

ORGANIZATIONAL BEHAVIOR: A Review and Reformulation of the Field's Outcome Variables

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INTRODUCTION

Organizational behavior is an interdisciplinary field that examines the behavior of individuals within organizational settings as well as the structure and behavior of organizations themselves. Macro organizational behavior (sometimes called organization theory) has roots in sociology, political science, and economics, and deals with questions of organizational structure, design, and action within social/economic contexts. Micro organizational behavior is rooted in psychology and deals with individual attitudes and behavior and how they are influenced by and influence organizational systems.

With both micro and macro branches, the field of "OB" often functions as two separate subdisciplines. Macro researchers are frequently sociologists who identify with the Organizations and Occupations section of the American Sociological Association, while micro researchers most commonly align themselves with the Industrial and Organizational Psychology division of the American Psychological Association. There are, however, some integrating mechanisms which draw these camps together. The Academy of Management serves both branches of the field and brings micro and macro researchers together in a single forum. And, more importantly, both sides of the field are commonly housed within a single department or subarea within American business schools. To date, this integration has resulted in some common language as well as a recognition of the joint contribution of the two perspectives, but most research is still distinctly psychological or sociological in its approach to variables and levels of analysis.

Organizational Behavior as an Applied Field

At present, the two sides of organizational behavior are moving at cross directions regarding the issue of basic versus applied research. At the macro level, the legacy has been one of descriptive empirical research (e.g. relating organizational size to differentiation) with very little concern for application. The macro orientation is now shifting with a surge of interest in questions such as organizational design, strategy, and policy formulation. At the micro level, the history has been one of extremely applied research, exploring determinants of very few outcome variables and compiling findings in an almost atheoretical way. The development of models at the micro level has been slow but the trend is now clearly toward more theoretical work.

Although there are conflicts between the directions of micro and macro research, one might characterize the field's overall orientation by the notion of fundamental research on applied organizational issues. The main concern in the

field appears to be upon important outcome variables, issues of concern to organizations and their participants. But, at the same time, there is increasing appreciation and some movement toward the development of fundamental theory, hypotheses that are neither simple collections of correlates nor direct applications of models from the parent disciplines.

Organizational Behavior as an Outcome-Oriented Field

The most popular way of summarizing the field has usually been some mixture of organizational practices (e.g. job design and pay systems), organizational processes (e.g. leadership and control), broad theoretical perspectives (e.g. reinforcement and expectancy theory), or outcomes (e.g. job satisfaction and productivity). Both Mitchell (1979) and Cummings (1982) touched on all three dimensions in their prior reviews for the *Annual Review of Psychology*. Mitchell concentrated on personality and individual differences, job attitudes, motivation, and leadership, while Cummings covered task design, feedback, structure, technology and control. The present review, like those of Mitchell's and Cummings', will concentrate on the psychological or micro side of the field. However, this review will be organized strictly by outcome variable, concentrating on issues directly related to organizational and individual welfare.

I have followed an outcome orientation for this review because it will highlight many of the shortcomings as well as opportunities for the field. To date, much of the research in industrial/organizational psychology has been devoted to questions of interest to personnel specialists, while micro OB has attempted to address issues related to managing human resources in organizations. The formulation of research has perhaps been broader in micro OB than I/O psychology, since the clients of OB have included general managers who are charged with running the entire organization rather than only those staff specifically engaged in personnel functions. Yet, both micro OB and I/O psychology can be criticized for taking an overly narrow focus. One criticism is that research questions are often biased to serve managerial rather than individual or societal interests (Braverman 1974). A second concern is that the field may not have even served managerial interests well, since research has taken a short-term problem focus rather than having formulated new forms of organization that do not currently exist (cf Argyris 1976). Finally, it could be argued that a descriptive science of organizations has been slow to develop because outcomes have been emphasized rather than more fundamental organizational processes.

While I am sympathetic to many of the criticisms of the field's outcome orientation (Staw 1980a), I will not in this review argue for a wholesale substitution of processes for outcomes. In my view, it is probably not the outcome approach per se that should be held responsible for the lack of progress

in micro OB, but the way outcome research has been conducted. To date, the outcomes of interest to researchers in the field have been extremely limited, and even the ways these few outcomes have been conceptualized have been restricted. Thus, in addition to describing recent research on the most prevalent outcome variables, this review will try to push the field a bit toward a reformulation of these traditional variables as well as an expansion of the list of outcomes relevant for future research.

The first and most extensive part of this review will concentrate on four of the most heavily researched outcomes, variables that still account for a very large proportion of the field's research: job satisfaction, absenteeism, turnover, and performance. For each variable, a summary will be provided of the major theoretical approaches and prevailing research trends. An exhaustive review of all recent empirical research will not be provided, since this would require a separate and lengthy paper on each of the subtopics. Instead, the review will emphasize the prevalent research assumptions and outline the possibility for new formulations. A principal goal of this section of the chapter will be to show how research on these four traditional variables can be revitalized by taking on a different point of view (e.g. employee as opposed to management) or some alternative theoretical perspective.

The second part of the chapter will consider briefly three additional dependent variables. A great deal of research has recently addressed job stress, one of the few variables now researched from the employee's point of view. Relatively unresearched, but still important, is the recent work on individual dissent and whistleblowing. Finally, of increasing future importance to organizations is the issue of creativity and innovation. Recent research and trends will be briefly summarized on each of these three subtopics, as they represent only a sampling of research that can be performed on newer outcome variables. The chapter will conclude with some general discussion of theory development and research in organizational behavior.

JOB SATISFACTION

Job satisfaction has probably attracted more research than any other dependent variable in the field. Because of its ease of measurement, as well as the continued dependence of the field on attitudinal surveys, satisfaction measures have played some role in a very large proportion of organizational research studies. At last count (Locke 1976) over 3000 studies contained some documentation or examination of job satisfaction.

While job satisfaction measures continue to be abundant in research (almost to the extent of being "throw-away" variables), a much smaller stream of studies have specifically addressed the issue. Research on job satisfaction per se probably peaked in the 1960s and then declined when the presumed link

between satisfaction and productivity was called into question (e.g. Schwab & Cummings 1970). However, satisfaction research has shown some resurgence of late as attitudes have been more specifically linked to absenteeism and turnover, once again providing an economic rationale for their study (Mirvis & Lawler 1977). Satisfaction research has also been aided by recent concerns over the quality of working life (e.g. Campbell et al 1976), the impact of work on mental health (Kahn 1980), and the relationship between work and family life (e.g. Kabanoff 1980).

Measurement and Meaning of Job Satisfaction

There is now wide acceptance of three job satisfaction measures: the Job Description Index (Smith et al 1969), the Minnesota Satisfaction Questionnaire (Weiss et al 1967), and the Michigan measure of facet satisfaction (Quinn & Staines 1979). Each of these is a simple additive measure of various aspects of the job, including supervision, working conditions, and the task itself. Very much out of favor are measurement devices which incorporate a particular theory of satisfaction such as need theory (Porter & Lawler 1968) or a weighted average in which some job factors are disproportionately emphasized over others (Herzberg et al 1959). Single items to measure overall or global job satisfaction are still in wide usage.

While much effort has historically been placed on developing reliable measures of satisfaction, little work has focused on the construct of satisfaction itself. With the exception of Locke's (1976) recent analysis of satisfaction as the fulfillment of individual values, there has been little debate about the meaning of satisfaction. The field's current usage of satisfaction is as a theory-free affective variable, yet the measurement of satisfaction probably involves additional conceptual baggage that leads one implicitly to discrepancy theories and models of social comparison. Dictionary definitions of the term usually note fulfillment or gratification, and it is not yet known what other connotations and cognitive schemata may be tapped by the term. Related but distinctly different terms such as job liking, vocational pleasure, or positive feelings may have different meanings, perhaps closer to general work affect. Thus, if we desire a relatively theory-free measure of job attitudes, measures such as Scott's (1967) semantic differential or Kunin's (1955) faces scale may be more appropriate than current indicators.

Correlates of Job Satisfaction

Over the last 30 years, most of the research on job satisfaction has been a rather atheoretical listing of variables that are statistically associated with work attitudes. Large-scale surveys as well as countless studies with more limited samples have examined the relationship between various working conditions, pay, supervision, promotion, and job features with satisfaction. As one might

expect, data show that satisfaction covaries with level of pay, degree of promotional opportunities, the consideration of supervisors, recognition, pleasant working conditions, and the use of skills and abilities (see Locke 1976 for a review).

