

# Motivation Theory in Industrial and Organizational Psychology<sup>1</sup>

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AFTER ATTEMPTING TO DEFINE motivation and motivation theory as they pertain to the study of individual behavior in organizations, the historical roots of current motivational concepts are traced. A distinction is then made between the treatment of motivation in the organizational literature and in traditional experimental psychology, and the research and theoretical issues prevalent in the experimental literature are discussed. Motivation theory pertaining to organizational behavior is then discussed in terms of a process versus content distinction. Within the process theory category, several variants of cognitive expectancy theory and the research they have generated are reviewed. The research data are found to be inconclusive and a large number of problems facing expectancy theory research are cataloged. Content theories are discussed primarily in terms of attempts to develop taxonomies of job related rewards and taxonomies of more basic human needs. Empirical taxonomic work is amazingly sparse. Equity theory, goal setting, the theory of need achievement, and attribution theory are then discussed in terms of how they supplement or contradict the broader expectancy model. Finally, conclusions are drawn concerning (a) the state of the motivation theory art in organizational psychology, (b) the disparity between the experimental and organizational literature, and (c) some new directions for future research in organizational settings.

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## INTRODUCTION

This chapter is meant to be a discussion of current motivation theory applicable to individual behavior in organizations. In the best of all possible worlds we might hope that such an examination would produce a set of critical questions, the answers to which would allow us to choose between directly competing theories and ultimately explain considerable variance in organizational behavior. Even the most naive realize that such an aim is presently out of reach. For several reasons, the various theories or models actually conflict at very few points. Motivational theories are not theories in any rigorous sense, and it is difficult to derive directly competing hypotheses that can't be transformed into identical predictions by some parametric manipulation which is not prohibited by the antecedent model. Also, no pair of models deals with precisely the same domain of behavior, although some overlap more than others. This condition has been forced on us by the complexity of the behavior domain we have chosen to study. It simply is not possible to make one theory or model do the job and still keep it within comprehensible bounds. We wish to argue that the various models presented in the literature tend to be complimentary rather than competing and most of them have their own particular utility. Viewed in this way, the organizational motivation literature, while it still tends to be piecemeal and lacking in common terminology, is anything but depressing. Instead, we feel that the conceptual gains made during the past twenty years have been substantial and it is a worthwhile endeavor to examine them.

The period 1960-1975 has seen a large exponential growth in the theoretical and empirical literature concerned with behavior in organizations. In general, as the number of individual contributions to a research domain increase, the pressure for summarization, integration, and codification grows. The individual bits and pieces become too much to handle. This pressure has produced

a number of recent reviews of organizational motivation literature (Heneman & Schwab, 1972; House & Wahba, 1972; Lawler, 1973; Lawler & Suttle, 1973; Mitchell & Biglan, 1971). The recent book by Lawler is an especially valuable source.

Our intent is not to go over all this same ground again. Rather, we will try to view motivation in a somewhat broader context than most of the above sources, almost all of which were written with more specific objectives in mind, and fill in the gaps around them.

### What Is Motivation?

There is a frequently appearing expression in industrial and organizational psychology which reads,  $\text{performance} = f(\text{ability} \times \text{motivation})$ . Many people experience severe stomachaches over the meaning attributed to the term motivation. The primary reason for such pain seems to be the felt need to equate "motivation" with a particular behavior or physical state. For example, the term sometimes is used synonymously with deprivation level, effort expended, general activity level, or degree of satisfaction. Following such a path can quickly produce frustration and a desire to reject the concept altogether. This would be an unfortunate choice. Motivation does have meaning if we take it merely as a summary label that identifies a class of independent variable/dependent variable relationships. That is, the term does identify a certain body of theory which can be distinguished from other theoretical domains, and we shall try to characterize its parameters.

It would help if we expanded the  $\text{performance} = f(\text{ability} \times \text{motivation})$  function a bit. Performance can be regarded as almost any behavior which is directed toward task or goal accomplishment. It may involve the production of a certain number of pieces, resolving a conflict with a co-worker, getting a project done by a certain date, or being seen as a satisfactory performer by one's boss. We will worry about

the discrete versus continuous properties of performance goals later. Based on what seems to organize the prevailing literature best, the expression can be rewritten as follows:

$$\begin{aligned} \text{Performance} = & f \left( \begin{array}{cc} \text{aptitude} & \times & \text{skill} \\ \text{level} & & \text{level} \end{array} \right) \\ & \times \begin{array}{cc} \text{understanding} & \times & \text{choice to} \\ \text{of the task} & & \text{expend} \\ & & \text{effort} \end{array} \\ & \times \begin{array}{cc} \text{choice of degree} & \times & \text{choice to} \\ \text{of effort} & & \text{persist} \\ \text{to expend} & & \end{array} \\ & \times \begin{array}{c} \text{facilitating and} \\ \text{inhibiting conditions} \\ \text{not under the control} \\ \text{of the individual} \end{array} \end{aligned}$$

It is intended to illustrate some obvious points. Performance is not synonymous with effort, ability, or a combination of the two. The choice to work on the task, the understanding of what is to be done, the choice to persist, and the environmental constraints all play an important role. It seems most meaningful to view motivation as a label for the determinants of (a) the choice to initiate effort on a certain task, (b) the choice to expend a certain amount of effort, and (c) the choice to persist in expending effort over a period of time. That is, motivation has to do with a set of independent/dependent variable relationships that explain the direction, amplitude, and persistence of an individual's behavior, holding constant the effects of aptitude, skill, and understanding of the task, and the constraints operating in the environment.

### What Are Motivational Theories?

This question is necessary because there are several major classes of theory which operate in different parts of the motivational domain. To imply that all the available theories or models address themselves to the same set of questions is misleading. To com-

pare and contrast them, a certain amount of pigeonholing is necessary.

A major distinction pointed out by Campbell, Dunnette, Lawler, and Weick (1970) is between *mechanical* or *process* theories and *substantive* or *content* theories. The former attempt to provide a generalized explanation of the processes involved which lead to choices among alternative courses of action, varying degrees of effort expenditure, and persistence over time. Process theories first try to define the major variables which are necessary for explaining choice, effort, and persistence. For example, incentive, drive, reinforcement, and expectancy are major variables appearing in various models. Such theories then attempt to specify how the major variables interact to influence particular dependent variables. A simple example might be the assertion that an individual will choose the course of action that leads to outcomes with the greatest total utility to him. Drive theory, reinforcement theory, expectancy theory, and equity theory are all process theories.

Content theories are more concerned with trying to specify the substantive identity of the variables that influence behavior and less so with the process by which they do it. That is, what are the specific rewards people want? What are the basic needs which they try to satisfy? What incentives are the most powerful? Thus a content theory may try to identify the specific entities within a more general class. For example, it may assert that promotions, salary increases, job security, recognition, and friendly co-workers make up the general variable we call job performance outcomes. Another theory may suggest a substantially different list. Although there are usually additional statements which imply how these variables influence the individual, a content theory is not centrally concerned with specifying the precise form of the interactions among variables.

Besides being different in form, these two kinds of theory spring from essentially different traditions in psychology (e.g., see

Madsen, 1965). Process models such as those articulated by Thorndike, Hull, Spence, Hebb, Tolman, and Atkinson have been generated almost exclusively by the experimental side of the science; while the content theorists such as Freud, McDougall, Murray, McClelland, and Maslow have been in the clinical-differential mold. It is of some note that industrial and organizational psychology is one of the few domains in which these two orientations are being used jointly.

Unfortunate though it may be, motivational theorists can also be dichotomized relative to whether their empirical developmental work has been carried out on human or infrahuman subjects. To a certain extent, this phenomenon has pushed the respective theories farther apart than the original intent and the theoretical issues which developed in one set tended to lose relevance for the other.

### PROCESS THEORY

To understand many of the issues centered in current process theories it would be helpful to consider briefly their conceptual predecessors. We need not go back to the Greeks even though a number of motivational issues had their intellectual beginnings there (e.g., see Cofer & Appley, 1964).

#### A Bit of History

As noted by Birch and Veroff (1968) and Madsen (1965), attempts to postulate a formal explanation for the direction, amplitude, and persistence of behavior are of relatively recent vintage. Before the eighteenth century any notion of determinism was largely absent from discussions of behavior. Rationality and free will were the dominant antecedents and concepts, such as motives, needs, desires, etc., were given little credence. However, simple observation of human and animal behavior gave rise to an increasing realization that there were forces beyond rational will that seemed to

exert control, especially with regard to animal behavior (Bolles, 1967).

Descartes (1911) attempted to reconcile these observations with prevailing philosophy and theology by invoking a duality of mind and matter. Rationality was retained as an explanation of human behavior; but animal behavior was given over completely to mechanical responding set in motion by instincts. In many respects Descartes's concept of rational choice is very similar to cognitive expectancy as it appears in the models of Edwards (1961), Peak (1955), Vroom (1964), and others. Man makes choices among behavioral alternatives on the basis of his knowledge. For Descartes, the crucial knowledges concerned what was optimal for enhancement of man's God-given nature. Contemporary cognitive theorists are content with notions such as perceived utility and instrumentality for need satisfaction.

Much of the duality was wiped away and motivational concerns took on a more modern form when the English associationists and utilitarians articulated their hedonistic model (Cofer & Appley, 1964). Bentham's "Hedonic Calculus" postulated that individuals consciously calculated the relative pleasures and pains of various outcomes provided by alternative actions and sought to maximize their total pleasure. It's not all that long a jump from hedonic calculus to subjective expected utility (e.g., Edwards, 1961), and the foundations for a cognitive-incentive type theory of motivation had clearly been laid.

Two additional developments created a number of theoretical issues that still prevail in the motivation domain. The English associationists introduced the idea that at birth the mind is a clean slate and knowledge, desires, preferences, etc. are built up through associations of contiguous events. Such a notion was the forerunner of the learning, or associative component, of later models. This learning element was incorporated in a hedonistic framework by the associationists and was used to explain why

particular outcomes came to be evaluated as pleasurable or painful.

A second major development was Herbert Spencer's transformation of Darwin's model of biological evolution into a theory of psychological determinism. At the species level he postulated that since organisms indulged in pleasurable activities and avoided painful ones, a correlation developed between behaviors yielding pleasure and behaviors facilitating survival. The same model was applied on the individual level. Since individual behaviors which lead to pleasure are treated favorably by the environment, Spencer hypothesized that the organism's nervous system would evolve in such a way as to make that behavior more likely to occur in the future. Please note that Spencer's theory marks the first articulate conceptualization of the principle of reinforcement and shifts the pleasure principle from a goal or incentive orientation, in which the cognitive *expectation* is critical, to a focus on an individual's reinforcement history. Hedonism has been transformed from a pull theory to a push theory and the battle lines between the two were drawn.

The major component of twentieth century process theory that remains to be identified in earlier work is some conceptualization of how behavior is energized or set in motion. An explanation of behavior virtually requires some parameter representing the force that makes it go. The contemporary labels are *need* and *drive* and they are taken up in the various models discussed below. Their antecedents relative to animal behavior are relatively clear since the energizing force of *instincts* was frequently postulated, even before Descartes (Bolles, 1967). On the human side, various thinkers talked vaguely of impulses and desires but it was not until Darwin's evolutionary model introduced biological explanations into behavioral analyses that instincts were used to explain human behavior. McDougall (1908) gave the term *instinct* its greatest refinement but also stimulated an overuse of the con-

cept and lists of instincts proliferated beyond reason during the early part of this century. Partly as a result of this sheer promiscuity and partly because it was difficult to deal experimentally with innate behavior patterns, a new concept was needed and Woodworth (1918) coined the term *drive*. He defined the variable as neither innate nor specific to particular behaviors but as a unitary concept which represented an organism's overall activity level, and it was in this form that drive supplanted instincts in experimental psychology.

What this historical analysis is meant to illustrate is that most of the ingredients of contemporary process theories of motivation had been formulated before the twentieth century and before the migration of psychology to America. What remained was for these concepts to be refined and cast in theoretical models that were amenable to the empirical investigations of twentieth century science. For better or for worse this usually meant bringing them into the animal laboratory.

