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**Psychological Climate and Job Satisfaction:  
An Examination of Reciprocal Causation**

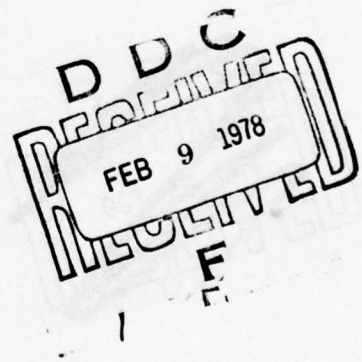
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analyses based on overidentifying conditions demonstrated that other causal models were also consistent with the data.

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## Psychological Climate and Job Satisfaction:

## An Examination of Reciprocal Causation

Theoretical and empirical relationships between individual perceptions of work and organizational environments (referred to here as psychological climate [James & Jones, 1974]) and job satisfaction have received considerable attention (cf. Downey, Hellriegel, Phelps, & Slocum, 1974; Gavin & Howe, 1975; Guion, 1973, 1974; Herman, Dunham, & Hulin, 1975; Hellriegel & Slocum, 1974; James & Jones, 1974, 1976; Johannesson, 1973; LaFollette & Sims, 1975; Newman, 1974; Payne & Pugh, 1976; Payne, Fineman, & Wall, 1976; Schneider, 1975; Schneider & Synder, 1975; Waters, Roach, & Batlis, 1974). From a theoretical standpoint, a distinction between descriptive (perceptual, cognitive) and evaluative (affective, emotional) orientations has often been employed to differentiate between psychological climate (PC) and job satisfaction (JS) (cf. James & Jones, 1974; Payne et al., 1976; Schneider, 1975), although it also has been assumed that a relationship should exist among the constructs. The empirical evidence is somewhat mixed, however. While Schneider and Snyder (1975) found low PC - JS relationships, other studies have generally indicated at least moderate relationships (cf. Downey et al., 1974; Gavin & Howe, 1975; LaFollette & Sims, 1975), but not to the extent that PC and JS could be considered tautological, as postulated by Johannesson (1973) and Guion (1973).

A causal assumption, often implicit in the studies cited, was that PC and JS are asymmetrically related; that is, perceptions of the environment were conceptualized as causal factors in the formulation of attitudes of job satisfaction. A similar assumption has been evident in the job satisfaction literature. For example, Locke (1976) presented the following causal model for job satisfaction: Object (situation) → perception (cognition) → appraisal

(value judgement) → emotion, where PC would be represented in the perception (cognition) stage and JS by the appraisal and emotion stages. In this cognitive information processing model, perceptions/cognitions of the situation serve as the initial stage of cognitive information processing and provide the major source of situational information employed by the individual in the formulation of attitudes such as JS, as well as expectancies and instrumentalities (cf. James, Hartman, Stebbins, & Jones, 1977). In effect, JS represents an additional stage of cognitive processing which reflects beliefs about "my" appraisals and value judgments, and emotional or affective reactions to them.

What the above model fails to take into account is the likelihood of a reciprocal influence of JS on PC. For example, it is reasonable to presume that a prior history of reinforcements or punishments in the same or even related situations, and the appraisals and emotions associated with such reinforcements and punishments, might influence not only what situational events will be perceived in a particular situation (e.g., selective perception) but also how they will be interpreted (e.g., perceptual/cognitive distortions such as redefinition, reconstruction, use of defense mechanisms to protect self-esteem or to preserve cognitive consistency, and so forth). Research in perception, cognition, memory, and learning also suggests that although perception/cognition and affect are qualitatively different constructs, they are nevertheless continuously interacting processes. Thus, measurement of variables (e.g., PC) from one domain intrinsically reflects the causal influences of at least some variables (e.g., JS) from the other domain (cf. Ittelson, Proshansky, Rivlin, & Winkel, 1974; Jones & Gerard, 1967; Mahoney, 1977; Stotland & Canon, 1972; Wyer, 1974; James, Hater, Gont, & Bruni, Note 1). For example, Jones and

Gerard (1967, p. 254) noted that "There is fairly impressive evidence in the literature on perception, learning, and memory that cognitive processes are geared to the construction of a subjective reality that is compatible with beliefs, values, and attitudes. This cognitive construction of events involves varying amounts of distortion and nonveridical representation".

Based on the above rationale, it was assumed that although PC served as the first stage in a cognitive information processing model, it was a product of various influences, which included a reciprocal causation relationship with JS (cf. James & Jones, 1974, 1976; James et al., Note 1). The objective of the present research was to provide a partial test of the hypothesis of reciprocal causation between PC and JS. The quasi-causal method employed to test this hypothesis was cross-lagged panel correlation. The hypothesis was tested separately for each of six dimensions of PC against one overall JS composite.

This study also provided an opportunity to examine more fully the role of cross-lagged panel correlation designs in causal analysis. In particular, it addressed the issue of alternative causal explanations for the same set of data. This is a crucial concern when methods such as cross-lagged panel correlations are employed, and one which has received insufficient treatment in the psychological literature.

#### Method

##### Sample

The sample consisted of male, U.S. Navy enlisted personnel ( $n = 1,110$ ) on ships operating in the Atlantic and Pacific Oceans during the latter half of 1973. This sample was part of a larger sample ( $n = 4,315$ ) described by Jones and James (Note 2), and consisted of the enlisted personnel who participated in the first data administration and were available for data collection purposes

in a second administration. In the second administration, data were collected from a sample of eight ships, drawn from the 20 ships studied initially. However, the present sample of individuals and ships was representative of the initial sample; the reader is referred to Jones and James (Note 2) for a description of individual and ship characteristics. With respect to the present study, the first set of psychological climate and job satisfaction data was collected during the first few weeks of each ship's deployment from U.S. ports; another set of psychological climate and job satisfaction data was collected near or at the end of each ship's deployment (five to seven months after the first administration).

#### Instruments

Psychological climate. Psychological climate was defined as "the individual's cognitive representations of relatively proximal situational conditions, expressed in terms that reflect psychologically meaningful interpretations of the situation" (cf. James et al., Note 1). The measurement of PC focused on psychologically meaningful "cognitive representations" of the situation (e.g., ambiguity, challenge, cooperation, friendliness, support, warmth, trust, etc.) rather than on specific (micro) descriptions of situational events (cf. James et al., 1977; Payne & Pugh, 1976; Schneider, 1975; James et al., Note 1). The rationale for this approach was based on interactional psychology and cognitive social learning theory, which stress the importance of the psychological meaning given to perceptions (cognitions), particularly when relationships between situational perceptions and individual attitudes and behavior are examined (cf. Ekehammar, 1974; Endler & Magnusson, 1976; Mischel, 1973, 1977).