The first problem with much of the correlational work on job satisfaction is that the determinants of satisfaction are usually measured by perceptions rather than more objective measures of the job situation. The spillover from job satisfaction to perceptions of the job environment on questionnaires make cause-effect inferences almost impossible (Staw 1977). This is especially problematic when questions about job features are asked in a value-laden way (e.g. "the pay is good," or "the job is challenging"). The fact that there are so few disconfirmations of common sense should, by itself, cue us to this problem. Seldom do respondents note on questionnaires that the job is satisfying because it is easy, does not involve responsibility for others, or allows the separation between work and family life. Thus, more research needs to be done on the design of questionnaires that are neutrally toned as well as greater reliance on the objective measurement of job environments.

In general, advances in understanding the causes of job satisfaction have not come from large-scale surveys which have noted many statistical correlates of satisfaction, but instead from more theory-driven data collections. Contributions to job satisfaction have arrived more from theories and research on job design, equity, leadership, and participation, than from the research specifically charged with job satisfaction. I will consider the research work in only two of these subareas as examples of recent advances.

Job Design

Research on job design is currently the most active forum for work on job attitudes. Although job design theories are often intended to be predictors of work effort and quality, relationships with job attitudes are more consistently found than associations with archival measures of performance. Job design research has also stimulated more fundamental debate over the formulation of job attitudes than behavior, with consideration being placed on the social construction of reality as well as more objective work conditions.

The dominant job design theory over the last 5 years has been Hackman & Oldham's (1976, 1980) Job Characteristics Model. This formulation has posited that five job characteristics (skill variety, task identity, task significance, autonomy, and feedback) contribute to internal work motivation and positive job attitudes. The Hackman and Oldham model is based on a need-fulfillment theory of motivation (e.g. Maslow 1954) and is derived from a long tradition of concern with intrinsic aspects of the job (e.g. Herzberg et al 1959). It is essentially a refinement of the earlier models by Turner & Lawrence (1965) and

Hackman & Lawler (1971), although there are greater efforts in the current work to present a unified theory of job design.

Being the dominant job design model, Hackman and Oldham's work has attracted a large share of criticism and has stimulated most of the new competing work on job attitudes. In terms of methodological problems, Roberts & Glick (1981) and Aldag et al (1981) have presented excellent summaries. The reliability of measurement, lack of discriminant validity with other attitudinal measures, reliance on perceptual rather than objective measures of jobs, and positive or negative halo among job characteristics all loom as potential problems. Even more fundamental is the fact that most of the supporting evidence for the Hackman and Oldham model comes from cross-sectional surveys where cause-effect inferences are difficult. Field experiments have provided very weak support for the theory, even though its implications for job redesign are rather direct (Oldham & Hackman 1980 have recently offered some explanations of this problem). Thus, while research on the Hackman and Oldham model has consistently supported the relationship between certain perceptions of work and job attitudes, we still do not know whether changes in objective job characteristics will change job attitudes and behavior as predicted by the theory.

In a theoretical critique, Salancik & Pfeffer (1977) have assailed the Job Characteristics Model as being a derivative of need satisfaction theory and subject at least indirectly to all of the vagaries and difficulties in testing models of human needs (e.g. Wahba & Bridwell 1976). In addition, Salancik & Pfeffer (1978) have offered a social information processing approach to explain job satisfaction. Rather than satisfaction being determined by intrinsic characteristics of tasks, it is, they contend, more a product of self-inference and social influence. Many experiments have shown, for example, that salient external rewards can decrease job satisfaction, though this literature is itself controversial (see Deci & Ryan 1980 and Sandelands et al 1983 for reviews). Self-inference of task attitudes has also been shown to be influenced by simple questionnaire manipulations of the frequency of behavior directed toward or against a particular task or activity (Salancik & Conway 1975). Neither of these streams of research, however, has had as much impact on job design and job attitudes as have several recent experiments on social influence in a work setting.

As posited by Salancik and Pfeffer, tasks can be ambiguous activities potentially interpreted in both positive and negative ways. Thus, the simple labeling of tasks by others has been found to affect task attitudes in several laboratory studies. In one study subjects were told that a task was either liked or disliked by people with previous experience with it (O'Reilly & Caldwell 1979), and in two other studies a confederate who worked along with the

subject noted positive or negative features of the activity (White & Mitchell 1979, Weiss & Shaw 1979). In each of these studies, positive labeling led to greater satisfaction than negative labeling.

Recently, in an effort to extend these findings to a field setting, Griffin (1983) experimentally manipulated both objective job conditions and social cues. Factory foremen were trained to provide cues about the job to their subordinates, and the effect of these cues as well as changes in more objective job characteristics were assessed. Results showed that perceptions of task characteristics (e.g. task variety, autonomous, feedback, and identity) as well as overall satisfaction with the job were affected by *both* objective job changes and social cues provided by the foremen.

At the present time, there is a conceptual stalemate between objective and subjective approaches to job design. Advocates of objective conditions being the determinants of job attitudes have relied on perceptual measures, no doubt capitalizing on subjective inference and halo in testing their models. Advocates of subjective conditions being the determinants of job attitudes have tended to hold job conditions constant or avoid the extremes of either boring or highly involving tasks. Thus, testing how much variance each approach explains may depend more on parameters of the tests themselves than upon the veracity of either of the models. Like the arguments of personality vs environment, a conclusive winner is not likely to be found. Future research, therefore, may be more productive in addressing boundary questions such as when objective changes will and will *not* be expected to change attitudes and when manipulations of social cues are likely to be major or minor events. Also useful will be studies to test the interaction of subjective and objective influences. Social influences and positive halo may be necessary factors for successful implementation of job redesign, and objective changes may be necessary to make credible many manipulations of the social reality surrounding jobs.

Comparison Theories

A second major group of studies concerned with job attitudes has taken the form of individual and social comparison theories. Comparison theories, like the social-information processing approach, have posited that satisfaction is not simply an additive function of the objective outcomes received by the individual. Comparison theories emphasize the choice of information sources about one's outcomes and the comparison between self and others. The major sources of research for this approach are work on adaptation level (Appley 1971), social comparison (Suls & Miller 1977), equity (Walster et al 1978) and relative deprivation (Martin 1981), not all of which has been carried forward or advanced in the organizational context.

At present, if one were to point to a dominant comparison model of satisfaction, it would probably be Lawler's (1973) formulation which incorporates

both social and self comparisons. This model notes that satisfaction is a function of the discrepancy between the level of outcomes desired and the perceived level of outcomes that actually do exist. Prior experience with various job outcomes as well as social comparisons are hypothesized to affect the desired or expected level of outcomes, while the evaluation of pay, working conditions, and other job features are considered to be determinants of actual outcomes received. Because the Lawler model is so global, little research has gone into testing this particular formulation. Its components have been addressed, however, through a number of subtheories of social and self comparison.

In terms of intraindividual comparison of outcomes, the clearest study has been a laboratory experiment by Ilgen (1971). By manipulating the level of performance over several trials, Ilgen created different levels of expectations. He then changed the performance level so as to violate the level of expectations in a positive or negative direction. The results showed that satisfaction with one's performance was as much a function of prior expectations as the aggregate level of feedback. Little work has followed this study in examining rising expectations in organizational settings or the impact of personal variables (e.g. age, seniority, education) on levels of expected outcomes. However, a recent laboratory study (Austin et al 1980) compared the relative impact of intrapersonal and interpersonal comparisons on satisfaction. Both processes were found to be significant determinants of attitude and of approximately equal magnitude. Given these results, it is unfortunate that almost all our attention in comparison theories has involved social as opposed to self comparison.

EQUITY THEORY In terms of social comparison models, equity theory has long dominated thinking in the area, though empirical research on equity has decreased drastically since the mid 1970s. For the conditions of underpayment, the hypotheses originally posed by Adams are now widely accepted, namely that underpaid workers will be dissatisfied, redress their inequity through lower effort, rationalize their underpayment through cognitive distortions, change social comparisons, or leave the field (Goodman & Friedman 1971). Adams' hypotheses for the condition of overpayment were the most controversial to the field, and they remain that way. Few researchers accept the notion that overpayment will lead to feelings of guilt and to subsequent behavior that may reduce the guilt such as increasing work performance.

As was the case over 10 years ago, two weaknesses plague equity theory. One is the ambiguity of the social comparison process, since the choice of a social comparison appears to be far from a deterministic process (Goodman 1977, Martin 1981). While equity experiments make salient a particular comparison other or limit the situation so that no information is available except as provided by the experimenter (e.g. "others have been paid \$10.00), natural

situations abound in ambiguity. To whom one compares himself may be a fluid enterprise, and the consequence as well as a cause of satisfaction. So far, the most thorough empirical investigation of social comparisons in an organizational setting has been a study by Goodman (1974) in which self, other (individuals, both inside and outside the organization), and system (the structure and administration of the pay system) referents were examined. In a series of recent studies, Martin (1981) also found deprivation to arise when individuals make comparisons across status groupings (i.e. secretaries comparing their outcomes with managers). Thus, although most theories of social comparison incorporate some notion of relevance or similarity for the determination of comparisons (Goodman 1977), the choice of referents may be subject to both social influence and conflict. Within the individual there may be a dynamic conflict between individual needs for accurate information versus the need for self-rationalization. External to the individual, there may also be conflicting social influences, such as when unions point to others who are substantially better off, while management notes how much things have improved.

