadvantage of studying veteran workers is highlighted in a study (Schonfeld, 1990b) of New York City teachers who averaged twelve years in the profession. Adverse school conditions (e.g., pupils fighting, underprepared students attending class) were directly related to the frequency of depressive symptoms. Compared to the mean scores (median mean score = between 7 and 8) found in 13 different surveys of representative samples of American community residents, the teacher sample had a relatively high mean score on the Center for Epidemiologic Studies Depression scale (CES-D), 13.0 (Schonfeld, 1990b). Moreover, 32% of the teachers had scores at or above the "clinical cutoff" (a score of 16), a marker of increased risk for major depressive disorder (Weissman, Sholomskas, Pottenger, Prusoff, & Locke, 1977). The median for the community samples is about 17% having scores at or above the clinical cutoff (Schonfeld, 1990b). Eleven percent of the teachers in Schonfeld's (1990b) sample had scores above the mean score for psychiatric inpatients, 24 (Radloff, 1977). These findings are consistent with research showing high levels of distress in other teacher samples (Finlay-Jones, 1986; Hammen & deMayo, 1982).

Despite their relatively high scores on the CES-D, there is reason to believe that the veteran teachers found in Schonfeld's (1990b) sample were likely to be individuals who made relatively successful adaptations to their jobs because the sample did *not* include individuals who left the profession. According to Kasl's (1983) view, the teachers not found in the veteran sample because of attrition in the New York City public school system (Esrig, 1987) would be likely to include the major casualties of stress. In a nationwide survey of teachers who left the profession, Harris, Kagay, and Leichenko (1986) found that teachers who quit often cited the stressful nature of the job as a factor prompting their leaving. Thus the effects the school-related stressors exerted on depressive symptoms in Schonfeld's (1990b) veteran-teacher sample were likely to be underestimated.

A Sample of Newly Appointed Teachers

In view of the problems inherent in research with veteran workers, a group that included *newly appointed teachers* as well as recently graduated

nonteachers was followed. The study began just as the teachers graduated from college but before they entered the work force.

A problem with conducting a longitudinal study that begins just after a cohort of employees is hired parallels a problem identified by Cobb and Kasl (1977) in their study of the effects of layoffs on the health of blue collar workers. Their study began soon *after* two plant closings were announced. Both types of studies are prospective, but not "prospective enough" (Kasl & Cobb, 1982). Cobb and Kasl's (1977) study is, structurally, similar to a study that begins *after* a cohort of recent college graduates spends its first weeks teaching in a dangerous urban public school. In both examples, the environment's effects on the subjects' health may be too immediate to be detected. In each type of study the health of the subjects *prior* to the key event, either the announcement of the closings or the entry into the dangerous new workplace, cannot be reconstructed.

Kasl and Cobb (1982) advanced the view that to make research on the effects of unemployment appropriately prospective, it is important to identify industries in which the economic climate may forecast layoffs at some future time. Investigators may then obtain measures of the health of workers employed in those industries some time *before* closings are announced. Research teams would then follow the workers longitudinally. Over time, some of the workers would experience layoffs and others would not. Later comparisons between employed and laid-off workers could be made with adequate controls for prior ("preannouncement") health.

Following a parallel line of argument, the study of newly appointed teachers began *before* the teachers entered the work force. Recruitment took place in upper-division, final-semester college education and psychology courses. Comparison subjects consisted of graduating psychology students as well as education students who later did not enter teaching. Test data, supplied to this project by the Educational Testing Service (ETS), from college populations not involved in this study suggest that individuals who major in education and psychology are similar demographically and academically although the individuals in the cohorts studied here tend not to sit for the examinations offered by the ETS (the examinations are not required at the cooperating institutions). Care was taken to conduct recruitment at institutions that in the past have supplied the New York City Board of Education with large numbers of teachers.

Measures of depressive symptoms were obtained on three occasions, once in the summer (preemployment), and twice during the academic calendar after those who would become teachers obtained jobs. Although

depressive symptoms were assessed, "stress" and "burnout" were not. There is evidence indicating that stress and burnout measures have a number of defects including the confounding of stressor and distress, vulnerability to attribution errors, and lack of construct validity (Kasl, 1978; Schonfeld, 1990b, 1990c, in press).