As reported in Jones and James (Note 2), a principal component analysis and varimax rotation of 35 item composites ( $n = 4,315$ ), composed of 145 items

describing perceptions of the work and organizational environment, provided six components of psychological climate. A brief description of the six components is as follows:

1. Job Challenge, Importance, and Variety. This component was based on item composites from the job characteristic literature (cf. Hackman & Lawler, 1971), and reflected a job perceived as challenging, important to the Navy, and involving a variety of duties. Also included were opportunities for dealing with other people, autonomy in making job-related decisions, feedback, and high standards of quality and performance.

2. Conflict and Ambiguity. This component reflected perceived conflict in organizational goals and objectives, an ambiguity of organizational structure and roles, a lack of interdepartmental cooperation, and poor communication from management. Poor planning, inefficient job design, a lack of awareness of employee needs and problems on the part of management, and somewhat unfair and capricious reward processes were also indicated.

3. Professional and Organizational Esprit. This component reflected perceptions that the job and the Navy had a positive image to outsiders and offered desirable growth potential. In addition, perceptions of an open atmosphere to express one's feelings and thoughts, confidence in the supervisor, and consistently applied organizational policies, combined with nonconflicting role expectations, were indicated.

4. Leadership Facilitation and Support. This component reflected the extent to which supervisors assisted in accomplishing task goals by means of scheduling activities, planning, and so forth as well as the degree to which supervisors facilitated interpersonal relationships and provided personal support.

5. Workgroup Cooperation, Friendliness, and Warmth. This component reflected the quality of member relationships and identification with the workgroup.

6. Job Standards. This component reflected the degree to which jobs had rigid standards of quality and accuracy, and included job pressure resulting from inadequate time, manpower, training, or resources to complete assigned tasks.

Each individual's PC scores were based on component scores (direct solution) in the first administration. For pragmatic reasons, the length of the climate questionnaire had to be reduced in the second administration. To reduce the length and still maintain the major thrust of the climate dimensions, the items in those composites which loaded highly on each climate component in the first administration were included in the second administration. A total of 82 climate items was retained. For the purposes of the present study, the climate data from both the first and second administrations were scored according to the paradigm developed for the second administration. Thus, the PC scores for each dimension, an indicator of a component, and administration were based on a summation of item scores (standard deviations were similar) for those items that were included in the salient composites that defined each component in the initial analysis. Each item was included in only one climate dimension.

This scoring procedure produced PC scores for each dimension that correlated highly with the original, direct solution scoring procedure, although orthogonality was not maintained. Based on first administration data, the correlations between the two sets of scores (i.e., a component and its corresponding dimension) ranged from .71 to .87, with the exception of Job Standards,

for which the correlation was only .29. The squared correlations among the PC dimensions scores (excluding Job Standards for reasons discussed later) ranged from .11 to .40 for the time 1 data, and from .15 to .46 for the time 2 data. These results suggested that the PC dimension scores were measuring at least partially different perceptual domains.

Job Satisfaction. A 17 item JS questionnaire was included in both data administrations. Many of the items were based on research by Porter (1961, 1962, 1963a, b, c; Porter & Lawler, 1968) and Hackman and Lawler (1971), and were selected on the basis of relevance for Navy enlisted personnel. The selected items included satisfaction with security, opportunity for personal growth and development, pay, opportunity to develop close friendships, and others. Additional items were developed specifically for a Navy enlisted sample, and involved satisfaction with rules and regulations regarding military appearance and training. Each of the 17 items was rated on a five-point scale (very dissatisfied, dissatisfied, indifferent, satisfied, very satisfied).

A principal component analysis of the 17 items indicated that one general component accounted for all but a small proportion of the variance. Thus, the 17 items were summed (item standard deviations were similar) to provide an overall JS score for both waves of data.

#### Analytic Procedure

The cross-lagged panel correlation (XPLC) design employed to examine reciprocal causation relationships between overall JS and each of the six PC dimensions is presented in Figure 1. In this figure,  $PC_1$  and  $PC_2$  refer to any PC dimension measured at time 1 and time 2. The same notation holds for  $JS_1$  and  $JS_2$ . The straight lines represent (a) autocorrelations ( $a_1, a_2$ ), which provide measures of stability as well as an indicator of self-causation (cf.

Johnston, 1972); and (b) cross-lagged relationships ( $c_{12}$ ,  $c_{21}$ ), which provide a partial basis for causal inference. The curved lines ( $s_{12}$ ,  $s_{21}$ ) are synchronous correlations, which are used to assess the equivalence of the underlying causal or structural equations (cf. Kenny, 1975; Pindyck & Rubinfeld, 1976).

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Insert Figure 1 about here  
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The assumptions employed in the XLPC design to examine the presumed reciprocal causation between PC and JS were as follows.

1. Synchronicity. Synchronicity refers to the question of whether the PC and JS variables were measured at the same points in time (Kenny, 1975). While it was true that the variables were measured on the same occasions (PC and JS were measured in the same questionnaire on each occasion), this was no guarantee that the information used by each individual to answer various PC and JS items reflected the same time intervals. That is, PC and JS items often require retrospection and thus past events were presumably employed to answer some items. However, the major concern underlying synchronicity is whether the variables have equivalent measurement intervals, and it was presumed here, without empirical verification, that the retrospective intervals for PC and JS were roughly equivalent.

2. Stationarity. Stationarity refers to whether the structural equations for PC and JS were the same at the two times of measurement. A necessary, but not sufficient, condition for stationarity is equivalent synchronous correlations ( $s_{12}$ ,  $s_{21}$ ) in a population, and, as a basis for inference, approximately equivalent correlations in a sample (cf. Pindyck & Rubinfeld, 1976). While the sufficient condition (i.e., an actual comparison of the population structural equations) could not be examined, a partial test was based on the approximate

equivalence (within the realm of sampling error) of the sample synchronous correlations (cf. Kenny, 1975 for less stringent criteria).

3. Stability. It was presumed that PC and JS would be subject to some change over time and that these changes would be accompanied by changes in rank ordering of subjects (McNemar, 1969). Nevertheless, it was assumed that PC and JS would be at least moderately stable, as indicated by at least moderate autocorrelations ( $a_1$ ,  $a_2$ ). No change in PC and/or JS (i.e.,  $a_1$  and/or  $a_2 = 1.00$ ) suggests no cross-lagged, causal effects, while if  $a_1$  or  $a_2$  were very low, that would suggest a relatively random event, again precluding causal inference.

It is important to note that  $a_1$  and  $a_2$  are not interpreted as test-retest reliability coefficients. That is, the XLPC design predicts that some change will take place in the true scores over time, thus violating a major criterion for test-retest reliability (cf. Blalock, 1970; Heise, 1969).