The second major problem with equity theory has been its lack of specificity about the ways individuals can resolve inequity. Adams (1965) did not offer much help when he stated that inequity is resolved in a manner that is least costly to the individual. However, some real progress might be made by merging the theories of relative deprivation and equity, with Crosby's (1976) recent theoretical integration of deprivation research serving as a point of departure.

A MERGER OF EQUITY AND DEPRIVATION THEORIES As Crosby noted, deprivation occurs when a person sees someone who possesses some outcome x , wants x , and feels entitled to x . To this point, the notions of underpayment inequity and relative deprivation are quite parallel, since they each posit a comparison other and some basis of calculating a just or equitable reward. The departure between the two theories occurs with predictions about how relative deprivation is resolved or channeled into action. An intro-punitive as opposed to extra-punitive orientation, or felt responsibility for not having x , might serve as major channeling devices. When blame is internally placed, reactions might be internally directed such as rationalization (e.g. distortion of outcomes) to increase the perception of outcomes received or concrete actions (e.g. self-improvement) to increase future outcomes. When blame is externally placed, deprivation may be reduced by more externally directed actions such as efforts to obtain greater outcomes from the system (e.g. politicking, theft, decreased quality) as well as efforts to change the system itself (e.g. lobbying, strikes, grievances). Another dimension that may also affect channeling is the feasibility of obtaining x . When outcomes are feasible and responsibility is internally directed, self-improvement can be hypothesized, whereas rationalization may

be more likely to occur when outcomes are not feasible. When blame is externally directed, the feasibility of obtaining outcomes may determine whether acts that are destructive to the system (e.g. industrial sabotage) are used rather than efforts for constructive change.

To date, most of the evidence for the channeling of deprivation comes from political and sociological research. Crosby has, however, conducted a laboratory simulation and a cross-sectional survey which have provided some preliminary tests of these ideas (Bernstein & Crosby 1980, Crosby 1984). Unfortunately, little of this type of research has yet appeared in the organizational literature, although it would seem to present a major means of revitalizing work on equity and job satisfaction. According to this line of thinking, the major question is not whether underpayment or deprivation can cause dissatisfaction, but how this dissatisfaction will be channeled in terms of individual and organizational behavior. Dissatisfaction, therefore, would not be conceived as a uniformly negative outcome, but also as a possible means for individual and organizational improvement.

THE FUTURE OF GUILT AND OVERPAYMENT While underpayment situations clearly comprise the most practical problem area for comparison theories, much of the early research and controversy centered on overpayment situations. Initial research showing that overpayment can lead to increased quality or quantity of work (e.g. Adams & Rosenbaum 1962) was criticized on many methodological fronts (e.g. Valenzi & Andrews 1971), and the field has basically given up on the belief that guilt can be harnessed into productive work. Yet the recent Austin et al (1980) study did show self-reports of guilt in overpayment conditions, and a few studies have shown that recipients of overpayments may be more helpful to others (Gibbons & Wicklund 1982) or lobby on others' behalf (Notz et al 1971, Staw et al 1974). These results suggest that overpayment may be disconcerting, if not dissatisfying, and that some means of restoring equity may be sought (see also Walster et al for a useful integration of the altruism and equity literatures). Future research might therefore assess guilt reactions by examining situations where there are strong social bonds and the expectation of future participation. When individuals care about the reactions of others and anticipate a long-term relationship, inequitably large rewards may need to be redressed. When social groups are less involving or transitory, overreward may simply be perceived as a message that one is superior on some dimension, or that social comparisons should be shifted to the group that receives greater reward.

Alternative Directions

While job design and comparison theories have served as the major means of examining job satisfaction, almost no work has looked at the individual's mood

as a determinant of job-related cognitions. This is unfortunate because we know from psychological research that mood can affect the input and recall of information (Zajonc 1980, Bower 1981). The implication of a significant mood effect is that job satisfaction may be subject to any influence (both on or off the job) that can alter the affective state of the individual (see Caldwell et al 1983 for an initial study). Thus, working conditions and physical surroundings may be important for their effect on individual mood, which in turn could affect the perception and/or evaluation of various task characteristics.

A second change in satisfaction research would be to focus upon individual differences rather than contextual features in determining satisfaction (cf Epstein 1979). At the extreme, one might even think of job satisfaction itself as an individual characteristic, and study the persistence of job attitudes over time. The tendency to be happy or unhappy may vary little over time and context, since positive or negative features of employment can be deduced from most any job. Some preliminary research has shown job satisfaction to be relatively unaffected by changes in job status and pay and to be highly consistent over time (Staw & Ross 1983). Stable individual differences in job satisfaction may thus explain why it is so difficult to change job attitudes through job enlargement as well as other logically desirable treatments (cf Oldham & Hackman 1982).

A third redirection for satisfaction research, one already pointed to in the work on deprivation, is to recognize some of the functions of dissatisfaction. Dissatisfaction with one's performance may, for example, be the impetus for self-improvement. Dissatisfaction with the organization may also be the spark that alters the institution. Too often dissatisfaction is treated from the status quo position, as if changes by the individual are unnecessary or that lobbying, grievances, and protests are inevitably dysfunctional for the system. If research validates any of these predictions, dissatisfaction may in fact be a theoretical variable that is richer than we have given it credit for.

ABSENTEEISM

Like job satisfaction, absenteeism has long been a target of study by organizational psychologists. Behind the concern with absenteeism is the practical cost of the number of work days lost by the labor force, the cost of temporary replacement, and the overstaffing prompted by absenteeism. Steers & Rhodes (1978) recently estimated that the costs of absenteeism may run as high as \$26.4 billion per year when estimated days lost are multiplied by approximate wage rates.

Current Models

Research on absenteeism has followed very simple theoretical lines. Historically, absenteeism has been considered as a form of withdrawal from work and a

rather direct behavioral consequence of one's job attitude (Johns & Nicholson 1982). In general, satisfaction and other attitudinal variables are found to be negatively associated with absenteeism, but the magnitude of the correlations is rarely so large as to preclude other influences (Locke 1976). Therefore, efforts have also been placed into documenting personal, situational, and environmental variables which might help explain additional variance in this dependent variable. As one might expect, personal characteristics such as age, sex, family size, and health all relate to absenteeism. Likewise, situational factors such as incentive systems for job attendance, ease of transportation, flexible working hours, and external labor market conditions also appear capable of explaining some variance in this dependent variable.

Probably the most accepted theoretical model of absenteeism is the recent formulation by Steers & Rhodes (1978). They consider attendance as a variable which is determined by both motivational and ability factors. Motivation to attend, they contend, is influenced by job satisfaction as well as a host of external pressures such as fear of losing one's job, work group norms, and incentive systems. But motivation alone will not determine job attendance since one's ability to attend is often constrained by health, family responsibilities, transportation problems, and other involuntary factors. Using the Steers and Rhodes model, one can understand why satisfaction is often such a poor predictor of absenteeism. First, as an indicator of attraction toward the work role, satisfaction is only one of many reasons to attend. And, even if satisfaction is high, attendance may be constrained by other situational influences.

A slightly different approach to absenteeism is to consider whether one attends work or not to be a product of individual decision making with costs as well as benefits resulting from job attendance. A good example of this approach is a study by Morgan & Herman (1976). As those authors have noted, non-work activities often take precedence over work, and this may be especially true when the costs associated with absenteeism (e.g. paid "sick leave") are minimal. Likewise, if work is a major source of involvement for the individual or the penalties for nonattendance great, absenteeism will be minimal.

Other examples of the decision-making approach would be studies which show absenteeism to be strategically related to personal concerns. Rousseau (1978), for instance, found that the scope of one's nonwork activities was a better predictor of absenteeism than job scope, one of the typical correlates of job satisfaction. Chadwick-Jones et al (1973) found that women's absence peaked at times when household demands were greatest, and Nicholson & Goodge (1976) found absenteeism to cluster around holiday periods (see Johns & Nicholson 1982 for a more detailed review). But probably the best empirical study of individual decision making about absenteeism comes from a field experiment by Smith (1977). In a study of salaried employees working for Sears, Smith examined the relationship between job satisfaction and attendance

at a very unusual time. After an extraordinary snowstorm that crippled the transportation system in Chicago, there was an extremely high correlation between job attitudes and attendance, while employees at a control location (New York headquarters) showed nonsignificant but positive correlations. These results were particularly interesting for two reasons. First, they show that it is possible to explain a significant portion of absenteeism behavior with job attitudes. Second, these results highlight the difference between situations in which attendance is largely voluntary (e.g. when a legitimate excuse for absence is provided) and contexts in which behavior is constrained by potential sanctions.