The first of the two postemployment waves of data collection was in November when most teachers completed two months of work. The second was in March four months later when there has been greater opportunity for adaptation. Case study data on teachers suggest that the demoralizing impact of job conditions is relatively immediate, and that the November data are critical to assessing this view.

Regression analyses were employed in examining the relation between working conditions and depressive symptoms among women who obtained teaching jobs soon after graduation. In addition to preemployment depressive symptoms, a number of other factors with known links to depressive symptoms were controlled: social support (Cohen & Wills, 1985), social class of origin and race (Kessler, Price, & Wortman, 1985), undesirable fateful life events (Dohrenwend, Krasnoff, Askenasy, & Dohrenwend, 1982), marital status (Brown & Harris, 1978), and age (Myers et al., 1984). Finally, LISREL (Jöreskog & Sörbom, 1989) procedures were employed in building models of the effects.

METHODS

Sample

As part of larger longitudinal study following 1987 to 1990 male and female graduating cohorts of undergraduate divisions of the City University of New York, data were collected on 169 women who graduated in the classes of 1987 and 1988 and who obtained teaching jobs in the September following their graduation. In addition, data were collected on 89 women who attended some of the same colleges as the teachers but who did not become teachers. Recruitment took place in the spring in upper-level senior-year education classes (four colleges in 1987 and five in 1988) that were identified by faculty and administrative informants as likely to include students who would go on to obtain teaching jobs in the September following their graduation. In 1988, graduating seniors in upper-level psychology courses at three of the five colleges were also recruited.

The recruiter (the senior investigator or a research assistant) asked the students in the education and psychology classes to indicate whether or not they were graduating seniors because an intensive check of contemporaneous official (registrars') records revealed numerous mistakes in the codes identifying graduating and nongraduating students. Errors extended to commencement programs. Because registrars' records would

TABLE 1 Descriptive Statistics: Teachers and Nonteachers

Factors	Teachers	Nonteachers	p
Mean Age	27.81	28.50	ns
Mean Social Class of Origin	2.70	2.75	ns
% Married	32.0	31.8	ns
% White	69.8	60.7	ns

Note: Social Class of Origin was measured by Hollingshead's (1974) two-factor index ranging from 1 (professional/managerial) to 5 (unskilled laborers).

not be complete until the September following graduation, too late to facilitate preemployment data collection, recruitment was most efficaciously conducted in the targeted classes in the spring. The recruiter ascertained that more than 90% of the eligible students (those identifying themselves as scheduled to graduate that spring or the coming summer semester) completed letters of informed consent right in their college classrooms during the ten minutes usually allowed the recruiter by the cooperating instructors. More than 85% percent of the women who signed letters of informed consent participated in summer (preemployment) wave of data collection. Demographic data are presented in Table 1.

Research Design

The women could be followed longitudinally by questionnaire for up to three years. This paper, however, is concerned with data collected at two (summer and November) or three (summer, November, and March) points in time during their first year. Differences in preemployment symptomatology in teachers and nonteachers were studied. The effects of the fall school environment on depressive symptoms in teachers controlling for preemployment symptoms were examined. In addition, a subgroup of 128 women were studied. They were teachers during the third wave of data collection, the following March, and participated in the study at all three points in time, facilitating the construction of structural equation models of the effects of the job environment on depressive symptoms (Jöreskog & Sörbom, 1989).

Measures

The summer, fall, and spring questionnaires supplied information on depressive symptoms and nonoccupational stressors (fateful, undesirable life events such as the death of a loved one). Depressive symptoms were

measured with the Center for Epidemiologic Studies—Depression Scale (CES-D; Radloff, 1977), a depressive symptom scale, widely used in epidemiologic surveys (Schonfeld, 1990b), with satisfactory reliability and validity. In the present sample, the alpha coefficients for the summer, fall, and spring CES-Ds exceeded .89.

Social support was measured in the summer with an 8-item version of Cohen's (Cohen, Karmack, & Mermelstein, 1983; Cohen & Wills, 1985) Likert-type revision of the Interpersonal Support Evaluation List (alpha = .79). The items assessed the availability of companionship (e.g., If I decide one afternoon that I would like to go to a movie that evening, I could easily find someone to go with me) and confidants (e.g., There is at least one person I know whose advice I really trust). The demographic section of the questionnaire provided information on age, marital status, social class of origin (Hollingshead, 1974), and race.