### Some Contemporary Issues

Most, but not all, of the theoretical and empirical development of process theory before 1960 took place at the infrahuman level. Some of the issues considered and principles developed have relevance for organizational behavior and some do not. It would be instructive to consider such issues relative to their possible contribution to our understanding of human behavior in organizations.

#### Determinants of Action: Three Major Theoretical Positions

Reinforcement, drive (or need), and incentives constitute three motivational determinants of behavior that have been used either singly or in combination to form the major motivational models of the twentieth century developed before 1960. The names

which loom largest as spokesmen for the various combinations are Clark Hull, B. F. Skinner, and Kurt Lewin.

Hull's theoretical life spanned two major phases and recapitulated a major portion of motivational phylogeny. Sometime before 1937, Hull explained behavior primarily on associative or reinforcement grounds (Bolles, 1967). That is, one could account for an organism's actions on the basis of its prior history of reinforced S-R connections. Hull was thus almost completely dependent on the Law of Effect as articulated by E. L. Thorndike. However, Hull eventually came to the position that S-R associations were not an adequate explanation of how behavior was energized and his model was expanded to include the drive component. Behavior or reaction potential ( $sE_R$ ) was viewed as a function of two principal components which combined in a multiplicative fashion. The associative component which gives behavior its direction was represented by  $sH_R$ , or habit strength, which was a function of the magnitude, frequency, and latency of the amount of reinforcements an organism had been given in the past for emitting a particular response. The motivational, or energizer, component was represented by drive ( $D$ ) which indicated the general level of pressure for activity. The resulting equation,  $sE_R = f(sH_R \times D)$  became a psychological household word. For Hull,  $sH_R$  represented learning and  $D$  represented motivation. Difficulties in defining these two components still persist and will be briefly enumerated below.

In what was perhaps a third phase of Hullian thought the basic model was altered to accommodate subsequent experimental results that the original algorithm could not handle (Hull, 1952). The discrepant data pertained to the abrupt shifts in behavior produced by changes in reinforcement magnitude (e.g., Crespi, 1942). The original  $sE_R = f(sH_R \times D)$  formulation incorporated reinforcement magnitude as one determinant of habit strength (i.e., learning). Thus changes in reinforcement magnitude

should produce only gradual changes in behavior as the associative bonds gradually become stronger or weaker. To accommodate the more sudden shifts the model was changed to  $sE_R = f(sH_R \times D \times K)$  where  $K$  represented the incentive value of the reward and changes in  $K$  would be reflected immediately by changes in  $sE_R$ . In yet a later modification of the Hullian model, Spence (1956) postulated that  $sE_R = f[(D + K) \times sH_R]$ . Within this model, behavior can be activated either by a significant drive state or the pressure of an incentive. These different versions of what explains behavior have stimulated a great deal of research within the experimental side of psychology and have created some of their own measurement and explanatory problems. We will touch on some of them later on.

Lewin (1951) was perhaps the only major process theorist who built his model around human behavior rather than the animal laboratory. In bare outline, his theory postulated that individuals have, at any point in time, certain physiological and psychological needs. Two consequences flow from the existence of an individual need structure. Needs create a state of tension which the individual attempts to relieve through appropriate action. They also influence the perceived attractiveness of various actions or outcomes in terms of their ability to relieve tensions. The perceived attractiveness of an activity was referred to as its *valence*. Lewin's concept of need was not too distant from Hullian drive in terms of its tension producing and energizing properties, even though Lewin spoke of an individual structure of specific needs while Hull viewed  $D$  as a generalized energizer. However, instead of combining tensions with associative strength to account for behavior, Lewin viewed the force on an individual to be a combination of the push of need tensions and the pull of highly valent outcomes. Lewin explicitly rejected reinforcement history as a predictor of behavior and preferred to know what value the individual *expected* from certain actions.

Thus an individual could take action directly counter to his reinforcement history if some new influence (e.g., new information) led him to assign a higher valence to it. This view was the first major twentieth century step toward incorporating human thoughts and expectations within a process model of motivation. It is the principal foundation underlying the bulk of current theory pertaining to work motivation.

The third major dominant force is Skinner who rejected theory building as a legitimate endeavor and preferred to base his explanation of individual actions on one overriding engineering principle, reinforcement. For Skinner, needs, drives, tensions, or other internal states are anathema and predicting behavior on the basis of perceived incentive value or valence is an indirect way to proceed and potentially misleading. To understand behavior and to control it, what we need to know are the reinforcement contingencies to which an individual or class of individuals has been responsive in the past. Upon this knowledge entire cultures can be built, changed, or destroyed (Skinner, 1948, 1971).

The Skinnerian position should be distinguished from a more theory oriented "pure associationism" view of motivation which retains certain parts of the drive concept. For example, as noted by Weiner (1972) association theorists such as Postman and Estes distinguish between the stimulus properties of a drive and the energizing properties. The internal stimuli produced by a drive state can be incorporated in a learning, or associationist, framework in the form of discriminative stimuli that influence response selection. The concept of drive as a generalized pressure on the organism to act is thus exorcised.

Thus our three major theoretical positions have been laid out. Hull combined reinforcement history and drive, or need state. Lewin combined need state and the incentive value (valence) of outcomes which were expected to occur if action were taken. Skinner focused exclusively on the orga-

nism's reinforcement history. What kinds of empirical questions have these theoretical positions generated? Since the Lewinian position underlies so much of the study of motivation in organizations, the issues and questions associated with need-expectancy-valence concepts will unfold in subsequent sections of this chapter. What we would like to do now is consider a brief list of research questions generated by the experimental and infrahuman literature. Such a list may help us delineate relevant issues that are pertinent to human behavior in organizations.

### Recurring Experimental Questions

What follows leans heavily on the review work by Atkinson (1965), Berlyne (1967), Bindra (1969), Birch and Veroff (1968), Black (1969), Bolles (1967), Cofer and Appley (1964), Kendler (1965), Madsen (1965), Weiner (1972), and Wike (1969).

1. How should the concept of drive, or need, be defined? Historically, drive was referenced to level of deprivation in physiological needs. The physiological referent soon proved untenable and for some (e.g., Miller, 1948) drive became synonymous with the need to reduce any strong stimulus, regardless of whether it was acquired (learned) or physiological. However, it did not take long to show that both animals and men will act to increase the intensity of certain stimuli, and constructs such as the curiosity drive were invoked. Current thinking seems to center on an optimal or homeostatic level of stimulation which may be influenced by many parameters associated with (a) the individual, (b) the situation; and (c) the particular drive at issue. A state of high drive is then the discrepancy between the current level of stimulation and the optimal level. Thus an individual in an employment role may have a drive or need for security, but his objective is to optimize, not minimize or maximize, the level of stimulation created by the need.

2. Is drive a unidimensional or multidimensional construct? Hull was clearly on

the side of a generalized activator of behavior. Regardless of whether drive level was increased by food, water, or sex deprivation, the effect on behavior should be the same. This position has been relatively well devastated (Atkinson, 1965; Bindra, 1969), but the alternative of postulating many specific drives is uncomfortable to contemplate theoretically. It is interesting to note that Bolles (1967) and Berlyne (1967), both hard core experimentalists, have called for what amounts to a factor analytic approach to obtaining a drive taxonomy.

3. By what mechanisms does drive influence behavior? Hull's original formulation stated that  $D$  was supposed to energize any and all behavior. It interacted with associative history (learning) to determine the *strength* of the dominant response, which had already been *selected* via learning. However, there are really two major alternative roles that drive could play. One focuses on the reinforcement properties of changes in drive. The other involves the effect of drive level on the incentive value of a stimulus, or valence of outcomes. This distinction is one of the major differences between a Hullian and an associationist type model, as mentioned in the previous section. The distinction is synonymous with the backward working or "push" characteristic of reinforcement and the forward working or "pull" characteristic of incentives (Weiner, 1972). To oversimplify, the question being asked is whether both  $D$  and  $K$  are necessary in the Hullian formula  $sE_R = f(sH_R \times D \times K)$ . If we concentrate on the reinforcing properties of drive, we again come up against the question of what kind of manipulation is reinforcing. Is it (a) drive reduction, (b) an increase in drive, or (c) a change in the direction of some optimal level that is reinforcing? The experimental literature suggests that it can be all three. The incentive view creates questions about how particular needs or drives ascribe values to specific stimuli, which is certainly a popular question in the organi-

zational literature. Running through both arguments is the question of whether the relationship between change in need state and change in incentive or reinforcement value is a continuous function or is discontinuous at one or more points.

4. Is the influence or reinforcement primarily a "learning" or "motivational" phenomenon? That is, did Hull (1952) and Spence (1956) make the right move when they factored  $K$  out of  $sH_R$  and made it a separate "motivational" component? In the experimental domain this question appears as a consideration of whether reinforcement acts to facilitate the gradual building up of associative connections or whether it operates on response strength independently of associative strength. Kendler (1965) argues that it is not possible to separate the two issues either conceptually or experimentally. However, the issue is important because if traditional learning concepts can explain all the relevant features of response then there is no need for a body of theory labeled motivation. In spite of the experimental difficulties involved both Bindra (1969) and Black (1969) argue strongly that learning or association cannot explain a number of experimental findings and that reinforcement indeed has a large motivational component. The most frequently mentioned datum is that a change in reinforcement magnitude produces abrupt shifts in behavior that cannot be accounted for by a gradual building up of associative bonds.

5. Consideration of the reinforcement literature also forces a confrontation between the Skinnerian and Hullian models. Do we really need concepts such as drive and incentives or can reinforcement explain it all? Attempts to demonstrate the independent effects of reinforcement and  $D$  have been singularly unsuccessful, except in the instance of food deprivation. Most of the experimental effects produced by variations in drive level have also been produced by changes in reinforcement magnitude or reinforcement latency. In sum, demonstrating

the independent effects of  $sH_R$  (learning),  $K$  (incentives), and  $D$  (drive or needs) has been experimentally slippery and scientifically frustrating. Perhaps it would be better to follow Skinner's dictum—reject all constructs involving surplus meaning, and stick to the observable effects of reinforcement, where a reinforcer is completely defined by the operations involved.

6. The Skinnerian point of view is perhaps not so simple as it first appears. For example, suppose we consider the following question a bit more closely. What is a reinforcement? It is very difficult, even in principle, to define reinforcement. Black (1969) notes at least two different uses of the term. First, it can refer to the empirical principle that certain stimuli or events can apparently alter the probability of a response if they follow that response closely in time. Such stimuli are called reinforcers. Second, reinforcement is often used to describe a set of operations performed by the experimenter as in the statement, "the subject was reinforced for every fifth response." Black (1969) notes that it is now the behavior of the experimenter, not the subject, that defines a reinforcement. The distinction creates certain difficulties. For example, how do we know that every fifth response, as perceived by the experimenter (or his recording device), is always the same response, or that there indeed were four, and not three or six, responses in-between each reinforcement? Such questioning exposes the fact that within experimental psychology there is also no conceptual formulation of what is a response. This may not be a problem in a highly controlled laboratory setting but when the behavior under consideration is more complex, the question of what responses are being reinforced is paramount. Also, the distinction between defining reinforcement in terms of subject versus experimenter actions leads to situations such as  $E$  offering a reward but  $S$  refusing to take (consume) it. Has a response been reinforced or not? If so, what is the real rein-

forcer? Parallel problems in the organizational situation are obvious.

In an attempt to get out of this bind, Black (1969) prefers to talk about "reinforcers" as stimuli which have in the past changed the probability of some response and "reinforcement" as the act of presenting reinforcers to subjects.

7. A number of heavily researched questions have revolved around reinforcement parameters. For example, what governs how secondary reinforcers develop? That is, under what conditions can a previously neutral stimulus become reinforcing? What are the asymmetries in behavioral effects between presentation of a positive reinforcer and removal of an aversive or negative one? What are the effects of punishment (presentation of a negative reinforcement after a response is not the same as removal of an aversive one after the response has been made)? What's the optimal reinforcement schedule? What is the effect of delayed reinforcement? How do variations in reinforcement magnitude affect the response?