Theoretical Trends

As noted, current theoretical trends on absenteeism are best characterized by the work of Steers & Rhodes (1978) and Morgan & Herman (1976). Starting with a large listing of personal and situational correlates to absenteeism, Steers and Rhodes have placed the literature into an aggregate model that has intuitive appeal. In contrast to this aggregate model, Morgan and Herman's expectancy approach would posit a more microscopic explanation in which individuals' perceptions of cost/benefit would affect the number of future absences. Even more microscopic would be Johns & Nicholson's (1982) recent suggestion that we treat each instance of absence as unique, because aggregation of absences over time may reduce our understanding of the event.

Two logical extensions of current approaches to absenteeism would be to more strongly emphasize either individual or situational characteristics. Some research already shows that a small portion of the working population accounts for a disproportionately large number of absences (Yolles et al 1975, Garrison & Michinsky 1977). Therefore, further studies of this "problem" group, either in terms of their nonwork environment, personal values, or causal reasoning, could be useful. Likewise, because rates of absenteeism can vary widely across companies, plants, and work groups, research might well examine norms about absenteeism and how absenteeism might be associated with other dimensions of the organizational culture (Johns & Nicholson 1982).

More Divergent Approaches

While most research has attempted to find new determinants of absenteeism or to reorder these determinants into a causal model that will explain substantial variance in the behavior, very little attention has been given to the construct itself. Johns & Nicholson's (1982) recent paper is a notable exception since it discusses the phenomenological meaning of absence, its possible use as an upward control mechanism, and the relation between absence from work and attendance to various nonwork activities. These research avenues hold potential because they seek an understanding of absenteeism from the individual worker's perspective as opposed to the usual viewpoint of management.

Another way to reconceptualize the absenteeism construct is to view it as an event with positive as well as negative consequences. As Staw & Oldham (1978) have noted, absenteeism can have two sides: a *technical dysfunction* from not being present to do the work that is expected, and also a *maintenance function* in which one's capacity to perform is increased. Most research only acknowledges the dysfunctional aspects of absenteeism, with the emphasis being placed on the number of days lost to work. Although it is well known that work outcomes can deteriorate under some personal or situational conditions, we rarely acknowledge the possible *costs* of attendance rather than absenteeism. One exception appears to be the legal limitation to the number of hours pilots can fly, air-traffic controllers can work, or bus drivers can drive. In these potentially hazardous occupations we readily acknowledge the possible costs to the public from fatigue or excess stress. For nurses and doctors we acknowledge the costs of overwork, but norms against such conduct are not often developed into rules or sanctions.

Staw & Oldham (1978) argued that jobs that are particularly dissatisfying or where there is a poor fit with individual characteristics require higher levels of absenteeism. They found a positive correlation between absenteeism and performance under dissatisfying conditions, but a negative correlation under more satisfying circumstances. Thus, it is possible that the positive contribution of absenteeism (e.g. a mental health break) can more than outweigh the costs of taking time off when a job is particularly frustrating or dissatisfying.

In a practical sense, one could argue that the term "absenteeism" should never be used when nonattendance has positive value. When executives spend time away from the office to visit prospective clients, organize long-range plans, or even to "decompress" from a strenuous travel schedule, these activities are not labeled as absenteeism because of their (assumed) productive purpose. Therefore, greater research needs to be directed toward identifying the productive use of time away from work by lower and middle level personnel as well as executives. The use of flexible working hours and the four-day work week have shown some potential in reducing absenteeism (e.g. Golembiewski & Proehl 1978, Ronen 1981, Narayanan & Nath 1982), but they have not yet been studied with the goal of specifying the optimal work week for different occupations. Nor have efforts to make lower and middle-level employees responsible for their own output (either through goal-setting or job design) been related to the workers' management of their own work attendance. Ideally, when work goals are emphasized, absenteeism should disappear as a managerial concern. Finally, as factory work is replaced by administrative and service tasks, and as computerization becomes widespread in these roles, the location of work may become more flexible. Already many staff functions can be performed at home on a computer terminal, and some firms encourage home as opposed to office work. However, as Becker (1981) has noted, the blending of work and family life may be a mixed blessing, because physical separation may

be needed for psychological rather than technological purposes. This is a major question for future research and one that can draw on new developments in the social ecology literature.

TURNOVER

Like the study of absenteeism, research on turnover has largely been stimulated by the desire to reduce the costs associated with personnel leaving industrial and governmental organizations. Turnover has therefore been viewed as an important organizational problem, one which is worthy of both theoretical explanation and predictive models.

Current Approaches

Historically, research on turnover has long utilized models of rational individual decision making. Starting with March & Simon (1958), turnover has been conceived as a conscious process where one evaluates present and future alternatives in deciding to stay or leave the organization. Mobley's (1977) more recent model of turnover, probably the most widely accepted at the present time, is a direct descendent of the March and Simon decision approach. Mobley's model posits that job and working conditions affect job satisfaction which in turn leads to thoughts of quitting, to evaluation of the utility of searching behavior, job search, evaluation of alternatives, comparison of alternatives vs the present job, intention to quit or stay, and finally to turnover or retention behavior. Although it would be unlikely for any single individual to go through all of these decision steps, Mobley's elaboration of turnover as a rational decision process has served as a useful guideline for research (e.g. Miller et al 1979).

Empirical studies have shown that satisfaction is generally correlated with turnover, but as in the case of absenteeism, the magnitude of the relationship is not large. Satisfaction, as one would expect, is more strongly related to other attitudes or behavioral intentions than actual turnover (e.g. Mobley et al 1978), while intentions to quit are more strongly related to turnover (Arnold & Feldman 1982). Just as March and Simon noted over 25 years ago, dissatisfaction may provide the impetus to look for another job, but such plans may be blocked if economic conditions are poor, pension plans nontransferable, and one's skills unsaleable (see Price 1977, Steers & Mowday 1981, Mobley 1982 for recent reviews).

Theoretical Trends

Although there has been a recent upsurge of theoretical interest in turnover, the research can be criticized for being fairly narrow conceptually. In one grouping, one can place many studies which have treated turnover in a rather

atheoretical manner, considering turnover simply as one of many possible dependent measures in the assessment of a new work procedure such as job redesign (e.g. Macy & Mirvis 1983) or a participative pay plan (Jenkins & Lawler 1981). Probably the only research on an organizational procedure with a theoretically developed link to turnover has been the study of realistic job previews (Wanous 1973, 1981). In a second grouping, one could place research on the process of turnover. The process research has taken an extremely rational decision making stance, examining the relation between intentions and actions or otherwise testing the cognitive links in the Mobley turnover model (e.g. Arnold & Feldman 1982, Miller et al 1978). These process studies have been criticized for examining theoretically unexciting relationships (Graen & Ginsburgh 1977), and it does appear that they have retread old ground from the expectancy and attitude-behavior literatures. Therefore, a possibility for improvement would be to incorporate recent ideas on attitude-behavior relationships (Fazio & Zanna 1981), but so far work linking job satisfaction, intention to stay, and turnover has not gone beyond commonsense theorizing.

One major advance in turnover research has been the recent work on organizational commitment. A program of research by Mowday et al (1982) has identified organizational commitment as being a prime determinant of turnover and as a more important predictor than job satisfaction. By commitment, Mowday et al mean a syndrome of variables such as belief in the organization's goals, willingness to work on the organization's behalf, and intention to stay in the organization. Conceptually, why this syndrome of variables are interrelated and how they can determine turnover remain unanswered questions. Also, because intentions to stay are included in the measurement of commitment, relationships with turnover may be overstated. Still, the empirical results showing the usefulness of commitment as a predictor of turnover are a welcome addition to the literature. In the future, alternative measures of commitment such as psychological investment in a job or side bets might be productively combined with the Mowday et al measures to predict turnover. Pension plans, number of children in school, home ownership, and friendship patterns are just a few of the economic and psychological bonds that may tie an individual to a job. The effects of these "behavioral" commitments on turnover have recently been documented in a longitudinal field study by Rusbult & Farrell (1983).