Two measures of the adversity of the school environment were developed: (1) the Episodic Stressor Scale; and (2) the Strain Scale. The Episodic Stressor and Strain Scales reflect a distinction made in the stress literature between eventful experience and chronically occurring conditions (Pearlin & Schooler, 1978). Both scales employed neutrally worded self-report items assessing the frequency with which the teachers encountered different types of stressors. In contrast to traditional stress and burnout items that assess the extent to which the teachers are annoyed, bothered, or otherwise disturbed by the stressors, neutrally worded items are less likely to be confounded with symptoms (Kasl, 1987; Schonfeld, 1990b, 1990c).

The Episodic Stressor Scale was calculated by computing the teacher's mean score on items assessing the frequency with which she encountered episodically occurring stressors (e.g., threat of personal injury, confrontation initiated by an insolent student, episode of vandalism). Each item was scored: (0) not at all; (1) once per month; (2) one per week; (3) 2-4 times per week; and (4) daily. The Strain Scale, a name that is consistent with Pearlin and Schooler's (1978) terminology, was calculated by computing the teacher's mean score on the items assessing ongoing stressors (e.g., overcrowded classroom, unmotivated students attending class, tendency of administrators not to enforce rules against disruptive pupils). Each item was scored: (0) not at all; (1) to a minimal extent; (2) to a small extent; (3) to a moderate extent; and (4) to a great extent. Both the Episodic Stressor and Strain Scales included positively worded items (e.g., "a parent praised you") in order to break any tendencies toward response set. These items were reverse scored. Alpha coefficients for the environmental stressor scales are presented in Table 2.

TABLE 2 Means, Standard Deviations, and Alpha Coefficients for the Fall and Spring Stressor Scales

<i>Stressor scales</i>	<i>M</i>	<i>SD</i>	<i>Alpha</i>
Fall (Time 1)			
Episodic Stressors	1.06	.56	.83
Strains	1.25	.58	.84
Spring (Time 2)			
Episodic Stressors	1.16	.58	.84
Strains	1.38	.60	.85

RESULTS

The findings are presented in three sections. Findings bearing on the preemployment differences in depressive symptoms in teachers and nonteachers are presented first. The second and third sections present the results of within-teachers analyses. The second set of results consists of ordinary least squares (OLS) regression analyses in which fall symptom levels are predicted by the school environment with preexisting symptoms and other factors controlled. Finally, structural equation analyses capitalizing on three waves of data collection are presented.

Differences Between the Teachers and Nonteachers

As shown in Table 3, the preemployment mean CES-D of women who became teachers was significantly lower than that of the women who did not become teachers. Teachers, however, did not differ significantly from nonteachers in either the fall or the spring. These findings suggest that selection may not account for variance in symptoms among teachers. The nonteachers, however, may not have constituted a suitable comparison group. Excluding full-time graduate students, about 28% of the nonteachers were not employed and 65% of the nonteachers who went to work obtained lower status jobs, according to the Hollingshead (1974) rating scheme, than the teachers.

Adverse School Conditions and Fall Depressive Symptoms

The correlations among the variables employed in the regression analyses are shown in Table 4.

In order to examine the influence of school conditions on depressive symptoms, the fall CES-D was regressed on a number of control vari-

TABLE 3 Mean Differences Between Teachers and Nonteachers on the CES-D

Time of year	n	M	SD	t	p
Summer (Time 0)					
Teachers	169	10.67	8.98	2.63	.01
Nonteachers	84	13.93	8.98		
Fall (Time 1)					
Teachers	163	12.37	9.00	1.59	ns
Nonteachers	84	14.36	9.81		
Spring (Time 2)					
Teachers	141	12.75	9.39	.63	ns
Nonteachers	73	13.64	10.38		

ables, including the preemployment CES-D, and the Episodic Stressor Scale (see Table 5). The other control variables included social class of origin, life events, marital status, race, social support, and age. Only the preemployment depression measure and the Episodic Stressor Scale were significantly related to outcome. It was expected that the other control variables would not be strongly related to postemployment CES-D when the preemployment CES-D was controlled. Cohen and Cohen (1983) wrote that "whatever relationship to [the postvariable] these other candidates may have is likely to already be represented (and to an even greater degree) in their relationship to [the prevariable]" (p. 423). The R^2 for the equation containing the control variables but not the Episodic Stressor Scale was .34; the R^2 increase when the Episodic Stressor Scale was entered into the regression equation last was .11.