4. The measurement interval corresponds to the causal interval. This is one of the most crucial and yet one of the most neglected assumptions of XLPC analysis (cf. Duncan, 1969; Bohrnstedt, 1969; Heise, 1970). It also represents the point at which the present research departs from the assumptions of traditional XLPC designs. That is, the XLPC design typically presumes that a causal interval is identifiable (i.e., a change in  $X$  should result in a change in  $Y$  during some specified interval [cf. Pelz & Lew, 1970]). However, it was assumed here that PC and JS were continuously interacting, internal processes. If PC and JS interact continuously, then it makes little sense to try to establish a causal interval. Rather, a more reasonable model would be one in which the mutual interaction between PC and JS takes place on a relatively rapid basis in essentially unidentifiable intervals. This rationale might not be

appropriate in an individual's initial acclimation period in an organization or following a major change (i.e., when the cognitive representations underlying PC and the affective reactions underlying JS are only beginning to be established or have to be changed drastically).<sup>1</sup> However, these were generally not the case in this study, and thus a condition of continuous interaction was considered appropriate.

Assuming continuous interaction between PC and JS, how might the XLPC design be employed? The answer is that a continuous reciprocal interaction between PC and JS should be inferable from an examination of relationships across time (cf. Rozelle & Campbell, 1969). That is, if PC is a cause for JS, and JS is a cause for PC, then  $c_1$  and  $c_2$  should both be meaningfully different from zero. A more stringent criterion is to require that not only are  $c_1$  and  $c_2$  meaningfully different from zero, but also that they are relatively similar to each other. In the extreme condition,  $c_1$  and  $c_2$  would be equal (in the population), although this is not a necessity.<sup>2</sup> Rather, as discussed below, a condition of reciprocal causation is suggested if both  $c_1$  and  $c_2$  are larger than a "no-cause baseline".

5. No-cause baseline. The major hypothesis for XLPC is spuriousness (Kenny, 1975), although failure to reject the null hypothesis does not constitute proof of spuriousness and rejection of the null hypothesis is not irrefutable evidence that unmeasured causes of PC and JS are unrelated (cf. James & Singh, Note 3). Given a condition of spuriousness, similar, or equal in the population, cross-lagged correlations are expected (Cook & Campbell, 1976; Kenny, 1973, 1975). That is, given synchronicity and stationarity,  $c_1$  and  $c_2$  should be approximately equal in the sample and less than  $s_1$  and  $s_2$

(assumed approximately equal) if PC and JS were caused by a common underlying factor.

As a test for spuriousness in unidirectional causal models, Kenny (1975) suggested that either  $c_1$  or  $c_2$  should be larger than  $s_1$  ( $s_2$ ) if causality is to be considered a viable possibility.<sup>3</sup> However, Cook and Campbell (1976) and Rozelle and Campbell (1969) noted that this criterion is, in general, too stringent because of attenuation of causal relationships over time, or temporal erosion (Kenny, 1973), and because  $s_1$  and  $s_2$  tend "to be inflated by shared time-specific measurement errors and the absence of temporal erosion" (Cook & Campbell, 1976, p. 293). Instead, Rozelle and Campbell (1969) proposed a "no-cause baseline" with which to test causal hypotheses, including those of reciprocal causation (cf. p. 79). That is, the no-cause baseline provides an estimate (expectation) of what  $c_1$  and  $c_2$  would be in the absence of PC  $\rightarrow$  JS, JS  $\rightarrow$  PC causation. Therefore, a possible test for reciprocal causation is to show that  $c_1$  and  $c_2$  are both larger than the no-cause baseline.

Although the no-cause baseline has been the subject of criticism by both Rickard (1972) and Kenny (1975), and received what might be considered equivocal support by Cook and Campbell (1976), it was nevertheless considered to be a meaningful intermediate criterion on which to examine reciprocal causation in the present study. In fact, given the inherent bias against finding cross-lagged correlations larger than synchronous correlations, the no-cause baseline appeared to be the more meaningful comparison base, even though a sampling distribution does not exist for significance tests and it is often unlikely that the same no-cause baseline should be employed to test both  $c_1$  and  $c_2$  (Rickard, 1972). It is here that the equivocality of the XLPC correlation design for causal inference is most apparent. However, as discussed shortly,

the XLPC design should not be employed except for suggestive purposes, and in the present study it appeared that even with its inherent faults the no-cause baseline was the more appropriate statistic.

The computation of the no-cause baseline requires separate reliability estimates for each variable ( $PC_1$ ,  $PC_2$ ,  $JS_1$ ,  $JS_2$ ) and may require homogeneous stability (i.e., approximate equivalence of the unattenuated autocorrelations in the sample, which suggests that all the nonerror causes of PC and JS change at the same rate over time).<sup>4</sup> Given reliability estimates and homogeneous stability, the no-cause baseline is computed as follows (a) correct  $a_1$  and  $a_2$  for attenuation, (b) average the two corrected autocorrelations (this provides an estimate of overall stability), and (c) multiply this estimate by the average of the two synchronous correlations (Kenny, 1975, p. 893; note that a geometric mean was not used in the second step). If a hypothesis of reciprocal causation is viable, then  $c_1$  and  $c_2$  should be larger than the no-cause baseline (as noted above, significance tests do not exist).

In summary, it is important to note that the XLPC design is intermediary in terms of causal explanation between purely correlational designs and well-elaborated structural equation models (cf. James et al., in press; Kenny, 1975; James & Singh, Note 3). That is, the XLPC design is based on "passive data" (Cook & Campbell, 1976), does not attempt to delineate a causal system, and does not attempt to identify or to estimate underlying causal or structural parameters. On the other hand, Kenny (1975) suggested that XLPC designs were perhaps more applicable to social science data than structural equation models, given the present state of (incomplete) theoretical systems and the pragmatics of measurement (e.g., theoretically, structural equation models require perfectly reliable measures while XLPC does not). Nevertheless, as with struc-

tural equation analyses that employ passive data, the results of a XLPC analysis should be employed only to examine the logical consistency of the data with causal hypotheses and models. The goal of these examinations is to identify those hypotheses that are consistent with the data and to reject hypotheses that are inconsistent. Because there will, in effect, always be multiple, and perhaps conflicting, hypotheses that are consistent with the data, the XLPC design should never be employed to conclude that a particular causal model is unique or unassailably correct.

Given stationarity, stability, and homogeneous stability or some variant thereof, the primary concern here was whether a causal inference of reciprocal causation was consistent with the data in each of the XLPC analyses. Thus, if both the cross-lagged correlations in each PC-JS XLPC analysis were significant and larger than the no-cause baseline, then the suggestion was made that the null hypothesis of spuriousness could be rejected and that a causal hypothesis of reciprocal causation was one viable alternative. On the other hand, one could not conclude that PC and JS were in fact reciprocally related because (a) other causal models might "fit" the same data equally well, (b) the effects of unmeasured variables were not fully ascertained, and (c) the possibility of contamination due to random and nonrandom measurement errors (cf. Duncan, 1969; Bohrnstedt, 1969; Goldberger, 1971; Heise, 1970; Pelz & Lew, 1970; Weaton, Muthin, Alevin, & Summer, 1977). Thus, when interpreted cautiously, the XLPC design provides a basis for suggesting plausible causal inferences, but not without consideration of the fact that there are almost always plausible alternative explanations. Examples of tests for alternative causal explanations are discussed later in this report.