Some New Ideas

There have been three recent departures from traditional turnover research. In a very thorough yet innovative essay, Steers & Mowday (1981) discussed the importance of understanding the consequences of turnover decisions on the individuals making the decision to leave as well as other employees who are observers to the departure. For example, deciding to remain in an organization,

though not satisfied, may constitute a dissonance-arousing decision and trigger increases in subsequent satisfaction. Likewise, leaving an organization when one is reasonably satisfied may cause one to justify the decision after departure. Equally interesting are the consequences of turnover on those who stay in the organization. As Steers and Mowday note, those who remain may become dissatisfied simply by watching others leave for other organizations. Such demoralization may be conditioned, however, by the attributions stayers make about the reasons for leaving (see Mowday 1981 for a recent test of these ideas).

An alternative approach to studying the consequences of turnover was suggested by Staw (1980). He noted that turnover has usually been considered a negative outcome variable or cost to be minimized by organizations (e.g. Gustafson 1982). The costs of turnover such as recruitment, training, and possible disruption of operations are all very real, but they are not the only consequences of turnover. Perhaps too little attention has been paid to possible benefits of turnover such as hiring someone with greater skill, increased mobility of others in the organization, and possible innovation (Dalton & Todor 1979, Dalton et al 1981). Interestingly, sociologists have long been interested in executive succession as a possible source of organizational improvement (e.g. Gamson & Scotch 1964, Brown 1982), but this literature has had little effect on traditional models of turnover. The reason for this disparity may be that organizational psychologists tend to develop models of concern to personnel managers—the occupational group charged with reducing the costs of turnover rather than documenting its benefits.

Some obvious benefits of turnover come when the relation between skill and tenure has an inverted U shape, such as on sports teams in which performance peaks at an intermediate range of experience. Recently Katz (1982) has shown that research groups may also start to lose their productivity after they have remained together for many years. Future research needs to address the issue of whether turnover has an optimal level below which it should not be reduced. Although we know from U.S. Labor Department data (see Staw 1981) that slow-growing industries have the lowest rates of turnover, cause and effect is impossible to establish with these data. Ideally, there should be research to discover the level of turnover appropriate for organizations in environments with various rates of change or innovation. There should also be work on the optimal level of turnover for various types of jobs within organizations, since each job (e.g. research scientist vs accountant) may have its own performance curve. Finally, it may be possible to reap the positive benefits of turnover by job rotation and reassignment rather than having to displace people from the organization. These are important but as yet unanswered questions on the consequences of turnover.

Even though there is almost no empirical research on the consequences of turnover, Pfeffer (1983) has already moved this literature an additional theor-

etical step. He notes that turnover rates may be less important to organizations than tenure distributions. Turnover rates do not reflect whether newcomers continually leave an organization or whether departure occurs throughout the ranks. Turnover as well as hiring patterns may create an evenly distributed organization or one with a bimodal distribution, and he argues that a bimodal distribution can create the potential for organizational conflict (see McCain et al 1984 for an early empirical study). With a bimodal distribution, conflicts of interest can develop between two distinct factions that consume organizational resources, and there may be a reduction in the coordination necessary for organizational effectiveness. Though sports teams seemingly adjust their rosters to maintain a desired mix of talent (e.g. rookies and veterans) and universities often attempt to maintain a flat distribution of faculty ranks, organizational psychologists have not yet addressed these demographic concerns.

MOTIVATION AND PERFORMANCE

Since its inception, the micro side of organizational behavior has considered individual performance as its primary dependent variable. While most studies do not focus directly on performance per se, nearly every research write-up attempts to draw some implications for management or for current organizational practices. These implications are usually couched in performance terms, since the major audience and/or sponsors of research have been managers of business and governmental organizations.

Because studies of most individual and organizational processes try to draw implications for performance, it is difficult to formulate any boundaries around performance research. I will therefore, rather arbitrarily, consider theories of work motivation as most directly relevant to performance, since they are primarily devoted to predicting changes in performance, with measures of motivation sometimes even being used as proxies for individual performance. I will also consider two techniques for increasing performance, reinforcement and goal-setting, since they have attracted a great deal of attention and appear capable of producing reliable changes in individual behavior.

Current State of Work Motivation Theory

As recently as a decade ago, researchers in the work motivation area could be placed rather neatly into one of three theoretical camps. Reinforcement theorists (e.g. Hamner 1974) were primarily concerned with behavior modification, demonstrating the power of extrinsic rewards in changing behavior, and arguing that motivation is basically a noncognitive form of learning in which one's actions are shaped by the scheduling of rewards and punishments. Contesting this radical form of behaviorism were need theorists who argued that knowledge of the need state of an individual is essential to behavioral

prediction, because much of human motivation comes from inner drives which augment as well as define the value of external pleasures and pain (e.g. Porter 1961, Locke 1976). Largely allied with the need theorists were expectancy researchers (e.g. Vroom 1964, Lawler 1973) who posited that individuals seek to maximize valued outcomes, with those outcomes being determined by the reward system of the organization as well as the person's capability in achieving high performance.

In recent years, need theory has come under increasing attack on both methodological and theoretical grounds. Reliable scales of individual needs have been difficult to develop, and the leading model of human needs, Maslow's (1954) hierarchy theory, failed to be validated in several empirical tests (Wahba & Bridwell 1976). Aside from Alderfer's (1972) revision of Maslow's theory, direct interest in individual needs has therefore diminished. Still, need theory has continued to play a strong *indirect* role in several models of organizational behavior, having been folded into job design theory through needs for competence and personal achievement (Hackman & Oldham 1980) and integrated into expectancy theory through the use of valued outcomes (Lawler 1973).

Besides the deemphasis on explicit models of needs, motivation theory has recently witnessed a rapprochement between the reinforcement and expectancy perspectives. Debates between radical reinforcement and cognitive views of motivation have generally ebbed, with some notable exceptions (e.g. Karmel 1980, Locke 1980, Komaki 1981). In large part, expectancy theorists now acknowledge how previous reinforcement schedules can affect perceptions about future events—whether one's efforts will lead to accomplishment and whether accomplishment will lead to reward. Also, reinforcement theorists have started to acknowledge the cognitive side of learning with notions like personal efficacy (Bandura 1977), as well as renewed interest in behavior modeling and vicarious learning. The result of this accommodation can be seen in several hybrid motivation models such as those of Staw (1977), Naylor et al (1980), and Feldman & Arnold (1983). These models are amalgams of expectancy, reinforcement, and need theories, yet they are primarily expectancy-based, integrating past learning and needs into a hedonism of the future.

While expectancy formulations have probably assumed a dominant role among motivation models, their assumptions are themselves contested on empirical and conceptual grounds (e.g. Connolly 1977). One group of dissenters is composed of attribution researchers who have posited that intrinsic and extrinsic outcomes may not be additive in their effect on motivation. Starting with Deci (1971) and Lepper et al (1973), a whole body of research has developed over the question of whether extrinsic rewards can decrease intrinsic motivation (Deci & Ryan 1980 and Sandelands et al 1983). Although many

social psychological studies have demonstrated the interaction of intrinsic and extrinsic rewards, this subarea of research has yet had little substantive influence on models of motivation in organizational behavior. One reason for this lack of influence is that most field studies have failed to demonstrate a negative relationship between pay and intrinsic interest (see Boal & Cummings 1981). Within industry, pay may be the only real feedback one has on performance, thus constituting a source of personal achievement rather than external control (Rosenfield et al 1980). And since payment is expected for industrial tasks, monetary rewards may not constitute the kind of unusual external control shown to alter the self-perception of motivation within laboratory studies (Staw et al 1980). Unfortunately, little work has yet focused on the effect of pay upon *voluntary* activities—those organizational actions that are truly motivated by an intrinsic rather than extrinsic source. Likely candidates for research would therefore be the conversion of voluntary to paid work and the use of external controls on activities which are normally voluntary such as participation in social events, expressions of loyalty, or work beyond the call of duty.

A second group of dissenters to expectancy theory has been forming around the information processing perspective, and this group may be more likely to reshape our notions of motivation than theorists concerned with intrinsic motivation. Because expectancy theory is basically a model of individual decision making, it is subject to all the limitations of human cognition. As shown by the recent work of Langer (1978) and Taylor & Fiske (1978), many of our daily activities are either noncognitive or governed by the most crude analyses of the situation. In addition, when behavior *is* based on conscious decision making, such decisions may be subject to numerous cognitive heuristics and biases (Nisbett & Ross 1980). Thus, rather than positing a thorough analysis of gains and losses as described by expectancy theory, there is now more interest in specifying the crude schemas, scripts, and prototypes that are used by individuals in social decisions.