To put the OLS analyses in context, consider that a unit difference on the Episodic Stressor Scale was associated with a 6-point *adjusted* (for preemployment symptoms, etc.) increase in postemployment symptoms based on the unstandardized regression weight found in Table 5 (as in the difference between classrooms in which episodic stressors occur at a rate of about once per month [an Episodic Stressor Scale score of 1] and classrooms in which stressors occur at a rate of about once per week [scale score of 2]). Given the normative landmarks of the symptom measure (Schonfeld, 1990b) as well as the R^2 increase, such an effect is important from a public health standpoint.

TABLE 4 Correlations Among the Variables

Variable	1	2	3	4	5	6	7	8	9
1. Age	—								
2. Social support ^a	-.00	—							
3. Life events	.10	-.25*	—						
4. Soc. class of origin ^b	.14	.06	-.02	—					
5. Married ^c	.60**	-.01	.06	.11	—				
6. White v. Nonwhite ^d	.09	-.17*	-.13	-.29**	.04	—			
7. Summer CES-D	-.21*	-.32**	.14	.07	-.13	-.16*	—		
8. Episodic stressors	.17*	-.11	.00	-.05	.17*	.06	.03	—	
9. Strains	.14	-.10	.04	-.01	.17*	.04	.11	.63**	—
10. Fall CES-D	-.14	-.24**	.09	.02	-.03	-.05	.57**	.36**	.39**

Note: $163 < n < 169$; * $p < .05$; ** $p < .01$.
^aHigher scores reflect increased support.
^bHigher scores on the Hollingshead (1974) scale reflect lower social status.
^c0 = Not currently married; 1 = Married.
^d0 = Nonwhite; 1 = White.

TABLE 5 Regression of the Fall CES-D on the Episodic Stressor Scale and Control Variables

Variable	B	Beta	p
Age	-.14	-.12	ns
Social support ^a	-.67	-.03	ns
Life events	.36	.02	ns
Social class of origin ^b	.19	.02	ns
Not married ^c	.89	.05	ns
White v. Nonwhite ^d	.75	.04	ns
Summer CES-D	.53	.53	.0001
Episodic stressors	6.00	.35	.0001

Note: With pairwise deletion of missing values $n = 158$.

^aHigher scores reflect increased support.

^bHigher scores on the Hollingshead (1974) reflect lower social status.

^c0 = Not currently married; 1 = Married.

^d0 = Nonwhite; 1 = White.

A parallel set of regression analyses in which the Strain Scale replaced the Episodic Stressor Scale showed no substantive change in the findings. The Strain Scale was significantly ($B = 5.40$; $Beta = .34$, $p < .0001$) related to postemployment symptoms controlling for all other factors. The R^2 increase associated with the Strain Scale was .11.

The issue of the timing of the measures bears on this analysis. The test of the relation of fall school conditions to fall symptoms constitutes a concurrent analysis. The analysis leaves open the possibility that depressive symptoms "cause" the occurrence of so-called environmental stressors (e.g., depressed teachers promote classroom environments conducive to student rule-breaking, a version of the "event proneness" model described by Dohrenwend and Dohrenwend [1981]). In order to assess the viability of this alternative hypothesis, the zero-order correlations between summer symptoms and the two fall measures of the school environment were examined. The correlations between summer symptoms and the fall Episodic Stressor and Strain Scales were nonsignificant (see Table 4). The two occasions at which the factors were measured were sufficiently close in time to cast doubt on an event-proneness explanation of the findings.