Results

Reliability estimates (coefficient alpha), synchronous correlations, and autocorrelations for the six XLPC analyses are presented in Table 1. Of interest initially were the time 1 and time 2 reliability estimates. As shown in Table 1, with the exception of the Job Standards PC score, all reliabilities were at acceptable levels (i.e.,  $>.70$ ). For Job Standards, however, not only were the coefficients alphas unacceptable, but the autocorrelation ( $a_1$ ) was quite low. As a result, Job Standards was deleted from further consideration in this study.

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Insert Table 1 about here  
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Stationarity. The synchronous correlations ( $s_1$  and  $s_2$ ) were significant and generally quite similar for each of the five remaining XLPC analyses. That is, the differences in correlation ranged from .00 for the two synchronous correlations between Conflict and Ambiguity and overall JS to .05 for the two Workgroup Cooperation, Friendliness, and Warmth - overall JS correlations. None of the differences among the synchronous correlations was significant at the .05 level, and it was concluded that the stationarity assumption had been reasonably met in each of the five remaining XLPC analyses.<sup>5</sup>

Stability. The autocorrelations for the five remaining PC dimensions ( $a_1$ ) varied from .54 to .64. The autocorrelation for overall JS ( $a_2$ ) was .54. These results suggested that the stability assumption had been reasonably satisfied.

Cross-lagged correlations. The cross-lagged correlations are presented in the first two columns of Table 2. Examination of the cross-lagged correlations demonstrated that (a) the two cross-lagged correlations in each of the five

XLPC analyses were of moderate magnitude and significantly different from zero, and (b) the two cross-lagged correlations within each XLPC analysis were, within the realm of sampling error, equivalent. Significance tests, using the Pearson-Filon  $z$ -test, demonstrated that none of the differences between the cross-lagged correlations reached significance ( $p < .05$ ). These results were consistent with a state of PC-JS reciprocal causation as well as with the possibility that PC and overall JS were cosymptoms of an underlying common factor. These alternative causal inferences are addressed below.

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Insert Table 2 about here  
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No-cause baseline comparisons. Estimates of the unattenuated autocorrelations are presented in the third and fourth columns of Table 2. With the exception of the comparison between  $a_{11}$  and  $a_{21}$  for Leadership Facilitation and Support and overall JS, the differences between  $a_{11}$  and  $a_{21}$  in each XLPC analysis were significant ( $p < .05$ , Pearson-Filon  $z$ -test). Such differences suggested that the cross-lagged correlations would be significantly different, where the more stable variable (PC) would appear to be an effect and the less stable variable (overall JS) a cause (cf. Heise, 1970; Rozelle & Campbell, 1969). However, as noted above, the cross-lagged correlations were not significantly different, nor were the synchronous correlations (which might also change over time as a function of differences in stability). In addition, examination of the internal consistency reliability coefficients for each variable in each wave of measurement demonstrated that neither overall JS nor any of the PC components were either increasing or decreasing in reliability (the effect of increases or decreases in reliability is the same as increases or decreases in stability [cf. Kenny, 1975]). Thus, the significant differences in the unatten-

uated autocorrelations did not appear to have a contaminating effect on the cross-lagged correlations. Moreover, as noted earlier (see Footnote 4), given essentially equal  $c_1$  and  $c_2$ , stationarity, and essentially equal reliability coefficients, approximately equal unattenuated autocorrelations in a sample does not appear to be a necessity (it can also be shown rather simply using path models that homogeneous stability is an almost impossible criterion to satisfy unless the PC and overall JS variables are parallel measures of the same construct). Therefore, it was considered appropriate to compute no-cause baselines in this study.

The no-cause baselines for each XLPC analysis are presented in the fifth column of Table 2. Comparisons between  $c_1$  and  $c_2$  and the no-cause baselines within each XLPC analysis demonstrated that in 9 out of 10 comparisons the cross-lagged correlations were larger than the no-cause baselines. These differences were not large, however, ranging to a high of .07. Nevertheless, these results suggest that a causal inference of reciprocal causation between PC and overall JS should not be rejected and thus should be considered as a viable causal hypothesis.

#### Discussion

In its short history, climate research has focused almost exclusively on content. Given that situational perceptions have constituted the initial unit of analysis, climate research has focused on questions such as the number of common factors underlying the perceptions or the level to which various types of composites of the perceptions can be aggregated to represent situational attributes. Questions regarding the perceptual and cognitive processes underlying situation perceptions have been addressed only infrequently (cf. James & Jones, 1974; James et al., 1977; Payne & Pugh, 1976; Payne et al., 1976;

Schneider, 1975) and often incompletely (James et al., Note 1). For example, responses to the assertions that climate and job satisfaction were tautological (Guion, 1973; Johannesson, 1973) involved an apparently oversimplified distinction between perception and affect.

In broad perspective, the perceptual and cognitive processes underlying climate perceptions can be addressed from very basic cognitive information processing models such as those proposed by Broadbent (1971, 1977), Erdelyi (1974), Neisser (1967), Shiffin & W. Schneider (1977), and Weyer (1974), or the more abstract and socially oriented social learning and cognitive social learning models of Bandura (1977), Mischel (1973, 1976, 1977), and Stotland and Canon (1972). In addition, perceptual and cognitive processing theory pertaining to environmental perceptions in general has been provided by both environmental psychologists (cf. Ittelson et al., 1974) and interactional psychologists (cf. Bowers, 1973; Ekehammar, 1974; Endler & Magnusson, 1976). As reviewed by James et al. (Note 1), the general orientation of many of these models and theories is that although perception/cognition and affect may be qualitatively different from the standpoint of content, the underlying causal processes may be highly interrelated and reciprocal. This suggests that much of the debate in the literature over climate and job satisfaction is spurious. Indeed, instead of the emphasis on content differences between the two constructs, the theoretical literature suggests that the time and effort would be better served by concentration on whether and how the processes underlying the constructs are dynamically and reciprocally related.

The present study has suggested that an inference of reciprocal causation between psychological climate and overall job satisfaction is a meaningful possibility. This provides direct support for the existence of reciprocal

causation between perception/cognition and affect that has been assumed in many of the perception, cognitive, environmental, and interactional theories referenced above. Two further questions are now salient, namely (1) How should the results be interpreted substantively?; and (2) What types of alternative causal models are also consistent with the data? These questions are addressed in the following discussion.

#### Interpretation of Results Based on Reciprocal Causation

With only two waves of data in a XLPC design, it is difficult to differentiate among various hypotheses regarding the direction of causal effects (Rozelle & Campbell, 1969; Yee & Gage, 1968). For example, a positive  $c_1$  suggests that increases in PC cause increases in overall JS. However, a positive  $c_2$  also suggests that increases in overall JS cause decreases in PC. Moreover, the finding that  $c_1$  and  $c_2$  were approximately equivalent in each of the XLPC analyses leads to direct inconsistencies in the interpretation of the causal direction for both PC and overall JS. For example, a positive  $c_1$  suggests that increases in PC cause increases in overall JS, while a positive  $c_2$  suggests that increases in PC cause decreases in overall JS.