The experimental evidence is relatively clear on a number of points. The powerful effects of variable ratio reinforcement are well known. It is also clear that the magnitude of reinforcement is associated with the strength and latency of a response. Unfortunately, the asymmetry between positive conditioning and aversive conditioning is not well known and there are no generalizations to offer. Punishment does weaken a specific response if an alternative response is permitted. However, by necessity in most instances, the response which is reinforced is that elicited by the punishment itself. Since this is almost always some sort of fear or withdrawal response the societal or organizational value of punishment can legitimately be questioned. Evidently, it is not possible to produce competing behavior via punishment such that the new response can be positively reinforced (Bolles, 1967).

From the standpoint of organizational psychology, perhaps the most serious gap in

the experimental literature is the lack of any definition for a response. In the laboratory, the question may be moot but in the organization it is the touchstone of the entire enterprise.

8. What is the locus or mechanism of reinforcement? That is, what makes a reinforcer reinforcing? Such a question would not concern a Skinnerian, but it has concerned others. As noted by Berlyne (1967), most of the experimental work has revolved around three possibilities. The first involves the change in stimulus conditions, originally stimulus reduction. Following this route leads directly to a search for the crucial stimuli. Is it stomach contractions (altered by eating), the sight of an onrushing car (altered by running), or the fluid pressure in the cells (altered by drinking)? The second alternative suggests that consummatory behavior itself is reinforcing. The experiments comparing the reinforcing properties of saccharin versus sugar (e.g., Sheffield & Roby, 1950) and those where food is allowed to reach the stomach normally versus being injected through a fistula (e.g., Berkun, Kessen, & Miller, 1952) have been attempts to determine the reinforcing properties of the consummatory act itself. A third alternative involves changes in the central nervous system. That is, considerable recent research (e.g., Miller, 1957; Olds, 1956, 1962; Olds & Milner, 1954; Roberts, 1958) has centered around trying to find specific locations in the brain whose chemical or electrical stimulation will reinforce behavior.

Premack (1965) has recently offered yet another conceptualization for what makes a reinforcer reinforcing. His explanation is centered in the response and says that if the situation is such that a low probability response is followed by a high probability response, the latter will reinforce the former. For example, turning left in a T-maze is a low probability response for a rat and eating at the goal box is a high probability response. Thus, any response can serve as a reinforcement if the situation can be arranged properly. A rat may operate an activ-

ity wheel for a chance to lick water from a cup or vice versa. Similarly, an individual may work hard to be able to spend money or he may spend money for a chance to work hard. It depends.

In sum, research in this area has produced a number of interesting hypotheses but there is as yet no definitive answer as to what makes a reinforcer reinforcing. Future work may or may not produce unifying concepts which will tie together consummatory acts, response probabilities, and changes in the central nervous system (CNS).

9. Berlyne (1967) argues that the concept of drive has indeed foundered on our inability to give it a reasonable definition and would agree that reinforcement can assume most of its functions. However, it may yet be possible to preserve its role as a general behavioral energizer if we root it firmly in an organism's physiological functioning. In this form, drive level has been defined as *arousal* or *activation* level and has spawned a very recent but large and growing literature. We can thus ask questions concerning the locus of arousal and how it interacts with rewards and incentives to influence behavior.

The precise definition of arousal is an active issue and the arguments are analogous to those centering around intelligence. Berlyne wishes to deal with a general factor derived from the first principal component. Others (e.g., Lacey, 1967) believe the low intercorrelations among some measures suggest a number of common factors which have different behavioral effects. The issue is not resolved. Variables frequently used to measure arousal are such things as heart rate, galvanic skin response (GSR), certain derivations of EEG recordings, and the level of stimulation in certain areas of the reticular formation and hypothalamus.

When viewed in this physiological framework, it seems permissible to make a few generalizations from the empirical evidence. For example, arousal can be made experimentally independent of reinforcement and it does enhance the effects of rein-

forcement under certain conditions. Changes in arousal may also serve as positive or aversive reinforcements in their own right. Whether a change in arousal is positive or aversive depends to a large extent on the magnitude of the change in relation to the existing arousal level. Larger and larger departures from the base level have increasingly positive effects until a peak is reached. The curve then drops and further increases eventually become aversive. This curvilinear relationship also seems to hold for the interaction of arousal and incentives. Individuals prefer the difficult instead of the easy, and novel instead of routine tasks under conditions of moderate, rather than high or low arousal.

10. By what mechanisms is an organism's behavior sustained or stopped? Even a cursory reading of the motivation literature shows that to a great extent theorists have been concerned with explaining behavior over a relatively short run of responses. The Skinnerian model, as well as most reinforcement theories, is strangely silent on the matter. By implication, reinforcement theories tend to fall back on some notion of satiation or homeostasis which tends to limit them to reinforcers operating on tissue needs, which in turn severely limits their applicability to behavior in organizations. Premack (1965) offers a non-tissue hypothesis when he suggests that behavior will stop in a given situation when the probability of the reinforced response begins to approach the probability of the reinforcing response. Helson's (1959) notion of adaptation level suggests that for every stimulus (physiological, social, or whatever) there is a region or level of adaptation (AL) at which the organism will no longer respond. The adaptation level is a function of three components: (a) the current stimulus magnitude, (b) the context in which it occurs, including the magnitude of stimuli recently presented, and (c) some parameter associated with the individual. Thus, other things being equal, continued presentation of the same reinforcing stimulus will even-

tually raise AL and render the reinforcer ineffective. Few data are available to suggest what this function might look like in an organizational situation. Thus the general experimental literature is not much help to use in explaining how behavior is motivated over a long period of time.

**SUMMARY COMMENT.** Infrahuman though it is, we think there are a few provocative suggestions that can be drawn from the experimental literature. For example, the concept of drive has not proven useful in explaining experimental results and the preponderance of opinion is that it should be discarded. Bolles (1967) feels reinforcement can do it all. Berlyne (1967) opts for an interaction of arousal, as he defines it, and reinforcement. A possible moral for organizational psychologists is that we should stop talking about need theories and start putting together lists of reinforcements while at the same time trying to get some handle on the assessment of arousal. The power of variable ratio reinforcement and the impotence of punishment in laboratory settings also begs for consideration in the organizational situation. However, lest we get too enamored with a Skinnerian view of reinforcement, the literature reviewed by Bindra (1969) and Black (1969) reminds us that it would be worthwhile to worry about the incentive properties of reinforcement. It might also pay dividends to explore the adaptation level phenomenon with regard to rewards and incentives under the control of the organization.

Unfortunately, the experimental literature provides few guides as to how to identify reinforcers and how to define a response. Skinner (1971) passes over these problems much too glibly. Perhaps the most unfortunate by-product of infrahuman research is that the nature of the species precluded intensive investigations of cognitive type models, even though Black (1969) would argue that a concept such as "assumed reward value" is useful for explaining nonhuman behavior. Thus a large

chunk of the theoretical domain for which these were ample historical antecedents went virtually unexplored, until recently. It is to these recent developments in cognitive models that we now turn.

### Cognitive Process Theories

Although we cannot verify the state of affairs for infrahuman species, we can be reasonably certain that women and men have thoughts about things that have happened to them, have expectations about what might happen in the future if they follow a particular course of action, and will most likely state their intentions if asked what they intend to do vis-à-vis some goal. In short, people think; and motivation theories which take account of this fact can be labeled cognitive theories. Most of these models are of relatively recent origin and they are being developed in the context of organizational, or at least adult, task behavior.

Most of the current cognitive theories owe their immediate ancestry to Lewin (1938) and Tolman (1932). Both held that individuals have cognitive expectancies concerning the outcomes that are likely to occur as a result of what they do, and they have preferences among outcomes. That is, an individual has an idea about the possible consequences of his or her acts and conscious choices are made among consequences according to the perceived probability of their occurrence and their perceived value to the individual. Tolman and Lewin's theoretical notions gave rise to at least three important models which have relevance for organizational behavior: (a) a model of human decision making (e.g., Edwards, 1961), (b) the theory of need achievement, and (c) Vroom's expectancy-valence theory of work motivation.

Another body of cognitive theory, but stemming from somewhat different origins, is that having to do with cognitive consistency or cognitive valence as an internal state toward which individuals strive. The

dominant model in this category is Festinger's (1957) theory of cognitive dissonance. The organizational manifestation of this theoretical theme is in the form of equity theory (Adams, 1963a) which will be discussed later in this chapter.

Since Vroom's expectancy-valence model soon became, rightly or wrongly, the dominant motivational theory in organizational psychology (Lawler, 1973), we would like to discuss briefly its basic parameters and their subsequent modifications. More detailed treatments of these topics can be found in Lawler (1973) and Mitchell (1974). Following a brief look at the expectancy-valence theory, we will examine the other major theoretical forces in terms of how they compliment or contradict the expectancy-valence model.

### Vroom's Theory of Work Motivation

The Vroom model attempts to predict (a) choices among tasks or (b) choices among effort levels within tasks. In brief, he sees the force on a person to choose a particular task or effort level as a function of two variables: the valence, or perceived value of outcomes stemming from the action, and the expectancy, or belief, that the behavior will result in attaining these outcomes. Thus, the Vroom formulation in its simplest terms is  $\text{Force} = (\text{Expectancy that effort results in attaining outcomes}) \times (\text{the valence of the outcomes})$ . The complete model is a bit more complicated than this and contains three basic constructs: valence, instrumentality, and expectancy.

*Valence* refers to the perceived positive or negative value ascribed by the individual to the possible outcomes of action on the job. In the case of the effort model there are really two kinds of outcomes. The first is simply the level of performance achieved. Different levels can take on different valences. The second type are those outcomes which might be contingent on performance. These can include pay, promotion, transfer, peer acceptance, working irregular hours,

recognition, supportive supervision, etc. These outcomes are also ascribed a valence.

*Instrumentality* refers to the perceived contingency that one outcome has for another. That is, given any "index" outcome the model implies we should ask questions about the extent to which possessing the index outcome is instrumental for obtaining other outcomes. One very important index outcome is the level of performance an individual achieves. For example, high performance by an hourly paid carpenter on a construction job will probably not result in a pay raise. Thus, the instrumentality of high performance for pay is low. However, the instrumentality of high performance for supervisory recognition may be quite high. Instrumentality is seen by Vroom as a correlation coefficient varying from +1.0 through 0 to -1.0. That is, high performance could mean avoidance of certain outcomes as well as their attainment.

Valence and instrumentality combine to determine the valence of a given performance level. More specifically, the valence of a given performance level is obtained by multiplying the instrumentality of that performance level for obtaining each outcome times the valence of that outcome, and these products are summed over all outcomes.

Recall that the basic Vroom model was given above as  $\text{Force} = \text{Expectancy} \times \text{Valence}$ . The last term of this equation has now been specified. *Expectancy* refers to the perceived relationship between a given degree of effort expenditure and a given level of performance. Its metric value is viewed as a perceived probability varying between .00 and 1.00. An example of a low effort-performance expectancy situation would be an assembly line where the number of units produced is determined largely by the speed of the line. In this situation the probability is very low that increased effort would result in producing more units.

At this point we have specified the model's three basic ingredients. The *Force* on a person to choose to expend a given level of effort is a function of these three

variables and is determined by the sum of the products of expectancy  $\times$  valence, or, as Vroom puts it:

The force on a person to exert a given amount of effort in performance of his job is a monotonically increasing function of the algebraic sum of the products of the valences of different levels of performance and his expectancies that this amount of effort will be followed by their attainment. (p. 284)

Finally, the level of effort a person will choose to expend is that for which force is a maximum.

Please note that Vroom's theory is stated in terms of expectations and perceptions of *future* consequences. The individual's previous reinforcement history plays no role. Neither does the concept of drive or need; however, Vroom notes that if the perceived valences of a number of outcomes are highly correlated and the individual seems to have a high or low preference for the entire class of outcomes, we could think of the class as representing a "need." But in no sense is the concept of need basic to the theory. What the model says we need to know are: (a) the value an individual anticipates for each outcome in an exhaustive list of outcomes, (b) the degree to which each outcome is perceived as being contingent on various levels of performance, and (c) the perceived probability that the individual can attain each of those levels of performance.

We would now like to discuss a number of elaborations and modifications of Vroom's original statement. Again, the reason for this approach is that the expectancy-valence formulation is currently the dominant theme in work motivation.