LISREL Models

Provisional models of causal pathways were developed from the same covariance matrix with the help of the LISREL (Jöreskog & Sörbom, 1989) computer program. According to Hayduk (1987), sample size ($n = 128$) was sufficiently large for model building. To simplify model building all control variables except the summer CES-D were excluded. This was justified in view of the results of OLS analyses that failed to show significant effects for the other control variables with summer CES-D in the regression equation. Different variables, however, were brought into the LISREL models. In order to construct a latent fall (Time 1) school environment variable (Env1 in Figures 1 and 2), both the fall Episodic Stressor (Evt1 for School Events Time 1) and Strain (Str1) Scales served as indicators. Similarly, the spring Episodic Stressor (Evt2) and Strain (Str2) scales served as indicators of the Time 2 school environment (Env2). Env1 and Env2 were forced to be in same units as the Episodic Stressor Scale. Summer (preemployment), fall, and spring CES-Ds were used to construct the Time 0, 1, and 2 Symptoms measures, respectively. Each Time 0, 1, and 2 Symptoms factor was forced to have the same scale as its indicator.

Collecting three waves of data was helpful in estimating reciprocal effects. The availability of preemployment (Time 0) symptom data, in this analysis an "instrumental" variable, made possible the estimation of reciprocal effects (Kenny, 1979). For this purpose, LISREL resembles two-stage least squares analyses. LISREL, however, has the added capability of building measurement error into the model. Based on the reliability (alpha) coefficient of each CES-D indicator, an error term for each indicator was entered into the model. Since both the Time 1 and 2 Environment factors each have two indicators, LISREL was allowed to estimate the error term for each school-stressor indicator. The epsilon terms for the two Strain indicators were the only two error terms that were allowed to be correlated. This was justified on the grounds that the Strain Scales measure ongoing types of stressors.

One of the models that fit ($X^2(8) = 7.83$, $p = .45$) the data is depicted in Figure 1. The model shows reciprocal effects at Time 1 and again at Time 2. The effect from Environment to Symptoms was at each time substantially greater than the ("halo") effect from Symptoms to Environment (n.s.). The model was improved slightly when the paths representing the halo were dropped; however, the paths representing the halo were retained here in support of the view that the effect of adversities in the school environment on teachers' depressive symptoms is causal controlling for individual differences in the teachers' tendencies to report on their work environments.

FIGURE 1

LISREL Model of Reciprocal Effects in Female Teachers

* $p < .001$

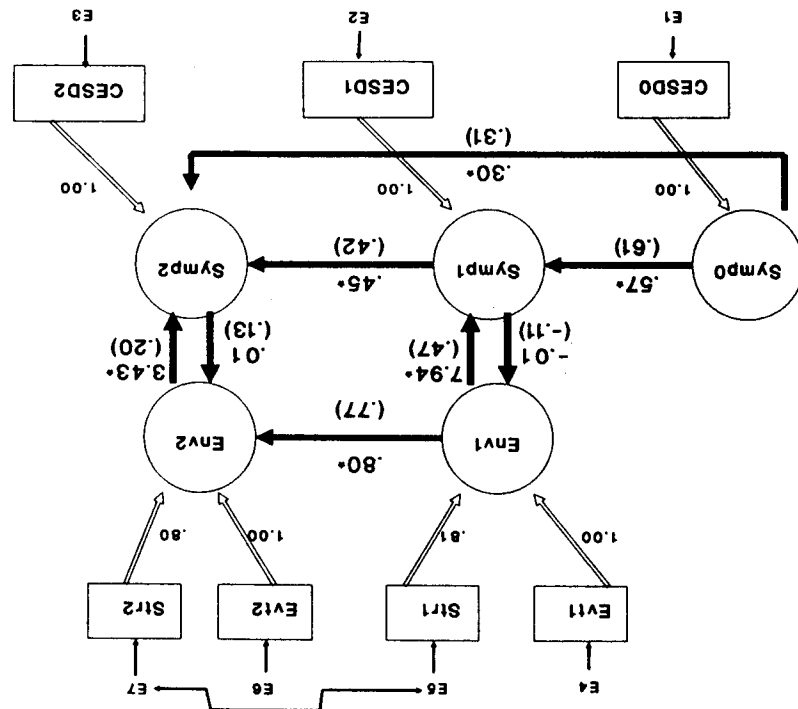
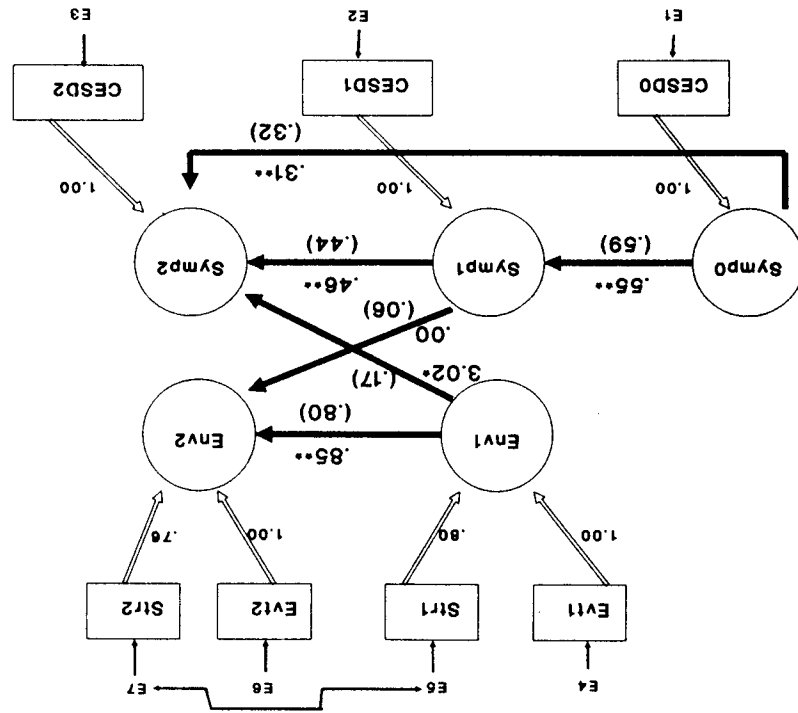