In an attempt to select meaningful hypotheses regarding causal direction, prior research, statistical inferences, and subjective judgement are required (Kenny, 1975). With respect to statistical inferences (and not conclusions), Cook and Campbell advised that if  $c_1$  and  $c_2$  were both larger than the no-cause baseline, then a positive, direct relationship was indicated. This suggests that increases (decreases) in Leadership Facilitation and Support, Job Challenge, Importance, and Variety, Organizational Esprit, and, to a lesser extent, Workgroup Cooperation, Friendliness, and Warmth, led to increases (decreases) in overall job satisfaction. The same logic applies also to Conflict and

Ambiguity, namely that increases (decreases) in this PC dimension led to decreases (increases) in overall JS. Partial support for these inferences is also provided by the fact that the synchronous correlations had the same signs as the cross-lagged correlations (Kenny, 1975).

Support for the PC  $\rightarrow$  JS causal inferences presented above can be found in the literature. For example, increases in JS have presumably been a result of increases in leadership facilitation and support (cf. Bass, Valenzi, Farrow, & Solomon, 1975; Stogdill, 1974), workgroup cooperation, friendliness, and warmth (cf. Porter, Lawler, & Hackman, 1975), job challenge, importance, and variety (cf. Hackman & Oldham, 1975, 1976), and organizational esprit (cf. Rotondi, 1975). These are, of course, generalizations. Many of the studies on leadership, group behavior, job and role characteristics, and esprit (which is closely tied to identification with the organization) have been descriptive and lacking in a base for causal inference. Nevertheless, experimental and longitudinal studies do tend to support the causal inferences above (cf. Beehr, Walsh, & Taber, 1976; Billings, Klimoski, & Breaugh, 1977; Daum, 1975; Greene, 1975; Levine, 1973; Likert, 1967; Lowin, Hrapchak, & Kavanagh, 1969; Murnighan & Leung, 1976; O'Connell & Cummings, 1976; Robey, 1974; Umstot, Bell, & Mitchell, 1976; Weed, Mitchell, & Moffitt, 1976), and the general trend of thinking in organizational psychology certainly favors these hypotheses.

It must be noted that numerous moderators have been suggested for many of the PC  $\rightarrow$  JS relationships addressed above (e.g., higher order need strength, self-esteem, cognitive consistency, etc.). While moderators remain potentially salient, the magnitudes of the PC-JS relationships in this study, both static and longitudinal, indicated that the effects of moderators would not be overwhelming. In addition, a number of studies have differentiated between extrin-

sis and intrinsic sources of satisfaction. However, as noted earlier the JS items in the present study were most meaningfully treated as a single composite. The resulting unavailability of different types of satisfaction was perhaps a limitation, although the single overall JS composite was likely more a function of the sample than of the questionnaire. Essentially the same questionnaire has been employed in another study on health managers, and the underlying component structure differentiated among one intrinsic and two extrinsic dimensions (James, Hartman, Jones, & Stebbins, 1975).

Evaluation of the hypotheses for the overall JS  $\rightarrow$  PC relationships has been difficult, primarily because of the paucity of literature in this area. Based on Cook and Campbell and Kenny, there was a strong inclination to presume that increases (decreases) in overall JS caused increases (decreases) in PC for all but the Conflict and Ambiguity dimension, where an inverse causal flow was again presumed. For example, in early (accommodative) stages of cognitive development, perceptions that the job was challenging, the leadership supportive, the workgroup friendly, and the organization "involving" would presumably result in predicted degrees of overall satisfaction. This in turn could reinforce the individual for the present cognitive representations and also stimulate (cause) the seeking out of more reinforcing perceptions. Following the development of a relatively stable (but not static) set of cognitive representations and affective reactions, an "equilibrium-type" condition (cf. Namboodiri, Carter, & Blalock, 1975; Miller, 1971; James & Singh, Note 3) would be expected, at least until a significant change took place. The equilibrium-type condition predicts that the major causal influences have worked through the system (i.e., the individual) and thus that the reciprocal causation between overall JS and PC had arrived at a homeostatic relationship. In other words, overall

JS and PC mutually reinforce each other by reciprocal causation.

While obviously speculative and in need of more extensive research, the explanations advanced are consistent with the data. On the other hand, the causal effects of JS on PC might take other forms. For example, decreases in overall JS, which could be a function of any number of events, could stimulate the individual to seek out more pleasing environmental events in the interest of developing more pleasing environmental perceptions. This would depend on how the source(s) of dissatisfaction were attributed (e.g., external vs. internal), and might even involve compensatory mechanisms (e.g., seeking of more challenging aspects of the job to compensate for dissatisfaction with the supervisor). A dissonance model might also be considered; the continued completion of a hard, monotonous, and in general, dissatisfying task might stimulate the need to perceive the task as important, but perhaps not challenging.

In sum, it is simply not possible here to resolve fully the inconsistency between the plausible, but competing JS  $\leftrightarrow$  PC hypotheses. Perhaps both operate simultaneously? This may reflect lack of ingenuity on the part of the present authors but it also emphasizes the almost total absence of empirical treatment (not models, which have been plentiful) of reciprocal causation in psychology. The present study has taken a first step by indicating the plausibility of reciprocal causation, but a more complete explanation of the processes and antecedents underlying the reciprocal causation will require intensive empirical studies and more encompassing causal systems.

#### Alternative Causal Models

As stressed throughout this report, the XLPC design can be used only to test alternative causal models and to identify those that are consistent with the data and to eliminate those that are inconsistent. As noted by Kenny

(1975), the null hypothesis that is tested most frequently in XLPC designs is spuriousness. However, there are, in effect, an infinite number of models that involve spuriousness in XLPC designs (cf. Duncan, 1969, 1972, 1975). For example, a simple model of spuriousness (see Figure 2A) is seldom, if ever, appropriate in psychological research. This reflects the facts that seldom, if ever, will all variables be measured with perfect reliability, will the variables included in the research exhaust the causal system (e.g., PC is not the only cause of overall JS), or will various forms of nonrandom measurement error, such as method variance, not contaminate measurement. Stated simply, since it is impossible to avoid all sources of error and misspecification in a XLPC design, there will always exist alternative causal models that are consistent with the data.

Because it is impossible to test all possible alternative causal models, it is appropriate to devote particular attention to a subset of models that take account of obvious problems in a particular set of data. This is illustrated by obvious limiting factors in the present study, including (a) the possibility of method variance because both PC and JS were measured in the same questionnaire, (b) the lack of perfect reliability in the measures in each wave of measurement, and (c) the fact that PC was not the only cause for overall JS, nor was overall JS the only cause for PC. These limiting factors could operate, either separately or in combination, to inflate spuriously the synchronous and cross-lagged correlations as well as the autocorrelations. The spurious inflation of correlations can be viewed as resulting from correlations among the error or "disturbance terms" for each of the variables. For example, if a stable, unmeasured cause underlies  $PC_1$  and  $PC_2$ , then this cause will correlate with itself over time and result in a correlation between the  $PC_1$  and  $PC_2$

disturbance terms (i.e., an unmeasured cause is included as part of the error or disturbance term [cf. Heise, 1970; James & Singh, Note 3]).