### Graen

The extension proposed by Graen (1969) is an attempt to broaden the conceptual base of the expectancy-valence model by incorporating theoretical notions from attitude theory, role theory, and the interpersonal in-



fluence process. The resulting increase in the number of parameters that must be considered may seem to give his model considerable additional complexity; however, his purpose is to make explicit at the outset variables that must be faced sooner or later anyway. These can be grouped under two major headings.

First, the task of a motivational model is not seen as merely to explain effort or choice as they are directed toward greater performance on some specific task, but to consider the full spectrum of job behavior in a system of multiple employment roles. Some examples of different roles are effective performer, friendly co-worker, leader, manager, and dependable employee. The set of roles which best describe the domain of organizational behavior is an empirical question. However, after making an argument for the importance of the multiple role orientation, Graen concludes that the roles of effective versus standard performer are the central roles in his model. A crucial point is that for any particular role there are a set of implicit or explicit standards which indicate whether an individual has or has not met the role expectations.

The second major modification is to consider all the possible outcomes of meeting or not meeting the standards for specific work roles and break them into three major classes. First, there is a class of intrinsic, or internally generated, consequences that individuals grant or do not grant to themselves as a result of meeting the standards for a particular role. Externally mediated outcomes are in turn broken down into the second and third sub-categories: One has to do with pressures to comply with the role expectations that emanate from some person(s) in power (e.g., the supervisor); the second, although not explicitly stated, has to do with role outcomes that are specified by the organization or the culture as being attached to that role. For example, meeting the requirements for "executive" may mean a high salary, neighborhood respect, but perhaps your undergraduate son's disdain.

Being a friendly co-worker may lead to interesting conversations and/or wasting time.

These three classes of outcomes are combined in a multiplicative way with the instrumentality of a particular role for achieving them to yield the overall attraction of a particular role for the individual. The effort an individual exerts to achieve the role (e.g., none, "standard amount," "superior amount") is then a function of the attraction of the role and the expectancy that a particular amount of effort will lead to meeting the role expectations. A more formalized, but abridged, schematic representation of the Graen model is given in Figure 1 for the two seminal roles of effective versus standard performer.

Basically, Graen is attempting to predict the probability of superior effort expenditure. As the bottom of Figure 1 indicates, the Probability of Superior Effort equals Path-Goal Utility plus External Pressures toward Superior Effort plus Internal Pressures toward Superior Effort. The first component, Path-Goal Utility, is similar to Force in the Vroom model. It is composed of what Graen terms Goal Attraction and Path Efficacy. Goal Attraction is similar to Vroom's Valence of Performance. It is the sum of the products of the valence of outcomes multiplied by the instrumentality of a given performance level for attaining these outcomes. The other component of Path-Goal Utility is Path Efficacy which is the perceived degree of relationship between a given effort level and attaining a given performance level, similar to Vroom's Expectancy.

The second component is External Pressures toward a given effort level. This component is composed of the individual's perceptions of effort levels others expect him to exert (a kind of valence index) multiplied by the amount of pressure these other persons would apply to influence his compliance (a kind of instrumentality).

Internal Pressures toward a given effort level constitutes the third component. It consists of the individual's preferences (va-

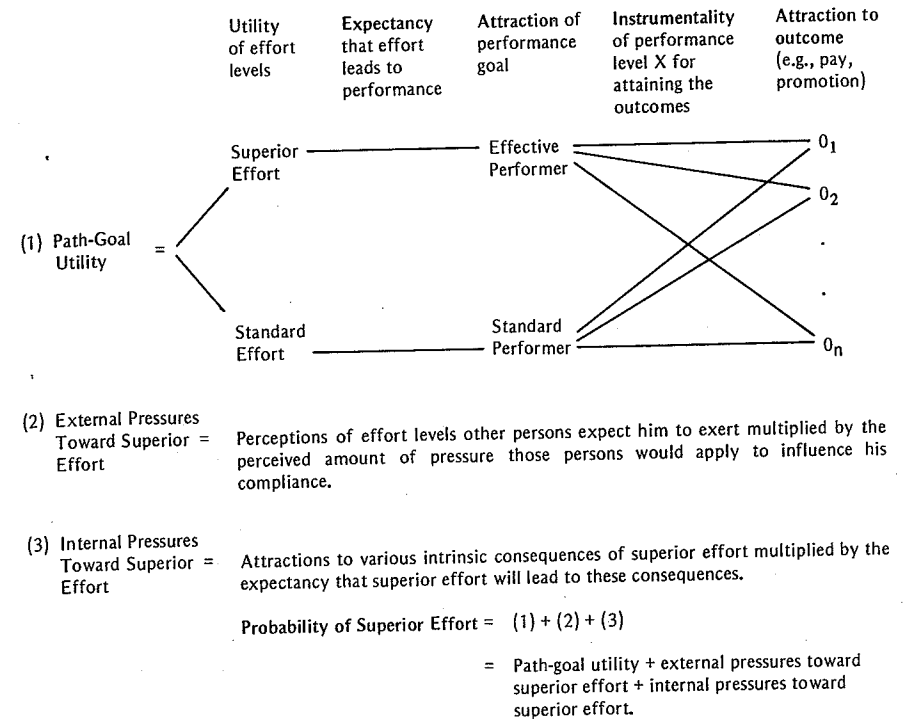


Figure 1. Schematic representation of simplified Graen model.

lences) for various intrinsic consequences of a given effort level multiplied by the expectancy that that effort level will result in these consequences. The total amount of internal pressures toward a given effort level is the sum of these products.

Graen postulates that the three components of this model—Path-Goal Utility, External Pressures, and Internal Pressures—combine additively to produce the overall probability of superior effort. Further, each of the three major components is weighted via a multiple regression beta weights. This implies that research is needed to determine the relative contribution of each to the weighted linear sum.

While this is the basic idea of the Graen model, the actual operationalization of the model quickly becomes more complex. This complexity results from Graen considering

the probability of superior effort, his ultimate criterion of motivation, as a function of the resolution of pressures toward or against superior effort applied through path-goal utility, external pressures, and internal pressures. He speaks of utility of superior effort versus the utility of standard effort; the goal attraction of the role of effective performer versus the goal attraction of not attaining the role of effective performer; external pressures toward superior effort versus external pressures against superior effort, etc.

Much of the real complexity of the Graen model, as well as the Vroom model, stems from using discrete effort levels such as high versus low, superior versus standard. Both models deal with the anticipated value of each of the effort levels to the individual. But how many effort levels are



needed? Graen talks about two, Vroom doesn't really say. The use of discrete effort levels makes operationalizing either of the models difficult. Even if one were to arbitrarily specify two effort levels as all that need be considered, someone interested in testing the Vroom model is forced to measure expectancies for *each* effort level and performance level combination. The problem is larger for the Graen model when external pressures and internal pressures, each composed of two sets of values, must be measured twice.

### Porter and Lawler

The model proposed by Porter and Lawler (1968) attempts to avoid the problem of dealing with discrete effort levels and differs somewhat from the Vroom and Graen models. It is the least complex of any of the expectancy-valence models we have so far discussed and the basic variables in their model are as follows:

Value of rewards is similar to the corresponding concept in the other models in that it refers to the perceived attractiveness of possible extrinsic and intrinsic outcomes to the individual.

The dependent variable to be explained is individual effort as it is directed toward performance. The usual distinction is made between effort and performance (i.e., performance has a number of other determinants besides effort and these are not the concern of the model). Performance refers to the sum total of successful role achievement and is not limited to productivity or physical output.

As the result of performing, an individual receives certain rewards, either from the organization or from himself. Thus Porter and Lawler make a distinction between intrinsic rewards, such as feeling of accomplishment that the individual grants to himself as a result of his performance and extrinsic rewards that are granted by other people, such as pay, recognition, or an opportunity to attend a T-group. Reward is

virtually synonymous with the term outcome as used by Vroom and Graen. One of the major determinants of effort is the individual's perceived *value of reward* (i.e., valence).

A second major determinant is *effort-reward probability* or the perceived contingency between effort expenditure and obtaining certain specific rewards. This overall relationship is composed of two component relationships. One has to do with the perceived contingency between effort and performance. Porter and Lawler state it in terms of the perceived probability that performance depends on effort. The second component is the perceived contingency between performance and obtaining rewards, or as they state it, the probability that rewards depend upon performance. They are not explicit about the nature of the function between effort and performance and between performance and reward. Does the individual carry a correlation around in his head, which implies a continuous function, or does he estimate probabilities only in terms of specific performance goals and/or specific effort levels (à la Vroom and Graen)? In subsequent research using their model, Porter and Lawler used questionnaire items asking the subject to estimate the overall relationship between effort and reward, which implies they are thinking of continuous functions.

In sum, the amount of effort an individual will expend toward performing is hypothesized to be a multiplicative function of the perceived value of rewards and the perceived contingency between expending effort and obtaining rewards, which in turn is a multiplicative function of the two components described above.

The model also incorporates two feedback notions. One straightforward hypothesis is that over time the perceived effort-reward contingency will change as a result of the actual reward practices that are followed by the organization (extrinsic) and the individual (intrinsic). This is merely saying that reinforcement history affects

cognitions. The second feedback loop involves the effect of felt satisfaction with a reward on subsequent anticipated value or satisfaction. Felt satisfaction results from the degree to which an individual perceives the rewards he receives to be equitable. Equity or inequity is derived by comparing the level of rewards actually received with the level an individual feels he should receive for a particular level of performance or for occupying a particular organizational role. The implication is that rewards can vary from fair to deficient. That is, Porter and Lawler do not deal with the consequences of over-reward. In any event, the question immediately arises as to whether satisfaction with a reward increases or decreases its perceived value in the future. Porter and Lawler enter a plea (legitimately so) of "no data" and surmise that it may have either effect, depending on the reward involved. At the extremes it is reasonable to hypothesize that satisfaction with food may decrease its perceived value as a reward but satisfaction with a feeling of accomplishment may increase the value of such a reward. Again it's a distinction between a "homeostatic" view and a "growth" model. It is unfortunate that motivation theory, especially in the human domain, has been so silent on the dynamics of this situation. Porter and Lawler at least acknowledge the issue.

### Lawler Modifications

The basic Porter and Lawler portrayal of expectancy theory subsequently has been modified, or rather elaborated, somewhat by Lawler (1971, 1973). The modifications are also discussed in Lawler and Suttle (1973). Basically, the elaborations entail a more formal specification of the parameters that determine an individual's expectancy that effort will lead to task accomplishment and an inclusion of a third feedback loop.

The individual's subjective probability that effort will lead to goal accomplishment is seen as being determined by (a) the task

information specific to the particular stimulus situation under consideration, (b) the individual's fund of information concerning how he or she has done on similar tasks in the past, and (c) the individual's self-esteem, or a relatively permanent characteristic of the individual's personality that reflects the generalized perception of competence across almost all task situations. Lawler does not specify how these components might combine to determine the expectancy judgment or whether "sub" expectancies defined by these various components might relate differentially to behavior.

The additional feedback loop concerns the effect of task success or failure on the individual's general self-esteem and on the specific expectancies which become characteristic of specific kinds of tasks. Lawler does not speculate further on what some of the dynamics of these feedback loops might be. For example, one might expect generalized self-esteem to change more slowly as the result of feedback than the specific expectancies of task success which are attached to specific tasks.

### A Composite Picture of VIE Theory

Expectancy-instrumentality-valence theory (VIE) represents the dominant theme in motivational explanations of human behavior in organizations and we have just reviewed three major versions. They differ somewhat in the terminology they use, the variables they deem as important, and the kinds of relationships they postulate. The distinctions are not great but they are sufficient to require a very careful reading before the similarities and differences are apparent. It would be desirable if we had some composite picture which combined the contributions of Vroom, Graen, and Porter and Lawler, seriously contradicted none of them, and provided a framework with which to integrate additional theoretical notions and research data. What follows is meant to be such a composite.