It could be argued that if theories of work motivation continue to be based on individual perceptions of behavior leading to outcomes, both the number of linkages and types of outcomes considered by individuals should constitute valid empirical questions. In situations where consequences are potentially large and where individuals are accountable for their actions, there may well be the careful screening of alternatives and assessment of rewards that expectancy theory now assumes (cf McAllister et al 1979, Tetlock 1984). However, in more routine situations, attention and cognition may be more limited. Empirical research should therefore focus upon those scripts and limited action plans that are actually used by organizational actors in various contexts, as opposed to conducting simple tests of normative models of motivation. Research should also focus upon ways in which positive work behavior can become the scripted

alternative, so that good performance will not call for either salient rewards or external exhortations.

Two Motivational Techniques

The two motivational techniques that have produced the most reliable changes in performance are behavior modification and goal setting. These two techniques are derived from extremely different theoretical positions, but in practice their operationalizations have often overlapped (Locke 1980). Behavior modification techniques, although derived from noncognitive reinforcement theory, frequently involve the setting of behavioral goals and instruction about desirable future behavior, in addition to the scheduling of positive outcomes such as verbal praise or bonus pay (e.g. Hamner & Hamner 1976, Komaki et al 1978, 1980). Behavior modification experiments often result in dramatic improvements in performance, significant in practical as well as statistical terms.

Goal-setting techniques derive from a distinctly cognitive approach in which individuals are assumed to formulate behavioral plans or strategies as a way to achieve personal values (Locke et al 1981). The principal addition of goal setting to a cognitive theory of motivation is the assumption that goals or intentions are an intermediate regulator of human action. A large body of research has shown that setting a specific goal leads to greater performance than general instructions to “do your best.” There also appears to be a positive relationship between task difficulty and performance, although few studies have tested goal setting in a context where a difficult goal may create opposition or lack of acceptance. Participation in goal setting has not been found to be an important determinant of performance, but again, few studies have investigated the role of participation in gaining acceptance of unpopular performance goals. Finally, as Locke et al (1981) have noted, little is known about the exact mechanism by which goal setting works—by directing attention, mobilizing energy expenditure, prolonging effort, or developing task-relevant strategies. Even with these uncertainties, however, goal setting appears to be one of the most robust tools currently available for improving performance in organizations.

Specifying “Correct” Behavior

Because goal setting and behavior modification techniques have been so successful in changing behavior, the “correctness” of such behavioral changes has sometimes been highlighted as a potential problem for organizations. While many early debates centered on ethical questions of manipulation, more recent attention has focused on the unintended consequences of motivation techniques. Kerr (1975), for example, has outlined many cases in which behaviors are modified in ways that are dysfunctional for the organizational system. Platt

(1973) has also outlined many individual and social traps in which short-term individual gains do not aggregate into longer-term, collective welfare. These questions are extremely important for conceptualizing the criteria of performance and how they are affected by behavioral changes, yet they remain virtually unexplored issues.

In most studies researchers have taken the organization's criteria of evaluation as accurately reflecting system performance. Often the construct of performance is relatively clear, as when physical or mechanical tasks must be completed with certain quality and quantity specifications. However, when tasks are ambiguous or involve the absorption of uncertainty (such as on administrative or managerial roles), the specification of "correct" behavior is more problematic. Naylor et al's (1980) recent theory of performance is explicit in its acceptance of managerially defined behavior as being task relevant, while most models of performance have made this an implicit assumption. My view is that a major emphasis of performance research should go into investigating how specific individual indicators of performance combine to create collective welfare or dysfunctions (Staw 1982). The performance of organizations is frequently found wanting, not for lack of motivation, but for performance of the wrong behaviors.

Although Naylor et al's recent model of performance does not address the relationship of individual to collective performance, it does make several advances in formulating the motivation construct. These authors note that increasing performance is seldom accomplished by raising the amount of energy expended by individuals, since total energy expenditure is probably rather constant over time. Instead, they argue that performance is raised by increasing one's attention to task-relevant activities as opposed to personal, family, or task-irrelevant concerns. By using this formulation, work motivation questions evolve from investigations of effort expenditure to issues of work involvement and the trade-offs between work and family life (Kabanoff 1980). This is a direction that should spur useful philosophical debate as well as empirical research.

Another issue that is provoked by the recent Naylor et al treatise is how individuals can better direct their energies on the job. In previous research, strategies for task accomplishment have not been addressed except by the implicit assumption that aptitude and ability are somehow fixed by individual differences. How individuals form performance strategies and how they can be improved are important questions that should not be assumed away as individual differences. To date, motivation has focused almost solely on the effort component of motivation and avoided the question of direction (see Terborg & Miller 1978 and Katerberg & Blau 1983 for notable exceptions). Logically, both the amplitude and direction of behavior can be governed by individual choice, subject of course to both external constraints and individual differ-

ences. The Naylor et al formulation has helped open this issue, one that could revitalize performance research.

Outcome Curves and the Multiple Purposes of Reward Schemes

Most research using a reinforcement perspective concentrates on the contingency between behavior and outcome. Likewise, research using an expectancy perspective has focused on the subjective probability that an action will be followed by a positive or negative outcome. Therefore, using either of these viewpoints, the contingency of outcomes could be stated by a curve relating behavior to outcomes (Lawler 1981). Unfortunately, little research has actually investigated the effects of various outcome curves, except for the simple linear positive relationship between performance and reward.

Numerous outcome curves are probably in existence within industry. In firms where company survival is dependent on key personnel, especially large raises may be given to high performers, thus creating a discontinuity at the high end of the curve. In other firms trying to rid themselves of poor performers, a discontinuity at the low end of the outcome curve could be present. Finally, in order to emphasize cooperation among employees, some firms may flatten the slope of the outcome curve, whereas other firms may heighten the slope in efforts to motivate most employees. Little is known about the prevalence of different outcome curves, how they relate to other organizational characteristics, and what their effects are likely to be on employee performance, turnover, or attitudes. Some preliminary ideas can be found in Staw (1983) and Naylor et al (1980), but empirical research has not appeared on these questions.

An Alternative Direction

Recently, one of the most severe challenges to conventional theories of work motivation has come, not from motivation theorists, but from organizational sociologists doing cross-cultural work (e.g. Cole 1979, Ouchi 1981). In comparing American and Japanese organizations, differences frequently have been cited in socialization, mobility, and production practices, and the popular press has often held out Japanese practices as the model to emulate. While there are many questions about the applicability of motivational practices across cultures (e.g. Schein 1981), these comparisons have highlighted what could be a fundamental omission in our motivation theories.

In organizational behavior nearly all models of motivation have been designed as direct forms of hedonism. Regardless of whether the driving force is thought to be prior reinforcement, need fulfillment, or expectancies of future gain, the individual is assumed to be a rational maximizer of personal utility. In line with these theories, our recommendations for practitioners usually attempt to link more closely personal welfare to performance, using contingent re-

wards, goal setting, or job enrichment (e.g. Latham et al 1981). In stark contrast, the Japanese model of motivation stresses attachment to the organization and achievement of organizational goals. Greater stress is placed upon cooperation, extending extra effort on behalf of the organization, loyalty and service to the long-term interests of the organization. Although there are many differences between the two approaches, a persistent theme dividing them is the relative emphasis upon collective versus individual motivation. Whereas Western models of motivation emphasize individual gain and self-interest, the Japanese system relies more heavily on motivation for collective welfare and appears to be more altruistically based.

Ouchi (1981) has outlined a syndrome of variables associated with Japanese management, but his descriptions have not specified which set of variables and by what theoretical mechanisms these variables operate on performance. A more explicit theoretical framework has been posited by Lawler (1982) which specifies how individual and organizational welfare can be linked through organizational practices such as participation, job redesign, and profit sharing. To Lawler, collectively oriented behavior is motivating if it contributes to organizational performance which, in turn, is linked to intrinsic or extrinsic rewards received by the individual, as follows:

$$\text{Collective Motivation} = \text{Prob}(P_i \rightarrow P_o) \times \text{Prob}(P_o \rightarrow O_i)$$

where

P_i = Performance of the individual

P_o = Performance of the organization

O_i = Outcomes for the individual.

In essence, Lawler's model reduces collective motivation to a question of individual welfare, relying on an expectancy formulation. It can be argued, however, that the model does not fully capture the collectively oriented form of motivation. Although there is no empirical research on the model, this formulation logically would be a weak predictor of cooperative acts, since one person can rarely affect the performance of an entire organization, and even if he or she could, the relationship between organizational performance and individual outcomes would be at best tenuous.