Tests for nonrandom measurement errors such as method variance, as well as other misspecifications in causal models such as omitted causal variables, were presented by Costner (1969), and were adopted to test for alternative causal models in this study. The alternative causal models used as a base the model for spuriousness presented by Kenny (1975). As shown in Figure 2A, "the chief alternative explanation of any causal effect" is spuriousness. Thus,  $Z_1$ , an unmeasured variable, causes  $PC_1$  and  $JS_1$  simultaneously at time 1, and  $Z_2$ , the same unmeasured variable, causes  $PC_2$  and  $JS_2$  simultaneously at time 2.  $Z_1$  is the cause for  $Z_2$ , and any relationship between PC and JS is due to spuriousness and not causality.

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 Insert Figure 2 about here  
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The terms in Figure 2A were defined as follows.  $p_1$  and  $p_2$  are path coefficients leading from the causal factor  $Z$  to PC and JS at each point in time. The path coefficients leading from  $Z_1$  to  $PC_1$  and  $PC_2$  (and from  $Z_2$  to  $JS_1$  and  $JS_2$ ) were set equal because the assumption of stationarity was satisfied by the empirical data.  $p_3$  is the path coefficient leading from  $Z_1$  to  $Z_2$ .  $d_1$  through  $d_4$  are disturbance terms, and include random and nonrandom measurement errors as well as the effect of unmeasured variables (the paths from  $d_1$  and  $d_3$  to  $PC_1$  and  $PC_2$ , respectively, are equal to  $\sqrt{1-p_1^2}$ , and the paths from  $d_2$  and  $d_4$  to  $JS_1$  and  $JS_2$ , respectively, are equal to  $\sqrt{1-p_2^2}$ ). Finally, associated with each model are a set of overidentifying conditions. The rationale for overidentifying conditions is beyond the scope of this report (cf. Costner, 1969; Duncan, 1972; Namboodiri et al., 1975); it can be noted here only that

the model must satisfy all of the overidentifying conditions, within the realm of sampling error, to be considered viable.

Figure 2A reflects a condition of spuriousness but no random measurement error, nonrandom measurement error, or misspecification. Overidentifying conditions (1), equal synchronous correlations (nonsignificantly different in a sample), and (2), equal cross-lagged correlations, were satisfied. However, overidentifying condition (3), a consistency criterion based on the same logic as a Spearman tetrad, was not satisfied in any of the five PC analyses. That is, the product of the autocorrelations ( $a_1, a_2$ ) was significantly different ( $p < .05$ ) than the product of the cross-lagged correlations ( $c_1, c_2$ ) (based on the Spearman and Holziner test reported in Duncan [1972]). Thus, the model in Figure 2A was rejected because it was inconsistent with the data. Specifically, the reason for rejection was that the product of the correlations between two "different" variables with a time-lag would have to be equal to the product of their respective autocorrelations. Because it is probable that a variable will correlate more highly with itself over time than with other variables over time, it was considered unlikely that Figure 2A would be of much use as an alternative causal hypothesis, at least in psychology.

Figure 2B presents a model with contemporaneously correlated disturbance terms, which was a possibility in the present study because of (a) the likelihood of method variance, and (b) the fact that reciprocally related variables tend to have correlated disturbances (Johnston, 1972). Again, overidentifying conditions (1) and (2) were satisfied, but overidentifying condition (3), which was the same as the consistency test for Figure 2A, could not be satisfied in any of the analyses.<sup>7</sup> Thus, Figure 2B was rejected as a plausible alternative causal hypothesis in this study. It is important to note that rejection of

Figure 2B casts doubt on the assumption that method variance seriously contaminated the results of this study.

Figure 2C incorporates autocorrelations of the disturbance terms for both PC and JS. This will almost always be the case in XLPC designs because, as discussed earlier, stable, unmeasured variables, represented in the d terms, tend to correlate with themselves over time. Moreover, in this study a slight possibility existed for the measurement errors of PC (and JS) to be correlated over time because the variables were not perfectly reliable. This rationale received support because both overidentifying conditions were satisfied in all XLPC analyses (although the reason(s) for this satisfaction of assumptions could not be ascertained fully). Thus, although a consistency test was not available for Figure 2C, the model represented by this figure could not be rejected. In other words, Figure 2C was consistent with the data and therefore was every bit as much an alternative causal hypothesis as reciprocal causation! Figure 2D, based on developed logic, must also be accepted as an alternative causal model consistent with the data.

In summary, models incorporating spuriousness and autocorrelations of the disturbance terms (Figure 2C) as well as such autocorrelations plus contemporaneous correlations of the disturbance terms (Figure 2D) must be considered viable causal hypotheses based on the data in this study. This does not mean necessarily that reciprocal causation is still not a viable causal hypothesis; the fact is simply that there are other, plausible causal explanations as well. Based on the models in Figure 2 as a group, the alternative causal model with the greatest likelihood was Figure 2C, which suggested specification error due to unmeasured variables which affected causally either PC or JS, or both. Because a direct model for spuriousness (Figure 2A) was rejected, as well as a

model involving only contemporaneously correlated disturbances (Figure 2B), where method variance would presumably have its major effect, the unmeasured variable hypothesis appears to be the most probable.<sup>8</sup> Figure 2D is also less probable because the model in Figure 2B was rejected. Thus, as noted earlier, what is needed is additional theory and research to build a theoretical system which identifies antecedents for the PC-JS relationship.

#### Conclusion

Based on theoretical literature pertaining to perceptual and cognitive processes, the present study demonstrated the viability of a reciprocal causation hypothesis between situational perceptions and one measure of overall affect. These results question not only previous studies that employed unidirectional models to study climate and job satisfaction relationships, but might also be extrapolated to research on perception, cognition, and affect in general. That is, many of the perception and cognitive models referenced earlier in this section suggest that a reciprocal causation model underlies many of the relationships among perceptions, cognitions, and affect (cf. James et al., Note 1). For example, the cognitive schemas underlying the interpretative functions of perception are learned (i.e., based upon a developmental history), and are often associated intrinsically with need satisfaction (cf. Stotland & Canon, 1972). Because schemas, including schema-based motives arising from needs, tend to be unified and dynamically related, a reciprocal causation model appears to be a most viable theoretical model. However, empirical verification of this general hypothesis, especially with constructs relevant to industrial/organizational psychology, await additional research. Thus, it is strongly recommended that future research in climate as well as in other content areas which often rely upon perceptions (e.g., job and role

attributes, leadership, communication, group attributes and processes, etc.) pay more attention to perceptual processes and the possibility of reciprocal causation.