Again, our justification of trying to com-

bine the above ideas and present yet another picture is that we do believe that an expectancy-valence model adds clarity to our thinking about motivation, even though it very quickly becomes operationally complex. Human beings have thoughts and intentions which influence their behavior. Expectancy-valence theory takes account of these phenomena, reinforcement and drive theory do not. Also, we will see that such a model can profitably incorporate a number of additional theoretical notions, as well as a lot of research data.

The schematic shown in Figure 2 is meant to point out the relevant variables and their juxtaposition. In our opinion, a precise statement of how they interact is still beyond the power of our science and we will come back to that problem later. However, knowing the precise nature of the interactions, perhaps it is not necessary for the model to be useful in guiding research and practice. We will proceed sequentially through the diagram and try to indicate where the same variable or relationship was present or absent in previous models.

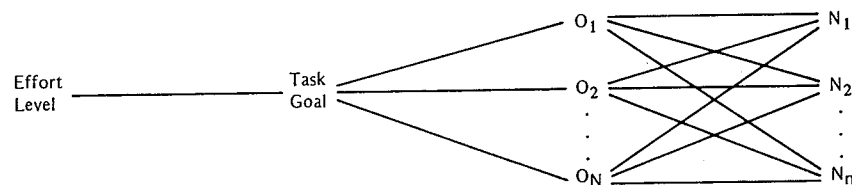
### Basic Components

The *dependent variable* to be explained is either (a) the *choice* which is made

among alternatives, (b) the amount of *effort* which is directed toward some goal, or (c) the *change* in effort or choice which results from what actually happens to the individual over time. Vroom's Force and Graen's probability of an act are consistent with all three while Porter and Lawler's discussion of effort as a dependent variable makes no mention of choice behavior. For purposes of brevity, most of what follows deals with amount of effort as a dependent variable. Again, as was stated earlier in this chapter, performance is not the dependent variable the model is trying to explain, since performance is a function of many other antecedents besides the "motivational" ones.

Choice or effort is directed toward something. For our own purposes, we wish to talk about choices directed toward alternative tasks and effort directed at performance levels within tasks, and we would like to refer to a specific performance level for a specific task as a *task goal*. Porter and Lawler talk only of effort directed at total performance, Vroom's Force to act is directed at choices among tasks or choices among specific performance levels. For Graen, the probability of an action governs a choice between performance levels within a particular organizational role. Our use of the term task is not meant to conflict with the term role. First, a role is simply a very

Force to expend specific level of effort	Expectancy that specific level of effort will/will not accomplish task	Valence of task goal accomplishment/failure	Instrumentality of task accomplishment/failure for job outcomes	Valence of job outcomes	Instrumentality of job outcomes for need satisfaction	Valence of "basic" needs
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a For purposes of simplicity this schematic portrays only one level of effort and one level of success on one task goal. A similar set of relationships exists for alternative levels of effort and alternative tasks or alternative levels of success.

Figure 2. Composite expectancy-valence model<sup>12</sup>.

general kind of task. Obviously, task goals incorporate several parameters that might have important implications for behavior. For example, tasks vary in terms of their *content*, or simply the kinds of behaviors that are required to perform them. Second, another related parameter would be the *dimensionality* (e.g., unifactor versus multifactor) of the task's content. In most job situations Graen's "effective" performer goal would be multidimensional. Third, given a certain content structure, tasks vary in terms of their level of difficulty. Fourth, task goals can vary in terms of their relative clarity or ambiguity. At one extreme both the methods to be used to accomplish the task and the criteria for judging when the goal is met may be readily apparent to all observers. At the other end, there may be very little agreement. Fifth, task goals can vary in terms of who determines them. That is, who specifies their content and difficulty? Is it the organization, the experimenter, or some other external agent, or is it the individual? Thus, it becomes relevant to ask whether task goals are *externally* or *internally* mediated, and sometimes it may not be so obvious. The supervisor or experimenter may think the individual is behaving in accordance with an externally formulated goal when in fact the individual has redefined the situation quite differently and has a quite different goal in mind. All this is a somewhat roundabout way of saying that task content, difficulty, goal clarity, and locus of goal definition are important parameters of tasks to consider when using an expectancy type model.

At this point, we should also keep in mind the distinction between behaviors, or actions, and the outcomes produced by the behaviors or actions. As Lawler (1973, p. 46) notes, this is not always an easy distinction to make when dealing with expectancy theory. Parenthetically, we should also note that it is not an easy distinction to make when evaluating any kind of performance, and it has plagued the "criterion" problem for decades (Campbell, Dunnette, Lawler,

& Weick, 1970). In the present context the observable behaviors taken in pursuit of a goal are actions, in the Vroom sense, while the *decision* as to whether the actions, or products produced by the actions, meet the criteria for goal accomplishment is an outcome.

As the result of achieving or failing to achieve a particular task goal, certain other outcomes may result. That is, a number of other outcomes may be directly contingent on achieving or not achieving the goal (performance) outcome. This array is composed of outcomes which are the direct result of task performance. Following Graen's discussion of internal versus external pressures and Porter and Lawler's intrinsic versus extrinsic rewards, we think it makes sense to talk about two major classes of goal contingent outcomes. *External* outcomes are those provided by the organization or other people (e.g., recognition, money, promotion, harassment, or freedom from harassment) and *internal* outcomes are those mediated within the individual and which the individual grants to himself or herself (e.g., a feeling of having used abilities to the fullest or disgust at having to do something considered reprehensible). Making this distinction in a specific organizational context does not argue against the notion that internal outcomes originally came to be established via external mediators. That is, an individual's previous learning history produced the association of feelings of disgust with certain kinds of tasks. The distinction is really meant to differentiate between those outcomes which are under the direct control of the individual and those which are not.

Although not included in any previous model, we would like to distinguish between outcomes which are directly contingent on task accomplishment and another class of outcomes which are at least one step removed from the direct consequences of task performance. These more "distant" outcomes have to do with the satisfaction or somewhat more basic individual needs. For example, a salary increase (first level or performance

contingent outcome) might be used to finance the purchase of a new home (second level outcome). To the extent that a number of such outcomes are highly interrelated we might say that their principal component represents an underlying *need*. Vroom speaks of needs in much the same manner but he does not distinguish between first and second level outcomes. Within his framework, needs are seen simply as clusters of interrelated outcomes. Admittedly, adding this third class of outcomes to the picture introduces more complexity and it may indeed be empirically superfluous since using an expectancy model does not require these outcomes to be identified. However, what must be identified are the outcomes which are directly contingent on performance and to mix up these two kinds of outcomes in a measuring instrument would cloud the use of the model.

As it is in all expectancy models, a basic determinant of action is the *valence* of outcomes, that is, their perceived or anticipated value to the individual. The valence of an outcome may vary from extremely positive to extremely negative (aversive).

The outcomes contingent on task accomplishment acquire their valence through their ability to satisfy the more basic needs people have. Dependent on the need in question, positive valence may accrue from maximizing need satisfaction, minimizing it, or moving it toward some optimum level. It's sticky business and reminiscent of the arguments over the reinforcing properties of drive. The weight of the experimental infrahuman literature says the need concept is not required, but as we shall see, the organizational literature persists in talking empirically and theoretically about specific needs. The valence for the directly contingent outcomes is the same concept as Vroom's valence of outcomes, Graen's attraction of role outcomes, and Porter and Lawler's value of rewards.

As pointed out by Campbell, Dunnette, Lawler, and Weick (1970), there is difficulty regarding the association between ac-

quiring performance contingent outcomes and need satisfaction. As the magnitude or amount of the outcome increases, the needs on which it operates may change. For example, Cofer (1967) speaks of the sign versus consummatory value of money in that at lower pay levels a wage increase represents more of life's necessities, but at higher levels it is *symbolic* of greater achievement, more status, etc. Thus the function may be discontinuous at a number of points.

Following Vroom and Graen, it also makes sense to posit a *valence for performance* or task accomplishment itself which is in turn a function of the valence of the goal contingent outcomes and the *instrumentality* of performance (i.e., task success or failure) for obtaining these outcomes. For example, salary increases might be positively contingent on performance, unrelated to performance, or even negatively contingent (e.g., the boss is threatened by high performers and rewards them less).

Different types of metrics have been assumed for the instrumentality variable. Some people think of it as a correlation (e.g., Vroom) that varies from +1.0 to -1.0. Others think of it in terms of a subjective probability statement that varies from 0.0 to 1.0. Regardless of what metric is used, the instrumentality measure is fundamentally a conditional probability. That is, the model says to ask people to estimate the degree to which an outcome is dependent on performance, given that the performance goal is achieved or given that it is not achieved. For example, given that a project is finished satisfactorily, what are the chances that a bonus will be received? Or given that an individual meets the standards for "satisfactory performer," what are the chances that he or she will not be fired? It is also true that any correlation coefficient can be represented as an array of conditional probabilities. Thus the distinction between the operational uses of the two is still whether we want people to make the transformation from conditional probabilities to a correlation in their heads or whether the measure-

ment method used to assess instrumentality should make the probability judgments explicit. Asking for conditional probability estimates requires asking a larger number of questions, but it does allow use of something other than the bivariate normal distribution to represent the relationship between the level of performance which is reached and the amount of the outcome that is received.

The remaining link in the picture is an individual's expectancy that the task in question can be accomplished; that is, the subjective probability or degree of belief that the task goal(s) can be met. Again, there are two general ways we could query someone about their expectancies, regardless of the specific methodology that is used. First, the individual could simply be asked the question, "What is the probability that you can meet this task goal?" Second, the judgment could again be made a conditional one and the individual could be asked, "What is the probability that you can meet this task goal, if you expend X amount of effort?" As was the case with instrumentality, the first question can be transformed to the second if a representative sample of effort levels is chosen.

How should the issue of a conditional versus nonconditional mode of estimating expectancy and instrumentality be resolved? As we noted before, if estimates of expectancy and instrumentality must be obtained for several levels of effort and several levels of task accomplishment then using the model becomes quite expensive. Nonconditional estimates are cheaper but the distinction between linearity and nonlinearity is lost.

In general, if we wish to use the model to interpret what's going on in a field setting or make predictions about the *general* level of effort in a particular situation then overall estimates of the relationship between effort and performance and between performance and outcomes seem appropriate, given that we satisfy the conditions which make an overall estimate (i.e., the correlation coefficient) meaningful. However, if

we are motivation theorists interested in testing various aspects of the model itself then it is more informative to deal with effort as directed toward specific task goals or performance levels. For example, what level of expectancy generates the most effort? On its face, the model says the higher the expectancy the greater the effort, but perhaps that's not so. We could ask the same thing about instrumentality. The usual expectancy model says one thing, Skinner says another. To test such questions, a linear estimate of instrumentality will not do; we need to portray the distribution. In this situation we aren't required to deal with a large number of goal or performance levels but can choose those which suit our experimental purposes.

### Feedback Loops

Although they are not shown in Figure 2, recall that Porter and Lawler were the only previous expectancy theorists to include feedback loops in their model. Satisfaction, or the degree of perceived equity generated by the obtained rewards, most surely influences the perceived value of rewards in the future; but the direction of influence most likely depends on the kind of need for which the reward has instrumentality. For example, for needs such as achievement, the more rewards that are piled on the *more* the individual may want. It's unfortunate that this kind of feedback relationship has received so little empirical or conceptual attention. Besides a feedback loop for valence of outcomes, similar loops should be considered for expectancy and instrumentality. For example, what is the effect on expectancy of continued success or failure? What are the long term effects of a .50 probability for receiving certain valued outcomes? Answers to these questions are fundamental to our understanding of behavior over significant intervals of organizational life. They also take the expectancy theory out of its ahistorical context, which has both advantages and disadvantages.

### Summary of Composite Picture

In summary, within the organization itself, effort is a function of three determinants: (a) the expectancy that effort will lead to task accomplishment, (b) the instrumentality of task accomplishment for obtaining or avoiding task contingent outcomes, and (c) the valence of the outcomes. The joint function of instrumentality and task contingent outcome valences yields a valence for a specific standard of performance. Effort is then a joint function of expectancy and the valence of performance. The corresponding functions are:

$$\text{Effort} = f(\text{Expectancy} \times \text{Valence of Performance})$$

or

$$\text{Effort} = f(\text{Expectancy} \times \text{Instrumentality} \times \text{Valence of Task Contingent Outcomes})$$

While most expectancy theorists subscribe to a multiplicative function for combining the effects of these three major determinants, we feel uneasy about such a formulation and would prefer to leave the question open.