FIGURE 2

LISREL Model of Lagged Effects in Female Teachers

* $p < .05$, ** $p < .001$



The chi-square values of rival models, including a model with cross-lagged effects (see Figure 2; $X^2(10) = 44.80, p < .0001$), led to their rejection. In the cross-lagged model, however, it should be noted that the effect from Time 1 Environment to Time 2 Symptoms was significant but the effect from Time 1 Symptoms to Time 2 Environment was not. The OLS and LISREL analyses suggest that the effects of the work environment on depressive symptoms in teachers are *immediate* and begin with the commencement of the first school year.

DISCUSSION

Three sets of findings were outlined. First, t-tests suggested that it was unlikely that highly symptomatic women initially selected themselves into teaching. Of interest is the relatively high level of distress in the nonteacher sample, which may be reflective of (1) a high rate of unemployment after college and (2) the relatively low status jobs held by those who were employed; see Warr & Parry, 1982). Second, consistent with the view that the impact of the job environment on distress is relatively immediate, the OLS analyses indicate that there was a substantial effect of adverse job conditions on depressive symptoms as early as November, the third month of the teachers' careers. The correlations of preemployment symptoms with neutrally-worded self-reports on the work environment was nonsignificant, findings that are incompatible with an event-proneness explanation. Third, a LISREL analysis that included reciprocal effects suggests that the causal path from the school environment to symptoms is substantially stronger than the causal path from symptoms to environment.

Central to each of these analyses was the collection of preemployment data on symptoms. Without such data the selection hypothesis could not have been tested. Moreover, without preemployment symptom data the regression analyses would have been narrowly cross-sectional, offering little opportunity to test the event-proneness model. Preemployment symptoms constituted an instrumental variable that made possible either LISREL or two-stage least squares procedures (the results are similar although the latter procedures are not presented) to assess reciprocal effects.

The study has several limitations. First, only women were examined. Because teaching is a traditional "female" occupation, many more women than men were available to be recruited. In the future, after data from four cohorts have been collected, it is expected that sufficient numbers of male teachers will have been incepted into the study to supply the power required for statistical analyses. Second, the sample was restricted to new teachers in their first year on the job. The pattern of findings, however, is

consistent with results obtained with veteran teacher samples (e.g., Finlay-Jones, 1986; Hammen & deMayo, 1982; Schonfeld, 1990b). Future analyses of some cohorts will extend to later years on the job. Third, the women were older than "traditional" college graduates. Demographic trends, however, reveal that the average age of individuals attending college has been increasing in recent years, a trend that is expected to continue into the 1990s (Bruno, 1990; Gerald, Horn, & Hussar, 1989). The average age of the women, although not like that of college graduates in the 1960s, was in line with recent trends.