With respect to the XLPC design employed in this study, it was emphasized throughout this report that alternative causal models must be considered when XLPC designs are employed. This report has only touched the surface of the many models that might be considered. In addition, concerns such as the necessity-sufficiency of causation (Feldman, 1975) were not considered. Nevertheless, it is hoped that the rationale developed has adequately demonstrated why a two-wave, two-variable XLPC analysis provides a basis for rejecting untenable hypotheses but not for identifying a unique causal model. Finally, although Kenny (1975) suggested that the XLPC design, in comparison to the more elaborate structural equation models, was perhaps more applicable for psychological data, the present authors strongly recommend that attempts be made to employ structural equation models in the future. Although these models typically do not result in a unique causal conclusion, they do provide a stronger basis for causal inference.

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## Footnotes

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<sup>1</sup>In situations subject to major change, the XLPC design is not appropriate either (cf. Kenny, 1975).

<sup>2</sup>Typically, the XLPC design focuses only on unidirectional or recursive causal models, and the question is whether  $X_1 \rightarrow Y_2$  is preponderant over  $Y_1 \rightarrow X_2$ , even though both cross-lagged correlations are significant. Thus, significant as well as approximately equal cross-lagged correlations provide one condition for suggesting a reciprocal relationship.

<sup>3</sup>The dynamic correlation has also been used to test for spuriousness in XLPC designs. However, this statistic may provide very misleading information and was not employed here (James et al., in press).

<sup>4</sup>Rozelle and Campbell (1969) implied, and Kenny (1975) stated directly that homogeneous stability is required to compute a no-cause baseline. Applying path analysis theory to XLPC designs (cf. Duncan, 1969, 1972, 1975) involving multiple indicators for true scores (cf. Blalock, 1970; Costner, 1969;

Heise, 1969; Werts & Linn, 1970), it can be shown that this is not necessarily true. As discussed later in this report, the real question is whether changes in reliability and stability affect the magnitudes of the XLPC correlations. Furthermore, equal cross-lagged correlations, stationarity, and approximately equal reliability coefficients for each variable at each point in time suggest that the lack of homogeneous stability has not affected, at least substantially, the cross-lagged panel correlations.

<sup>5</sup>The significance test employed was based on the Pearson-Filon  $\underline{z}$ -test for nonindependent correlations (cf. Kenny, 1975, footnote 7).

<sup>6</sup>Although path analysis will be employed to set up consistency tests for overidentifying conditions, no attempt will be made to solve for the path coefficients (standardized structural parameters). This is because the XLPC design almost never meets the statistical assumptions necessary to solve for path coefficients (e.g.,  $\text{plim}(\frac{1}{n} \frac{PC}{d_4}) = 0$  [cf Kenny, 1975; James & Singh, Note 3]). Consistency tests may, however, still be conducted.

<sup>7</sup>It was assumed that  $\underline{m} = \underline{n}$  in Figures 2B and 2D because the stationarity assumption was satisfied and the reliabilities of PC (and JS) were approximately equivalent in each wave of measurement.

<sup>8</sup>Autocorrelated disturbances can also be a result of correlated measurement errors (Werts & Linn, 1970). A minimum five month time-lag, however, questions the contribution of this error. Furthermore, because PC and JS are obviously caused by a number of other variables, the unmeasured variable problem must take precedence.

Table 1  
 Reliability Estimates, Synchronous Correlations, and Autocorrelations  
 for Six Psychological Climate Dimensions and  
 Overall Job Satisfaction

Psychological Climate and <u>Overall Job Satisfaction</u>	<u>Reliability</u>		<u>Correlations</u>				<u>Autocorrelations</u>	
	Time	Time	<u>Synchronous</u>		$a_1^c$	$a_2^d$		
	1	2	$s_1^a$	$s_2^b$				
1. Conflict & Ambiguity	.72	.74	-.51	-.51	.54	--		
2. Lead. Facilitation & Support	.92	.93	.59	.63	.58	--		
3. Wkpg. Coop., Friend., & Wm.	.84	.84	.48	.53	.58	--		
4. Job Chall., Imp., & Variety	.83	.85	.64	.67	.64	--		
5. Organizational Esprit	.86	.87	.63	.64	.64	--		
6. Job Standards	.15	.10	-.32	-.36	.34	--		
7. Overall Job Satisfaction	.89	.89	--	--	--	--	.54	

Note:  $n = 1,110$ ,

$$a_{s_1} = r(PC_1, JS_1)$$

$$b_{s_2} = r(PC_2, JS_2)$$

$$c_{a_1} = r(PC_1, PC_2)$$

$$d_{a_2} = r(JS_1, JS_2)$$

Table 2  
 Cross-lagged Correlations, Unattenuated Autocorrelations and  
 No-Cause Baselines for Five Psychological Climate  
 Scores and Overall Job Satisfaction

Psychological Climate and Overall Job Satisfaction	Cross-Lagged Correlations		Unattenuated Autocorrelations		No-Cause Baseline
	$c_1^a$	$c_2^b$	$a_1$	$a_2$	
1. Conflict and Ambiguity	-.39	-.40	.74	--	-.34
2. Lead. Facil. & Supp.	.44	.41	.63	--	.38
3. Wksp. Coop., Fr., & Wm.	.38	.33	.69	--	.33
4. Job Chall., Imp., & Var.	.49	.46	.76	--	.43
5. Org. Esprit	.50	.48	.74	--	.43
6. Overall Job Satisfaction	--	--	--	.61	--

Note:  $n = 1,110$ , all correlations are significant ( $p < .001$ )

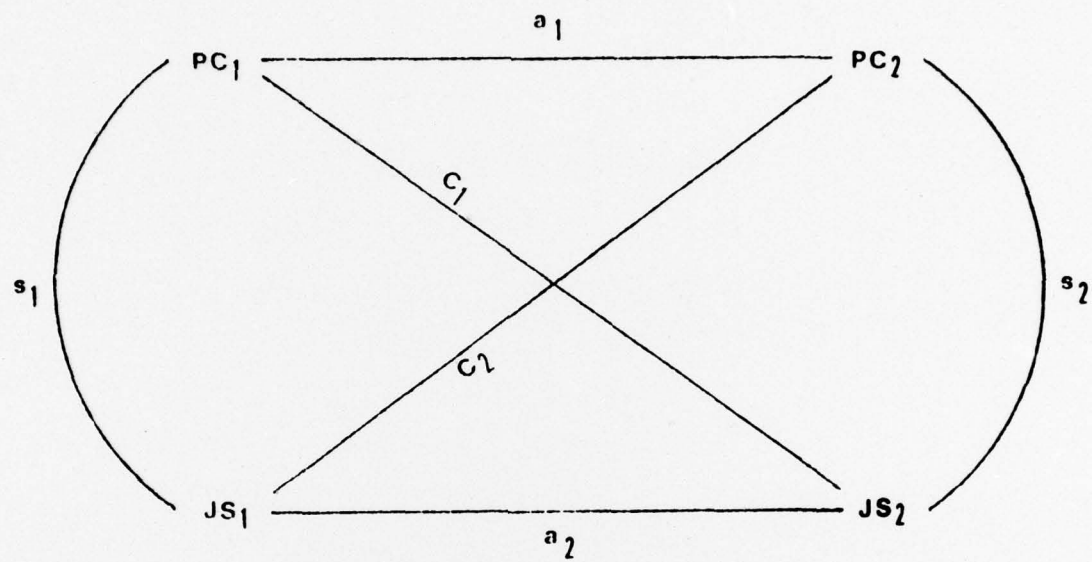
$a \quad \overline{c_1} = r(PC_1, JS_1)$