Certain predictions flow from the above picture, most of them relatively obvious, but worth listing, if only to call them into question.

1. The greater the individual's expectancy that effort will accomplish task goals, the greater the effort expended, other things being equal. Under certain conditions, other models make opposing predictions and we shall review some conflicting data.

2. The greater the instrumentality, or the perceived probability that reward is contingent on performance, the greater the effort expended, other things being equal. However, on their face the laboratory data on reinforcement schedules do not support this assertion and any garden variety Skinnerian would opt for a probability considerably less than 1.0.

3. The greater the valence of a performance contingent outcome, the greater the

effort expended, other things being equal. This prediction is general enough that there are few conceptual or empirical conflicts with it. It is well established in the laboratory at least that increasing the size of the reward increases the magnitude of the response. For the organizational situation there are problems like how to combine internal versus external outcomes when they have similar instrumentalities, but opposite valence (e.g., you get a lot of money for performing tasks that really disgust you). However, this seems to be a measurement problem rather than a theoretical one.

4. If expectancy, instrumentality, or valence is zero then effort in the direction of performance is zero. This makes reasonable sense and is one of the strongest arguments for a multiplicative rather than an additive model.

5. The model also predicts that if we think of job satisfaction as the extent to which important needs are satisfied by rewards, then satisfaction is a resultant of performance (that leads to rewards), but not vice versa. There will exist a correlation between performance and satisfaction if and only if the relevant instrumentalities are not zero.

### Research Evidence Pertaining to Expectancy-Instrumentality-Valence Theory

Before going on to consider the content side of organizational motivation theory and the more specialized models that supplement or contradict the general expectancy model at various points, we would like to consider briefly the present state of empirical research relative to expectancy-valence theory. This is in no sense meant to be a comprehensive literature review, but is intended to give, in rather Spartan fashion, a representative sample of the prevailing empirical winds and the methods by which the theory has been operationalized. More detailed treatments can be found in Vroom (1964, 1965), Campbell, Dunnette, Lawler, and Weick

(1970), Lawler (1971, 1973), Heneman and Schwab (1972), Dachler and Mobley (1973), Mitchell and Biglan (1971), House and Wahba (1972), Miner and Dachler (1973), and Mitchell (1974).

### Kinds of Studies

Before discussing specific findings we would like to outline two basic parameters that we think are important for distinguishing among studies. The first has to do with the research setting in which the study was done and whether it was experimental or correlational in nature. A cross-classification of setting and methodology yields five principal varieties.

1. A few investigators have conducted laboratory experiments using students as subjects (e.g., Arvey, 1972; Motowidlo, Loehr, & Dunnette, 1972; Pritchard & DeLeo, 1973). Keep in mind that VIE theory deals with perceptions as determinants of behavior and even though expectancy, instrumentality, valence, or some combination thereof might be manipulated experimentally, the model says that the subject's perceptions of the manipulated situation must then be obtained and related to behavior. Not all experimental studies purporting to "test" VIE theory have done that.

2. There have been at least two attempts to manipulate VIE components experimentally in simulated organizations (e.g., Graen, 1969; Jorgenson, Dunnette, & Pritchard, 1973). Both of these set up temporary "employer overload" type organizations and "hired" subjects to work on a very short term basis. Such studies preserve the controls necessary for a true experiment but also add a great deal of realism to the task content.

3. There has been only one study (Pritchard, DeLeo, & VonBergen, 1974) to date that has approached the nature of a true field experiment in which VIE variables were experimentally manipulated in an ongoing organizational setting.

4. Perhaps the bulk of the studies per-

taining to the VIE model are correlational field studies in which existing perceptions of expectancy, instrumentality, and valence are assessed via a questionnaire instrument and the resulting scores and combinations of scores are correlated with the dependent variable (e.g., Arvey & Neil, 1972; Dachler & Mobley, 1973; Gavin, 1970; Hackman & Porter, 1968; Lawler, 1968a; Lawler & Porter, 1967; Mitchell & Albright, 1972; Mitchell & Nebeker, 1973; Schuster, Clark, & Rogers, 1971).

5. A subset of the above would be a correlational field study which attempts to measure the independent and dependent variables at two points in time and then employs a panel design or path analysis in an attempt to gain some insights into the causal relationship. The Lawler and Suttle (1973) study is an example of such a design.

The second major parameter pertains to the way in which the dependent variable has been operationalized. Consider the following list:

1. A few studies have used occupational or job preference as a dependent variable (e.g., Holmstrom & Beach, 1973; Mitchell & Knudsen, 1973; Sheard, 1970; Vroom, 1966; Wanous, 1972). However, this is analogous to using the valence of specific task goal or performance level as a dependent variable and only one study (Sheridan, Richards, & Slocum, 1974) has attempted to predict an individual's actual choice of a job.

2. A number of studies have used "rated" effort as a dependent variable (e.g., Hackman & Porter, 1968; Lawler & Porter, 1967; Mitchell & Albright, 1972; Mitchell & Nebeker, 1973; Pritchard & Sanders, 1973; Schuster, Clark, & Rogers, 1971). Almost all of these studies have been carried out in a field setting, but there is a major distinction between studies that have employed self-ratings of effort versus those that have used supervisor or peer ratings.

3. Several experimental laboratory or simulation studies used measures of task performance (e.g., Arvey, 1972; Cartledge, 1972; Graen, 1969; Jorgenson, Dunnette, &

Pritchard, 1973; Motowidlo, Loehr, & Dunnette, 1972). Strictly speaking, the expectancy model does not try to explain performance, since there are many other determinants of performance besides motivational ones. However, for the most part, the laboratory and simulation studies have chosen very simple and repetitious tasks for which the explicit or implicit assumption has been that individual differences in performance should reflect primarily effort differences and not differences in skill, aptitude, or task understanding. Everyone should have more than enough skill to perform well. For example, Graen (1969) and Jorgenson et al. (1973) had subjects search for simple errors in computer printouts, and Arvey (1972) and Motowidlo et al. (1972) asked subjects to add two digit numbers. The field study reported by Dachler and Mobley (1973) also focused on simple production tasks, but for real jobs in two different manufacturing operations.

4. Finally, other studies have used the VIE model to predict performance or job satisfaction in relatively complex jobs. Again, these dependent variables are outside the confines of the VIE model, but a number of investigators have used them anyway.

### *Valence and Behavior*

While a few data do exist pertaining to the relation between valence and effort and between valence and performance, the results are quite mixed. In several managerial samples Porter and Lawler (1968) found in general that the greater the importance attributed to pay, the higher were the individual's performance evaluations and ratings of effort. In a study by Pritchard and Sanders (1973), 148 employees in a government agency who were learning a new job completed a questionnaire that included measures of valence of job outcomes and performance-outcome instrumentalities. The criteria consisted of self and supervisors' ratings of effort for different aspects of the training program. The sum of the valence

ratings correlated .54 with self-reported effort and .21 with supervisors' ratings of effort.

There are also some negative findings. Hackman and Porter (1968), in a survey of telephone operators found that the median correlation between measures of performance and valence of outcomes was only .16. However, when valence was multiplied by instrumentality to get an overall prediction, significant correlations with performance were obtained even though correlations of instrumentality with performance were also low. Further negative evidence comes from an experimental study by Jorgenson, Dunnette, and Pritchard (1973). Although the design was complex, the essential elements consisted of hiring subjects for what they felt was a real job and paying them on a high performance-outcome instrumentality pay system (a semi-piece rate) for six days. The valence of outcomes portion of the model would predict that for subjects on such a pay system, there should be positive correlation between the valence of pay and performance. However, this was not the case. Correlations between rated importance of pay and job performance ranged from  $-.18$  to  $.15$  for the six days, with a median of  $.05$ .

The predominant impression generated by these data is that no definite conclusions can be drawn regarding the predictive utility of the valence of outcomes portion of the model. However, three conditions must hold for the valence of outcomes to be related to effort or performance. Performance-outcome instrumentalities must be greater than zero, effort-performance expectancy must be greater than zero, and there must be some variability in the valence of outcomes.

Low correlations between valence of outcomes and effort or performance cannot in themselves be taken as negative evidence for the influence of valence of outcomes, if the research effort was not specifically designed to control for these factors. It is also apparent that methods used to measure valence have been of the crudest sort. In spite of the fact that the measurement of the utility of an outcome to an individual has consistently

been shown to be one of the most difficult problems in the investigation of human choice behavior (e.g., Becker & McClintock, 1967), organizational psychologists have exhibited little sense of urgency about the matter. The difficulty is illustrated by asking simply, "What is the utility to an individual of a \$500 salary increase?" The analogous question for other outcomes presents even more problems. Assuming they can put them all on a common dimension, asking subjects to rate the importance or desirability of various outcomes produces a weak ordering at best, yet the model requires a utility scale with substantial interval or perhaps even ratio properties. All of which argues that we should spend more time developing better measures of valence.

It would seem unwise to conclude that valences don't affect behavior since the literature in experimental psychology is quite clear in showing the significant influence of reward magnitude on subsequent behavior.

### *Performance/Outcome Contingency (Instrumentality)*

The literature on this component of the model is fairly extensive and offers consistent support for the effects of the component on effort and performance. The crucial question concerns the form of the relationship.

The classic study by Georgopoulos, Mahoney, and Jones (1957) surveyed 621 production employees on an incentive system in a unionized household appliance factory. Via questionnaire they measured both the instrumentality of high and low performance for the attainment of the three outcomes of making more money in the long run, getting along well with the work group, and promotion to a higher salary rate. The results indicated that subjects who reported high instrumentalities tended to be higher producers.

A number of other correlational studies have supported the link between instrumentality and behavior. Lawler and Porter

(1967) used a three-item composite measure of instrumentality which included two items dealing with performance-outcome instrumentality and one item dealing with effort-outcome instrumentality and found low positive relationships between this composite instrumentality measure and ratings of effort (median  $r = .18$ ), but little relationship between the instrumentality measure and ratings of performance (median  $r = .11$ ). In a more extensive study (Porter & Lawler, 1968), using a similar instrumentality measure, they found instrumentality generally related to ratings of performance, and even more strongly related to ratings of effort. Using the Lawler and Porter questionnaire with male and female managerial candidates, Gavin (1970) found that instrumentality was positively and significantly related to supervisors' ratings of performance for both males and females (median  $r = .27$ ). Schuster, Clark, and Rogers (1971) also used the Lawler and Porter measures in a survey of professional workers. They found that subjects who saw work quality and productivity as very important determinants of pay were rated as higher performers. Spitzer (1964) obtained uncross-validated multiple correlations of .40-.50 when the perceived instrumentalities for nine outcomes were used to predict five different productivity criteria. However, there were only ninety-six subjects (production foremen) and the lack of cross-validation is critical. Wofford (1971) found a correlation of .43 between the performance-outcome instrumentality averaged across outcomes and supervisory ratings of performance. However, the carefully done field study by Dachler and Mobley (1973) could find only small correlations between average instrumentality and performance, even when the research site was one using an incentive pay system.

In addition to these correlational studies, several experimental investigation studies of performance-outcome instrumentality have been reported. Jorgenson, Dunnette, and Pritchard (1973) manipulated performance-outcome instrumentality by paying em-

employees in a temporary organization created for purposes of the experiment on either an hourly basis (low instrumentality) or a type of piece rate (high instrumentality). After individuals had worked for three four-hour days under their respective pay systems, each group switched to the other system and worked for three more days. The data indicated that people under the high instrumentality pay system performed higher than those under the low instrumentality pay system for the first three days. Furthermore, immediately following the shift in pay systems, and for all three subsequent days, the performance of subjects who were shifted to the high instrumentality system was higher than their own performance under the low instrumentality system and higher than the performance of those subjects who were shifted to the low instrumentality system. Results similar to Jorgenson et al. were obtained previously by Graen (1969) who also hired subjects to work in a temporary organization. In Graen's study the subjects were females hired to find errors in batches of computer output. Changes in outcomes produced changes in performance only if outcomes were contingent on performance. Similar results in a laboratory simulation study were obtained by Pritchard and DeLeo (1973) who compared an hourly and incentive pay system.