It might be argued that the proper analysis would capitalize on lagged data, but only the data obtained in the fall (Time 1) and spring (Time 2). In each of those two waves *both* symptoms and the work environment were assessed. In the summer, only (and necessarily) symptom data were collected. Interestingly, this kind of lagged analysis would be prospective, but probably "not prospective enough" because the critical periods of data collection were the summer and November waves. This is reflected in the rejection of the LISREL model that included lagged effects from Time 1 Symptoms to Time 2 Environment and from Time 1 Environment to Time 2 Symptoms (Figure 2) and the satisfactory fit of the simultaneous effects model (Figure 1). It might be further argued that with teachers, data collection at multiple points in time between the September start date and November might be helpful in better ascertaining the zigs and zags of distress as a function of the school environment. Such a procedure, however, ought to be employed with caution because of the risk of reduced symptom scores owing to a "re-test artefact" (Jorm, Duncan-Jones, & Scott, 1989).

When to Use Preemployment Measures of Health?

It is not clear that preemployment measures of health are required in all studies of the effects of occupational stressors. Three criteria for making decisions on when to obtain preemployment measures of health are suggested. The first criterion involves selection. If there is evidence that individuals differentially select themselves into occupations, plants, schools, worksites, etc., there is reason to obtain preemployment measures. A second criterion is immediacy of effects. If there is reason to suspect that the impact of job conditions on health is relatively immediate, which may well be the case for teachers, preemployment data would be helpful in disentangling effects. Without preemployment data the analyses presented here would have been sharply limited to cross-sectional data or less-than-optimally time-lagged data. If the effects of particular occupational conditions on health are relatively slow-acting, preemployment measures would probably not be helpful as long as

longitudinal data collection begins at a point in time that is relatively early in the process in which the suspected stressors exert their effects.

A third criterion for collecting preemployment data is convenience. If (1) an investigator is studying an occupation in which the effects of environmental stressors are slow-acting and (2) self-selection for health is unlikely but (3) data on preemployment health can be collected conveniently, there is incentive to obtain such data. By obtaining preemployment health data the investigator can establish a baseline from which to map the trajectory of change in the individual's health. Preemployment medical data can sometimes be conveniently obtained from records at relatively little cost to the investigator because new employees may be required to take a physical examination that includes a few basic health measures like blood pressure and heart rate. In contrast to medical data, preemployment data on depressive symptoms and anxiety are less readily available.

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Workplace and Personal Stresses Antecedent to Employees' Alcohol Use

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There exists little documentation about the influence of work-related variables on employees' drinking behaviors. This study evaluates the impact of work role stresses (e.g., load, ambiguity & boredom), employment insecurity and recent life stressors on the level of drinking and the use of alcohol to relax. A large sample of health care workers was surveyed. Findings suggest a relationship between alcohol use and strains associated with recent stressful life changes and employment insecurity, though little evidence exists to suggest that work role stressors affect alcohol use.

It is estimated that five percent of the U.S. workforce is alcoholic (Schramm, Mandell, & Archer, 1978). If we included nonalcoholic problem-drinkers the estimate would probably be much higher. The costs associated with work-related alcohol abuse are difficult to evaluate, yet most of the popular literature acknowledges that the costs associated with alcohol-related accidents, work performance decrements, and social maladjustment are probably severe (Archer, 1977). Borrowing from Pattison and Kaufman (1982), we distinguish between alcohol use, alcohol misuse, and dependence. By definition, alcohol use may be motivated by the psychic rewards it provides its users. People drink to momentarily feed good, enhance socialization, improve their self-image, manipulate moods, and manage the transient trauma of personal loss (Caddy, 1978). Alcohol misuse is defined as excessive usage, though the individual may be neither chemically nor psychologically dependent. Misuse is construed primarily in terms of the negative consequences of alcohol use, where excessive usage leads to retarded psychomotor functioning, aggression, impairment of social roles, and/or marital maladjustment (Caddy, 1978; Hull & Bond, 1986).

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