An alternative and perhaps more fruitful approach to collective motivation is to view cooperation as prosocial behavior (Puffer 1983, Staw 1984). Rather than building more elaborate hedonistic models to explain behaviors such as cooperation, service, and loyalty, they might be more parsimoniously described as altruistic acts toward the organization. Many individuals (especially managers and staff employees) are asked to perform or to make decisions on behalf of the organization in ways which are irrelevant (and sometimes even damaging) to their personal welfare, yet these prosocial behaviors are commonly performed. Therefore, recent research on altruism (e.g. Wispe 1978, Rush-ton & Sorrentino 1981) might best be integrated into a model of collective motivation, as follows:

$$\text{Collective Motivation} = \text{ID} \times \text{Prob} (P_i \rightarrow O_o)$$

where

ID = Identification with the organization
 P_i = Performance of the individual
 O_o = Outcomes for the organization.

The altruistic model of collective motivation represents the situation in which employees identify with the organization and act in ways to improve its welfare. The model does not require the explicit linking of individual and organizational outcomes, but relies on an empathetic relationship between the person and collectivity. Although not specified directly, this equation is consistent with Ouchi's emphasis on long-term employment and strong socialization, since they may be two mechanisms by which organizational identification is built. The model is also consistent with many of the assumptions in Mowday et al's (1982) recent work on commitment, although they have mainly attempted to predict turnover rather than prosocial behaviors within organizations.

At present, while collectively oriented behavior is frequently lauded as vital to organizational effectiveness, almost no research or theory has squarely addressed the issue. There needs to be research on the two models presented here as well as competing formulations of this potentially important side of motivation.

OTHER DEPENDENT VARIABLES

Although research on traditional variables such as job satisfaction, absenteeism, turnover, and performance can be faulted for being rather narrow, it is, as I have noted, possible to reformulate many of these older research topics into more vital questions. The investigation of organizational outcomes should not stop with these four dependent variables, however. Other variables may be of interest to individual participants in an organization and the general public as well as management. Though the list of additional outcome variables is potentially large, only three will be considered in this review. Job stress and dissent will be considered primarily because of their importance to individual employees, while creativity and innovation will be addressed as a contributor to organizational welfare.

Job Stress

Within the last 5 years there has been a burgeoning of research interest in job stress and an effort to specify its determinants. This rise of interest has paralleled the increase in stress research within social, cognitive, and clinical psychology. It has also resulted from a greater appreciation of the impact of work experiences on the mental and physical health of individuals, as opposed

to the usual emphasis on the contributions of individuals to the organization (Katz & Kahn 1978, Kahn 1980).

Physical aspects of the work environment can be a common source of stress and result in an impairment of performance, accidents, and sheer personal discomfort. Physical factors such as noise, excess heat or cold, poor lighting, motion, and pollution have all been documented as potential stressors in the work environment which can adversely affect individual behavior (Poulton 1978, Ivancevich & Matteson 1980).

At the more psychological level the research becomes more confusing. Tasks which are monotonous or unchallenging have been identified as stressful (Gardell 1976), but so have jobs with substantial overload (Margolis et al 1974) as well as jobs with high degrees of role conflict and ambiguity (Kahn et al 1964). Likewise, jobs with little influence or participation have been associated with stress (French & Caplan 1972), whereas jobs with substantial responsibility for other people are found to be contributors to ulcers and hypertension (Cooper & Payne 1978). Finally, transitions in one's life have often been identified as major sources of stress (Holmes & Rahe 1967), but a well-documented study recently found job relocation not to be a stressful experience for most people (Brett 1982).

Some tentative conclusions might be drawn from this contradictory sample of research findings. First, it appears that almost any dissatisfying or negatively labeled work attribute can be associated statistically with stress, especially when work attributes as well as stress are both measured by a single questionnaire. Thus, greater confidence must be placed in studies which measure job characteristics and symptoms of stress in an objective manner (e.g. Caplan 1971) than in studies which rely on self-reports of both working conditions and stress. If we have learned anything from the information processing perspective (Salancik & Pfeffer 1978), it is that working conditions and individuals' reactions to them may be reported in ways that are consistent and make as much sense to the respondent as the researcher. A second conclusion that can be drawn from stress research is that the antecedents of stress and how they affect health outcomes are likely to be complex and moderated processes rather than a set of simple, direct relationships. Recent research (e.g. Seers et al 1983) showing the importance of social support of peers, supervisors, and subordinates on the effects of stress is one example. Another is Karasek's (1979; Karasek et al 1981) work showing reports of stress (and coronary heart disease) to be a joint function of high job demands and low decision latitude. A third conclusion is that we should expect the potential stressors of the work setting to impact individuals differently. Kobasa (1979; Kobasa, Maddi & Kahn 1982) has identified individuals who are hardy or resistant to stress, while Lazarus (1981; Lazarus & Folkman 1982) has concentrated on how individuals differ in their coping strategies.

Because of the many contradictions and complexities in individual reactions to stress, research in this area is still at a preliminary stage. Sources of stress, aside from the most physical of working conditions (e.g. noise, heat, crowding), are often difficult to understand and to reduce in an objective way. Those features which make jobs interesting, important, and meaningful, for example appear to be quite similar to those which induce stress. It would therefore seem that the study of coping strategies (both physical and psychological) will hold the greatest promise for future research. Understanding how people can manage the stresses inherent in jobs will probably prove to be more important than changing the job itself. This may be especially true as the managerial component of jobs increase, with the resolution of conflict and ambiguity becoming the most essential parts of organizational work roles.

Dissent and Whistleblowing

Unlike the study of job stress, research on dissent and whistleblowing has not yet been integrated into a unified stream of research or even a recognized concern for the field. This is not to say that either the public or employees of organizations find the topic unimportant. Newspaper articles and television have made the public increasingly aware of the possibility of illegal or morally questionable practices by both public and private organizations. Individual employees have likewise become increasingly aware of their employment rights and grievance procedures (Aram & Salipante 1981) as well as their responsibility to dissent from certain policies or practices of organizations (Graham 1983).

Although organizational dissent is potentially an extremely wide topic, ranging from interpersonal role negotiations (Graen 1976) to system-wide upheavals (Zald & Berger 1978), much of the recent interest has centered on whistleblowing. From the individual's point of view, whistleblowing may sometimes be the only viable way to contest the organization's demands, providing an important source of individual freedom. From the organization's point of view, whistleblowing can be extremely threatening if negative information is brought to the outside media. However, when whistleblowing occurs within a single organization (e.g. divulging ills to a larger branch of government or corporate headquarters), it can be viewed as an extraordinarily loyal act. Federal legislation now protects whistleblowers, although reprisals to individuals by their supervisors and employing subunits still appear to be very common.

Recent work on whistleblowing has been primarily descriptive and philosophical. Discussions of the history of whistleblowing and its relation to business ethics can be found in Weinstein (1979) and Westin (1981). A recent case study of whistleblowing at BART (Bay Area Rapid Transit System) showed the decision to take grievances to the public depended on the perception

of mutual support among dissenting employees as well as the view that management would resist change (Perrucci et al 1980). A recent survey of people who filed employment discrimination charges investigated a number of determinants of retaliation against whistleblowers (Parmerlee et al 1982). Parmerlee et al found that organizations were more likely to retaliate against whistleblowers who were highly valued by the organization, perhaps because of their potential threat to organizational interests, but retaliation was also concentrated on those who were vulnerable to counterattack—those who lacked public support for their charges. Finally, a large-scale survey of over 8000 government workers found that while 45% of all employees have witnessed gross mismanagement of funds or illegal practices, 70% have not reported these acts to supervisors or other governmental authorities. The perception of inaction by the government rather than a fear of reprisal was cited by most respondents as the source of their silence (Office of Merit Systems Review and Studies, 1981).

Although whistleblowing has attracted recent attention, it could be viewed as only the last recourse an employee has in dissenting from organizational policies and practices. If, for example, we consider individual freedom as an important organizational outcome, various forms of participation and governance can be viewed as ends in themselves. Along these lines, Strauss (1982) recently reviewed forms of worker participation, not as simple determinants of worker satisfaction or productivity (e.g. Locke & Schweiger 1979), but as means of serving norms of democracy and ways of developing a social consensus for decisions. Further work on the role of dissent and freedom in organizational governance should therefore be encouraged. Of course, there should also be future research on the relationship between dissent and outcomes received by the organization. Dissent might, for example, actually improve organizational performance by contributing to the thoroughness of decision making, if the recent research on minority influence (Nemeth 1979, Nemeth & Wachtler 1983) can be generalized to organizational settings. Internal dissent might also provide the capability for organizations to adapt to changing environmental conditions (Weick 1979a), although no empirical research has yet been conducted on this issue.