An experimental study by Arvey (1972) manipulated performance-outcome instrumentality by giving subjects who worked on an arithmetic task differing chances to "win" extra subject participation points for their introductory psychology course. In the high instrumentality condition, subjects who were high performers had a .75 probability of getting the extra points while in the low instrumentality condition subjects had a .25 probability. The results did *not* support the hypothesis since there was no difference in performance between the two levels of performance-outcome instrumentality.

One unfortunate characteristic of almost all the experimental studies that have manipulated the performance-outcome instrumen-

tality is that only two levels of instrumentality have been used. A good Skinnerian should be furious, since with only two data points we cannot distinguish a linear from a nonlinear relationship between performance and instrumentality, and it very well may be the latter.

In a laboratory study where female undergraduates were first paid an hourly wage for scoring test answer sheets and then were switched to an incentive condition, Yukl, Wexley, and Seymore (1972) were able to show that performance was higher when subjects were paid 25¢ per sheet on the basis of a coin flip (i.e., expected value of payoff probability = .50) than when they were paid 25¢ for every sheet completed. On the surface this appears to argue that effort is not maximized when instrumentality = 1.0.

Another source of data on performance-outcome instrumentality comes from the literature on the effects of incentive pay, since an incentive pay system is one where the instrumentality of performance for a financial outcome should be perceived as high.

Several reviews of this literature were made some time ago (Marriott, 1957; Vitelles, 1953) and the general conclusion seems to be that incentive plans tend to increase performance for non-salaried personnel but "may not realize their full potential in increasing performance because of the 'rate restriction' phenomenon..." (Campbell et al., 1970, p. 366). These results are very much in line with the model since increases in performance-outcome instrumentality should result in increases in performance but high instrumentalities for peer sanctions and for lowered piece rate wage would tend to decrease effort and performance.

#### *Effort/Performance Contingency (Expectancy)*

While there is not a great deal of literature on the perceived degree of relationship between effort and performance, the bulk

of the evidence seems to support it as a useful component. Schuster, Clark, and Rogers (1971) compared the performance ratings of those subjects who were higher in perceived effort-performance expectancy to the performance of subjects lower in effort-performance expectancy. While the difference was not statistically significant, it was in the predicted direction. In the experimental study by Arvey (1972) described above, subjects working on the arithmetic task were told they were in competition with other members of their group and that only a certain proportion of them would be designated as "top performers." Expectancy was manipulated by varying the proportion of subjects who would be designated as top performers from one fifth (low expectancy), to one half (medium expectancy), to three-quarters (high expectancy). The results of the study offered support for the expectancy model in that subjects in the low expectancy condition performed lower than subjects in the high expectancy condition. This finding was supported in a similar study reported by Motowidlo, Loehr, and Dunnette (1972).

Somewhat less direct evidence comes from studies which do not explicitly measure effort-performance expectancy, but rather measure the perceived degree of relationship between effort and outcomes. Such a measure combines effort-performance expectancy and performance-outcome instrumentality. All the studies using such a measure (Hackman & Porter, 1968; Lawler & Porter, 1967; Porter & Lawler, 1968) reported positive relationships between this expectancy-instrumentality composite and measures of performance, but the confounding of these two variables does not permit any direct assessment of the effects of perceived effort-performance expectancy.

Rather negative evidence comes from the study described above of government workers learning a new task (Pritchard & Sanders, 1973). Measuring effort-performance expectancy by questionnaire, it was found that expectancy correlated .14 with self-reports of effort, and this correlation

dropped to .02 when expectancy was compared with supervisory ratings of effort.

In sum, while the effort-performance expectancy variable shows consistently significant results (at least in a statistical sense), there is still considerable inconsistency in how this variable should be conceptualized and measured. While the question is most often posed in terms of asking the subject for a subjective probability estimate, the object of the question (i.e., what's meant by performance) varies considerably.

#### *Multiplicative Composites of VIE Components*

The literature we have reviewed so far seems to indicate that each component separately shows at least some moderate relationship to effort or performance, but another question is whether their combination increases the level of prediction over and above what each one does separately.

Considering first the combination of the valence of outcomes and the performance-outcome instrumentality, Lawler and Porter (1967) found in their sample of managers from five different organizations that predictions of performance and effort using instrumentality alone resulted in correlations from .17 to .32 with a median of .23. When outcome valences were multiplied by instrumentalities the correlations ranged from .18 to .44 with a median of .29. Correlational studies by Spitzer (1964) and Evans (1970) also showed only a small increment in correlations as a result of multiplying performance-outcome instrumentalities by the importance or desirability of outcomes. Similar, but generally stronger, differences were obtained by Hackman and Porter (1968), Porter and Lawler (1968), Galbraith and Cummings (1967), and Mitchell and Pollard (1973). In contrast, two other studies (Gavin, 1970; Pritchard & Sanders, 1973) found no increase in the accuracy of prediction when the two elements were combined. However, the Gavin (1970) study found a correlation of .91 between



measures of instrumentality and measures of valence and thus it would be highly unlikely for any combination of the two elements to increase prediction over each used alone. This correlation raises the issue of whether instrumentality and valence are independent events. The multiplicative VIE model implicitly assumes they are independent, but it also seems reasonable that they are not. Moreover, it is not unreasonable that for some people there is a positive relationship (i.e., the higher the instrumentality the higher the anticipated value of the reward) and for others the correlation may be negative.

There have been a number of studies which have combined all three major components in an attempt to predict individual effort, or performance on repetitive tasks. One of the most elaborate studies to date is reported by Dachler and Mobley (1973). It was a correlational field study carried out in each of two research sites using semiskilled operatives as subjects. Both sites were manufacturing operations but individuals were on an hourly pay plan in one plant and an incentive system in the other. The researchers took considerable care to identify as many of the relevant outcomes in the two situations as they could. Through interviews and questionnaires they eventually assembled a list of forty-five relevant outcomes. Rather than asking subjects to estimate correlations between effort and performance and between performance and outcomes they broke performance and valence into specific levels and asked the subjects to estimate the appropriate conditional properties. Their overall finding was that the multiplicative combination of expectancies, instrumentalities, and valences was the single best predictor of performance, but the correlation was only significant for the plant which used the incentive system; that is, where at least some outcomes were directly contingent on performance level. The correlation of .30 is in line with the previously cited research. The most important single component in the Dachler and Mobley study was

the expectancy variable and combining expectancy with the valence of performance levels, which in turn was a function of outcome valences and the instrumentality of that performance level, did not increase the correlations much. This finding is also consistent with previous findings.

An exceptionally thorough correlational field study utilizing a panel design is reported by Lawler and Suttle (1973). The subjects were sixty-nine retail managers each of whom was measured twice. Half the sample was remeasured after a six-month interval and the remainder after a twelve-month interval, which unfortunately resulted in a rather small *N* for the cross-lagged correlations. The questionnaire focused on eighteen outcomes and obtained data on their valences, the rated instrumentality of "good job performance" for each of the eighteen outcomes, the rated expectancy that "working hard" would lead to each of the eighteen outcomes, and the expectancy that "working hard" would lead to "good job performance." The dependent variables consisted of peer, superior, and self-ratings of effort and performance, and an objective performance measure consisting of sales data for the manager's department adjusted to correct for certain obvious biases. In an attempt to account for additional determinants of performance, data were also obtained on the subjects' role perceptions. In addition, the verbal, quantitative, and total scores from the *Thurstone Test of Mental Alertness* were available from company files. The analysis was thorough and consisted of calculating the static and cross-lagged correlations (for both six- and twelve-month intervals) of every VIE component, and combination of components, with each dependent variable. For our current purposes, the correlations with effort ratings are the crucial ones. For the static analysis, the correlations of the VIE components and component combinations range from approximately .30-.40 for self-rated effort, from .20-.30 for superior ratings of effort, and from .10-.20 for peer ratings of effort,

and there is little advantage to one combination over the other. Weighting instrumentalities and/or expectancies by valence did not increase the correlations. The correlations for the "full" model are .39, .27, and .15 respectively. The results of the cross-lagged correlations gave only slight support to a causal analysis.

A number of other studies (e.g., Arvey & Neil, 1972; Galbraith & Cummings, 1967; Mitchell & Albright, 1972; Mitchell & Nebeker, 1973; Pritchard & Sanders, 1973) also attempted to determine via correlational field studies the correlation between a multiplicative combination of the components in the complete VIE model and ratings of effort and performance. Although their instrumentation was generally less elaborate than either Dachler and Mobley or Lawler and Suttle the obtained results are quite consistent with these two studies.

#### Summary of Expectancy Theory Research

As of this date there have been approximately thirty-five published studies that have some relevance as "a test" of expectancy theory predictions. What summary statements can be made about the data themselves? We offer the following:

1. Almost all of the studies purporting to test the full model have been correlational field studies and the correlational "ceiling" seems to be approximately .30 when independent ratings of effort are used as the criterion. The mode seems to be closer to .25. Virtually the only time the *r*'s exceed this ceiling is when self-rated effort is used as a dependent variable. However, this introduces so much method variance into the correlation that interpretation of such a coefficient would be quite risky.

2. While a multiplicative combination of expectancy, instrumentality, and outcome valence typically yields a higher correlation than that for the individual components or simpler combinations of components, the differences are usually not very great. Expectancy or instrumentality usually accounts

for most of the variance that is to be accounted for and multiplying by valence seldom makes much difference. To date, it does not seem possible to choose sides between effort-performance expectancy and performance-outcome instrumentality as the more potent variable.

3. The results from the experimental studies do not seriously contradict the correlational investigations. That is, the variance in the dependent variable accounted for by the experimental treatment does not exceed .30<sup>2</sup>. One additional characteristic of the experimental studies is that significant interactions were not typically found (e.g., Arvey, 1972; Pritchard & DeLeo, 1973) which further supports the lack of advantage attributed to the multiplicative combinations. However, we should also point out that most of the experimental studies did not deal with *perceptions* of the VIE components as independent variables, as an expectancy theory says should be the case, but focused on the experimenters' manipulation. The Arvey (1972) and Motowidlo et al. (1972) experiments are exceptions, but their results do not change the overall conclusion.

4. In those experimental studies which used performance on a simple repetitive task as a dependent variable and which also obtained measures of ability, a brief aptitude or general intelligence test almost always accounted for much more variance in performance than did the motivational variables (Dunnette, 1972). However, keep in mind that this was not the case in the Lawler and Suttle (1973) correlational field study which used rated effort and performance on a managerial job as dependent variables.

5. The attempts to account for additional variance in performance by some multiplicative combination of motivational and ability variables have been singularly unsuccessful. However, the performance = ability  $\times$  motivation formulation is a muddled one at best and perhaps little else could have been expected.



6. There is a slight hint in the literature that performance-outcome relationships attached to internally mediated outcomes are more potent than those attached to externally mediated outcomes (e.g., Lawler & Suttle, 1973).

7. Although we did not review any of the evidence on this question, the available research comparing a multiplicative versus additive combination of the VIE components (e.g., Hackman & Porter, 1968; Porter & Lawler, 1968; Pritchard & Sanders, 1973) tends to show a slight advantage for the multiplicative formulation, but the differences are neither startling nor easy to interpret (Mitchell, 1974).

In sum, the available data do not portray the VIE model as a very powerful explainer of behavior. However, the above conclusion begs a number of questions and we would like now to turn to a discussion of various problems that plague both conceptual and research activity in this area. When all is said and done, we think the heuristic value of the expectancy framework will remain as a powerful force in organizational psychology even though its empirical house is certainly not in order.

### Difficulties with VIE Theory and Research

The expectancy point of view has not been without its critics and a number of conceptual, measurement, and inferential problems have been pointed out (e.g., see Behling & Starke, 1973; Heneman & Schwab, 1972; Mitchell, 1974; Schmidt, 1973; Wahba & House, in press). Perhaps it would be wise to briefly list the problems that confront anyone who wishes to use the full VIE model as an explanation of effort or choice behavior in organizations. In total, we think these problems constitute a strong indictment of the full multiplicative model, but we also think they point the way to more fruitful avenues of research on motivational issues.