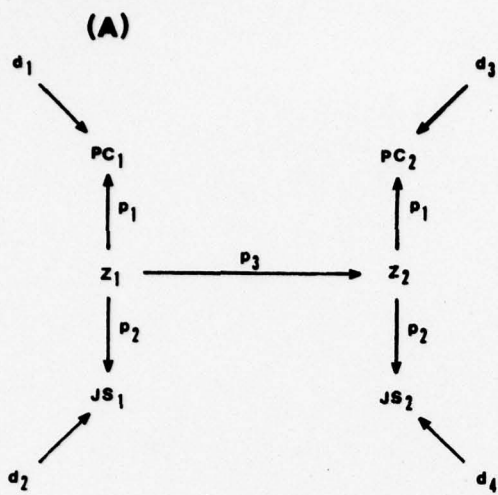
$b \quad \overline{c_2} = r(JS_1, PC_2)$

Figure Captions

Figure 1. Cross lagged panel correlation design for psychological climate and job satisfaction.

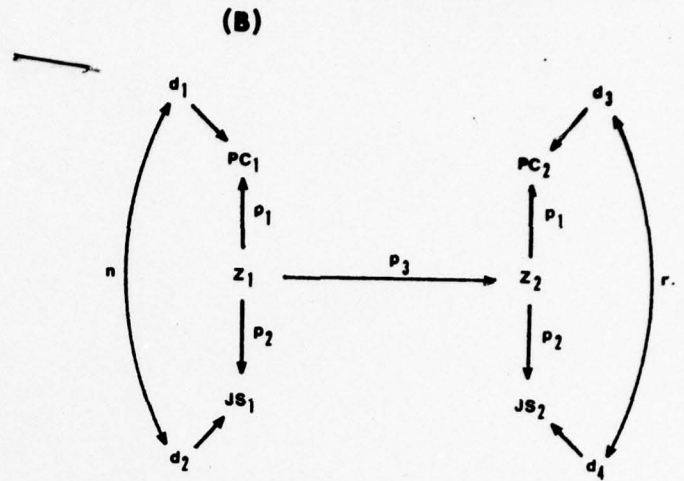
Figure 2. Selected overidentifying conditions for a cross-lagged panel correlation design relating psychological climate to job satisfaction.





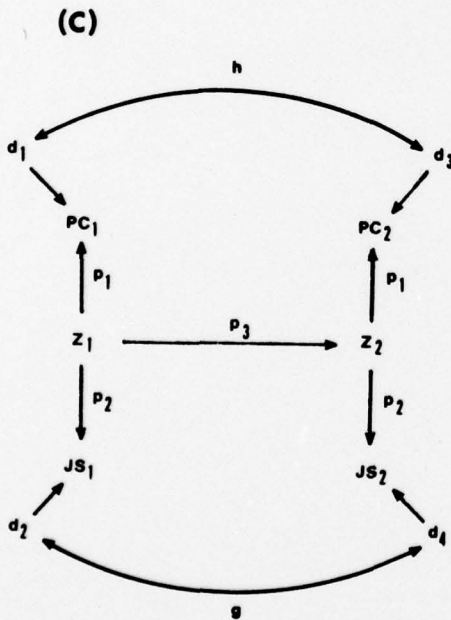
Overidentifying Conditions

- (1)  $\underline{s}_1 = \underline{s}_2 = \underline{p}_1 \underline{p}_2$
- (2)  $\underline{c}_1 = \underline{c}_2 = \underline{p}_1 \underline{p}_2 \underline{p}_3$
- (3)  $\underline{s}_1 \underline{s}_2 = \underline{c}_1 \underline{c}_2 = \underline{p}_1^2 \underline{p}_2^2 \underline{p}_3^2$



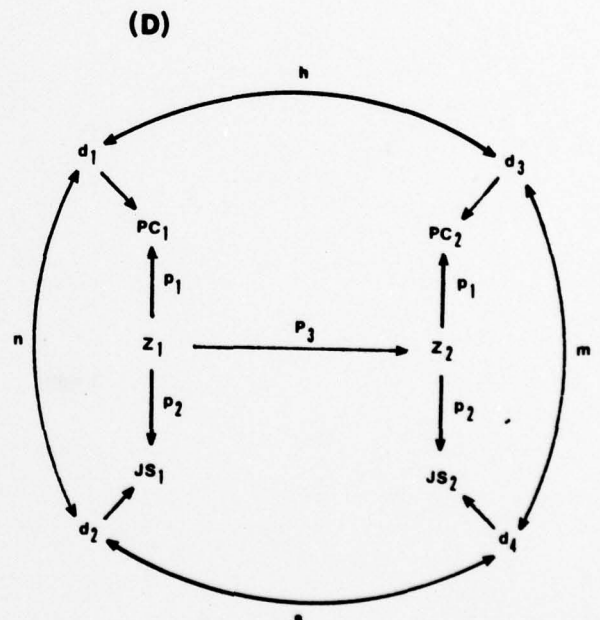
Overidentifying Conditions

- (1)  $\underline{s}_1 = \underline{s}_2 = \underline{p}_1 \underline{p}_2 + n \sqrt{(1-p_1^2)(1-p_2^2)}$  ; if  $\underline{m} = \underline{n}$
- (2)  $\underline{c}_1 = \underline{c}_2 = \underline{p}_1 \underline{p}_2 \underline{p}_3$
- (3)  $\underline{s}_1 \underline{s}_2 = \underline{c}_1 \underline{c}_2 = \underline{p}_1^2 \underline{p}_2^2 \underline{p}_3^2$



Overidentifying Conditions

- (1)  $\underline{s}_1 = \underline{s}_2 = \underline{p}_1 \underline{p}_2$
- (2)  $\underline{c}_1 = \underline{c}_2 = \underline{p}_1 \underline{p}_2 \underline{p}_3$



Overidentifying Conditions

- (1)  $\underline{s}_1 = \underline{s}_2 = \underline{p}_1 \underline{p}_2 + n \sqrt{(1-p_1^2)(1-p_2^2)}$  ; if  $\underline{m} = \underline{n}$
- (2)  $\underline{c}_1 = \underline{c}_2 = \underline{p}_1 \underline{p}_2 \underline{p}_3 + \underline{g} \underline{n} \sqrt{(1-p_1^2)(1-p_2^2)} + \underline{h} \underline{n} \sqrt{(1-p_1^2)(1-p_2^2)}$  ; if  $\underline{m} = \underline{n}$

Figure 2

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