Creativity and Innovation

While research on job stress and dissent addresses important individual concerns with employment by organizations, work on creativity and innovation may in the future become increasingly important from the organization's point of view. As a greater percentage of work becomes highly skilled and professionalized, the criteria of performance will likely become more ambiguous and subject to change. Therefore, questions of productivity may become translated into inquiries about working smarter rather than harder. These tendencies will

probably be compounded in situations where markets are rapidly changing or competition is fierce, such that innovation in these environments may become the organization's most important outcome variable.

At the individual level, there is a long stream of research on creativity, much of which is devoted to measurement issues and the relationship between creativity, intelligence, and various personality indices (see Barron & Harrington 1981 for a review). There is less work on the effect of external conditions such as supervision, job demands, and resources on creative behavior, but the literature is still substantial (e.g. Tushman 1977, Kohn & Schooler 1978, Basadur et al 1982). Unfortunately, most organizational research concentrates on the management of scientists and engineers (e.g. Allen 1977, Allen et al 1980) rather than upon ways to bring innovation into the production and general administration of the organization. Measures of originality and creative role making (Graen 1976) would therefore be useful additions to the measurement of individual performance in organizations, since determinants of efficient role behavior (e.g. quantity and quality) may be far different from the creative aspects of a role. The communality and possible trade-offs of these two sides of performance need to be addressed.

At the group level, there has been work on group composition and tenure upon the creative productivity of R&D groups (Katz 1982), structural dimensions of groups and group tasks which may impede the introduction of new ideas (Katz & Tushman 1979), and procedures for increasing group creativity (Maier 1970). Recent research on the effect of minority influence on the origination of new ideas (Nemeth & Wachtler 1983) and the effect of institutionalization upon conformity (Zucker 1977, 1981) are also noteworthy developments. However, little research has yet related group creativity to collective adaptation to new environmental conditions.

At the organizational level, literature ranges from population ecology research on the effect of environments on the life and death of organizations (Hannan & Freeman 1977, Aldrich & Mueller 1982, Freeman 1982) to the structural dimensions of organizations associated with innovativeness (Hedberg et al 1976, Galbraith 1977, Tushman 1977). Seldom are group or individual perspectives integrated into questions of organizational innovation or adaptation to new environments. One exception may be the recent theoretical work on organizational crises which merges individual reactions to threatening events with organizational responses to change (e.g. Billings et al 1980, Meyer 1982). A second exception may be the study of the escalation of commitment, in which rigidities in individual decision making are posited to account for (but not yet empirically linked to) organizational failures in adapting to changing environmental conditions (Tegar 1980, Staw 1982).

What is sorely needed at the present time are empirical studies which bridge the gap between research on individual and group creativity and organizational

innovation. One step that has already been taken is the drawing of theoretical analogies in learning and flexibility in behavior across multiple levels of analysis (e.g. Duncan & Weiss 1979, Staw et al 1981). However, the more significant and difficult work that needs to be undertaken is the examination of how individual, group, and organizational factors interact to make an organization creative.

CONCLUSIONS

This review has concentrated on the four most traditional dependent variables, emphasizing where research has been and where it could potentially go in each of these major subareas. Since research has been rather stable (even stagnant) on each of these organizational outcomes, I have emphasized ways to transform these variables into more interesting (or at least newer) theoretical questions. In my view, work on job satisfaction may be transformed into the study of affective mood and disposition as well as an inquiry into the functions of dissatisfaction. From absenteeism research one can derive a more detailed study of physical presence in organizations. From turnover research, there is already a trend toward examining the consequences of turnover and organizational demography. And, from work on motivation and performance there are emergent efforts to specify the type of behavior needed in organizations as well as nonhedonistic predictors of performance. Thus, from the narrow range of variables encompassing much of current micro organizational behavior, it is possible to move toward a more fundamental stream of work within each of the traditional outcome variables. It is also possible to expand the list of outcomes beyond short-term managerial concerns, as shown by the more recent work on stress, dissent, and innovation.

Outcome vs Process in Organizational Research

While this review has strictly followed on outcome approach to organizational research, it could be argued that processes are more important or fundamental. If understanding organizational functioning is the goal, then perhaps problems should not be defined by the management of firms, their employees, or even the general public. Research on organizations should, by this argument, follow theoretically derived questions rather than study either traditional outcome variables (e.g. job performance) or established organizational functions (e.g. pay systems, selection, and training).

Theoretical analyses of organizational processes have appeared in greater numbers in recent years. Pfeffer (1982) has, for example, reviewed most of the major theoretical approaches to both individual and organizational behavior. And, in terms of specific theoretical processes, conceptual reviews have been published on topics such as socialization (Van Maanen & Schein 1979),

information processing (O'Reilly 1983), decision making (Bass 1983), symbolism (Pfeffer 1981), justification (Staw 1980), time (McGrath & Rotchford 1983), and power (Pfeffer 1981, Porter et al 1981). At present, one of the "hottest" areas of research concerns the notion of organizational culture—whether it exists, how to measure it, its relation to organizational practices, and its possible contribution to organizational effectiveness (e.g. Deal & Kennedy 1982, Martin 1982, Louis 1983, Pandy et al 1983).

While theory-driven research is a necessary ingredient to our understanding of organizations, organizational researchers have had difficulty in sustaining interest in models that do not explain at least some variance in outcomes. Research on causal attribution, leadership, and group decision making are just a few of the areas that have declined because of this problem, and studies of organizational culture may be a future casualty [e.g. Martin et al (1983) found measures of culture to be unrelated to corporate performance]. Therefore, it may be advisable to bring multiple theories to bear on recognizably important problems rather than to frame organizational research around the testing of broad theoretical models. With the outcome approach, fundamental research would consist of middle-range theories that are capable of predicting specific outcomes, with processes and models organized around the type of outcome being examined. This approach would differ substantially from the usual practice of studying organizational processes across organizational outcomes or the testing of more general theoretical frameworks. It would also differ from the common practice of predicting outcomes by the atheoretical listing of correlates derived from empirical research.

From Method to Interdisciplinary Theory

As I noted at the outset of this review, the micro side of organizational behavior historically has not been strong on theory. Organizational psychologists have been more concerned with research methodology, perhaps because of the emphasis upon measurement issues in personnel selection and evaluation. As an example of this methodological bent, the I/O Psychology division of the American Psychological Association, when confronted recently with the task of improving the field's research, formulated the problem as one of deficiency in methodology rather than theory construction. An advisory group was formed and an excellent series of books have now been produced on innovations in methodology (Campbell et al 1982, Hakel et al 1982, Hunter et al 1982, James et al 1982, McGrath et al 1982, Van Maanen et al 1982).

It is now time to provide equal consideration to theory formulation. Of particular concern might be ways to integrate micro and macro organizational research on specific organizational problems or outcomes. Rather than arguing whether psychological or sociological constructs in general explain greater variance, there needs to be greater thinking and research on how to bring the

sides of the field together in explaining specific outcomes. One means of interdisciplinary research would be to dissect sociological constructs or to specify their intermediate mechanisms in psychological terms (e.g. Collins 1981). At present, sociologists find this threatening since it seemingly denies the macro level of analysis (e.g. Mayhew 1980, 1981). Actually, it should be no more threatening or less useful than the common practice of aggregating individual perceptions and attitudes into more global variables such as organizational climate or culture.

A second and perhaps more fruitful means of interdisciplinary research are studies that cross levels of analysis. The best developed of such research ventures are studies of the effect of organizational structure on individual attitudes and behavior (see Berger & Cummings 1979 for a review). Another example might be research on the effect of individual leadership style or the succession of leaders upon organizational effectiveness (e.g. Chandler 1969). To date, however, most of our efforts have been theoretical attempts to draw analogies between individual and organizational behavior or to speculate on the interplay between individual and organizational variables. For example, Hall & Fukami (1979) have discussed the relation between organizational design and individual learning; Weick (1979a) frequently has drawn analogies from individual cognition to organizational action; and Staw et al (1981) have reviewed the evidence or parallels between individual, group, and organizational reactions to threat. A summary of this literature as well as a thoughtful analysis of the problems and the possibilities for research to cross levels of analysis can be found in Rousseau (1983).

In my view, probably the best current candidate for progress in integrating micro and macro research is the examination of organizational innovation. Adaptation and flexibility are often mentioned as some of the most important criteria of organizational effectiveness (Steers 1977), and there is now widespread recognition of the problems of organizational growth as well as survival. A multilevel theory, which extends across the boundaries of psychological and sociological research, would therefore provide a valuable model for understanding how we can develop fundamental (and interdisciplinary) theory on organizational outcomes. Although multilevel research is fraught with methodological and conceptual difficulties (Roberts et al 1978), it is, I would argue, where the future of the field lies.

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