1. One major problem is with the dependent variable itself. The model attempts

to predict choice or effort and most of the research activity has been directed at the latter. However, organizational psychology is without any clear specification of the meaning of effort and consequently there is no operationalization of the variable that possesses even a modicum of construct validity. The most frequently used measures are self, peer, or supervisor ratings of overall effort after some attempt has been made to distinguish between performance and effort in the instructions for the rater. Most often this consists of reminding the rater that the amount of energy, concentration, and perhaps time, that an individual puts into a task is not synonymous with the performance outcome that results and that the latter is also a product of skills, task understanding, and whatever constraints may be operating. On the basis of this reminder, it is hoped that the rater will use these two factors in some fashion approximating their arrangement in the "true" factor space, even though we, as yet, do not have even the beginnings of a theory that would suggest what such a factor space might look like. Obviously, or perhaps not so obviously, the problem can be better handled in a laboratory setting where the dependent variable can be "sanitized" in various ways. However, a careful experimental operationalization of effort may destroy the ecological validity of the variable for translation to field settings. It is in the measurement of effort in situ where we are really hurting and it would be well worth our while to start an in depth look at the meaning and measurement of just this variable.

Aside from the overall conceptual vacuum, or perhaps because of it, the use of self and superior ratings of effort are each beset by a number of problems. For example, the supervisor simply may not know how the individual spends time; and even if the individual were observed constantly, by what indicator does an individual signal a high effort or a low effort input? Hand in hand with some intelligent conceptual analyses as to what effort might mean, it would

also be worthwhile to employ some policy capturing techniques in an attempt to "recover" the indicators that lead various kinds of raters to judgments of high effort or low effort.

Self-ratings of effort present special problems since the same individual provides ratings of the independent variables (i.e., expectancy, valence, and instrumentality) and the dependent variable. The method variance door is wide open and the two measures are not experimentally independent. It's no wonder the "model" correlates higher with self-rated effort than with independent ratings. In our opinion, self-ratings of effort should not be used in motivation research until we know more about them.

Practically the only systematic investigation of effort measures in a field setting is a multi-trait-multimethod study by Williams and Seiler (1973) who obtained self and superior ratings of both effort and performance for a sample of engineers. Two measures of each were used, a global rating and a dimensionalized set of scales constructed via the method of scaled expectations. The correlations among these variables suggested that (a) effort ratings obtained from independent observers correlated hardly at all (i.e., .24 and .33), (b) performance ratings show more convergent and discriminate validity than effort ratings, (c) effort ratings don't show any discriminate validity at all, and (d) superior ratings of effort exhibit more halo than self-ratings. This study and previous literature suggest that self-ratings of effort and performance correlate about .40-.50 while superior ratings of the same two correlate about .55-.65. Obviously, there are many explanations for why these correlations are not 1.00 besides the fact that there are real differences between the two factors.

2. An allied problem concerns the methods that have been used to measure the independent variables. Most often these have been questionnaire items using summated ratings (i.e., Likert) response formats and almost no effort (*sic*) has been devoted to

testing (via some kind of process analysis or scaling technology) whether the subjects are using the variable the way the researcher has in mind. A few studies (e.g., Sheridan, Richards, & Slocum, 1974) have used paired comparison methods, which at least permit a transitivity test, but that's about as far as we have progressed.

Someone must get busy and try to find out what subjects are really doing when they generate "scores" on these variables. For example, data gathered by the decision theorists in gambling situations (Slovic & Lichtenstein, 1968) suggests that when subjective probabilities are compared to objective probabilities, people tend to underestimate the probability of almost certain events and overestimate the probability of rare events. Does the same thing happen in an employment setting?

3. Mitchell (1974) points out that Vroom's theory was originally designed to make *within* individual not *between* individual predictions. That is, the basic question is what task or effort level would an individual choose from among a range of alternatives? As Mitchell also points out, almost all the research designed to test VIE theory has used *between* individuals comparisons. We have already alluded to the trade-offs involved. If a study is meant to be a within subjects analysis, as the Dachler and Mobley (1973) study was in part, then estimates of expectancy and instrumentality must be obtained for several effort levels and several performance outcomes and the number of questions the subject must be asked quickly escalates. If a between subjects analysis is to be used, then the meaning of a variable must be the same across subjects. Serious response biases (e.g., tendency to use extremes) or differing underlying utility functions would confuse the between people comparisons and confuse the observed relationships.

4. Without citing chapter and verse (see Mitchell, 1974, for a partial review), the available data concerning the reliability with which VIE components are measured sug-

gests that while internal consistency estimates are reasonably high, any estimate obtained by measuring the variable at two different times is usually quite low (i.e., .30-.50). We do not wish to get into a long argument as to what kind of reliability should be demanded of VIE variables, but it seems to us that the theory requires the true score to be relatively stable across at least relatively short time periods. This is not to say that the true scores could not change drastically as the result of some new informational input. After all, accommodating such an event is one of the virtues of a cognitive theory. However, in an ongoing work setting which is relatively stable in character, we might expect the expectancy and instrumentality estimates to be relatively stable. In general, they have not been and this does present problems for the model. It suggests that the high internal consistency estimates may be partially the result of common method variance rather than common substantive variance.

5. Another issue concerns the precise nature of the predictions to be made. The major focus of the model is really on the *change* in the dependent variable as a function of changes in the independent variables. However, to test the model, researchers have relied primarily on relating predicted effort to ratings of effort and performance via static correlations.

6. The available studies have also relied primarily on subjects from a single organization, who were all doing the same job. It would seem quite possible that data collection from one job in one organization could result in a serious restriction in range in expectancies and instrumentalities.

7. Yet another issue concerns the specific first level outcomes for which valence measurements should be obtained. The VIE model is a process theory and it does not specify which outcomes are relevant for particular people in a particular situation. Such specifications are left to the ingenuity of the individual researcher. As a conse-

quence, negative results or unsupported predictions can almost always be explained on the grounds that all the relevant outcomes operating on the subjects were not included in the study. Mitchell (1974) argues that the problem of outcomes breaks down into three sub-questions. How many should be used? How specific should they be? What is their content? There are as yet no systematic answers to these questions.

8. Most versions of the full VIE model contain sums of cross-products between valences and expectancies and between valences and instrumentalities. Computing such cross-products makes several assumptions. First, for the multiplication to make sense, the two terms being multiplied must be independent. That is, the model assumes no interaction between valence and expectancy or between valence and instrumentality, or between instrumentality and expectancy. This may or may not be the case but it seems hardly likely that such zero interactions are always true. For example, as Atkinson (1965) suggests, the outcome of, "I will feel a high sense of achievement if I accomplish task X" may be dependent on the individual's estimate of his/her probability of success on task X. Also, a drastic increase in the instrumentality of performance for obtaining some outcome may influence its perceived value. For example, outcomes that have a very low contingency on performance may be devalued. Recall the previous study by Gavin (1970) which obtained a correlation of .91 between instrumentality and valence.

9. Strictly speaking, variables must also be measured on a ratio scale if the scores are subsequently to be multiplied together. In a pointed discussion of this matter, Schmidt (1973) shows that the correlations of sums of cross-products generated by VIE type operations with other variables can be changed drastically by transformations that would be invariant if the scales possessed ratio properties. This problem is an old one in psychology, and it remains to be seen

whether the use of a scaling technology that is consistent with the multiplicative requirements of the theory will appreciably change the results it generates. Schmidt suspects that it will and worries that using "weak" measures with a very demanding theory may be very misleading.

10. Most versions of the model also carry the assumption that outcome valences are additive in some sense. However, perhaps individuals at work do not really sum valences, but combine them in some other fashion, such as focusing on a dominant outcome under certain conditions and forgetting about the rest.

11. There is inherent in the model a general notion that the world is built in a linear or at least monotonic fashion. The higher the expectancy the greater the force, the greater the instrumentality the greater the force, and the greater the valence the greater the force. All these linearity assumptions are grounds for debate, and to the extent they do not mirror reality, the predictions of the model are weakened. Atkinson (1965) and reinforcement theorists (e.g., Bolles, 1967) would challenge such an assumption for expectancy and instrumentality respectively. Certain "need" theorists (e.g., Maslow, 1954), to be discussed in the next section, might also challenge the assumption as it pertains to valence. For example, the valence of pay might change as we go up the pay scale because the need outcomes for which it is instrumental change as a function of amount. The valence of a salary increase at a high salary level may be much higher or much lower than a salary increase at a lower pay level because some people might see it as instrumental for status or some other powerful need, while for others pay is instrumental for nothing besides food, clothing, and shelter.

12. Finally, in the tradition of Spearman, the research using expectancy models has tended to adopt a general factor plus specifics as its view of the "structure" of expectancy, instrumentality, and valence. That is,

instrumentality is a general factor that is made up of a number of specific instrumentalities, expectancy is a general factor made up of a number of specific expectancies, and so on. However, it may be the case, for example, that the components of an individual's expectancy estimate attributable to general self-esteem versus that attributable to specific task characteristics may relate to behavior in different functional ways and some tasks may elicit more of the self-esteem component than others.

Some versions of the model do speak to a distinction between internally and externally mediated outcomes (rewards) and some studies have analyzed results separately for these two sub-general factors. However, what about positive versus negative outcomes (e.g., Reitz, 1971) and the nature of the instrumentalities attached to internal versus external or positive versus negative outcomes? Since internally mediated outcomes are under the control of the individual, perhaps the instrumentality of task accomplishment for obtaining these outcomes is nearly always 1.00.

### A Summary Comment

In general what message does this list of problems seem to convey? We think it says quite clearly that the VIE model is a simple appearing formulation that encompasses a highly complex and poorly understood set of variables and variable dynamics. Rather than strive for large scale studies that provide a complete test of the "full" model with superficial measures of poorly understood variables, we think researchers could better spend their time studying the individual components in depth. For example, a host of questions surround the expectancy variable. We shall talk about a few of these later on. We think it would be far better to ask what is expectancy and how does it relate to well defined variables than to ask what is the correlation between  $E \cdot \Sigma(V \times I)$  and a global rating of effort.

## SUBSTANTIVE, OR CONTENT THEORIES

The models described below attempt to suggest the specific identity of variables discussed in general terms by the process models outlined earlier. As such, the theorizing is mainly of a taxonomic sort, although considerations of dynamic relationships are not completely absent.

### Need Theory

Much of the history of this class of theory is rooted in theories of instincts which, as we have noted, fell into disrepute soon after the turn of the century because of the propensity to postulate a specific need for almost every human act. Relative to human behavior, instincts were again made respectable when they were transformed to the concept of needs acquired through learning. Needs were firmly rooted in contemporary motivational theory by Murray (1938).

Murray hypothesized the existence of a relatively large number of specific needs which human beings attempt to satisfy. The exact number in the list varies according to the particular stage in Murray's career, but about twenty basic needs were usually included. The following list and abbreviated definitions are abstracted from Hall and Lindzey's (1957) presentation of Murray's theory.

1. *Abasement*. To submit passively to external force. To accept injury, blame, criticism, punishment. To surrender. To become resigned to fate.

2. *Achievement*. To accomplish something difficult. To master, manipulate, or organize physical objects, human beings, or ideas. To do this as rapidly and as independently as possible.

3. *Affiliation*. To draw near and enjoyably cooperate or reciprocate with an allied other (an other who resembles the subject or who likes the subject).

4. *Aggression*. To overcome opposition forcefully. To fight.

5. *Autonomy*. To avoid or quit activities prescribed by domineering authorities. To be independent and free to act according to impulse.

6. *Counteraction*. To master or make up for a failure by re-striving. To overcome weaknesses, to repress fear.

7. *Defendance*. To defend the self against assault, criticism, and blame.

8. *Deference*. To admire and support a superior. To praise, honor, or eulogize.

9. *Dominance*. To influence or direct the behavior of others by suggestion, seduction, persuasion, or command.

10. *Exhibition*. To make an impression. To be seen and heard.

11. *Harmavoidance*. To avoid pain, physical injury, illness, and death.

12. *Infavoidance*. To quit embarrassing situations or to avoid conditions which may lead to belittlement: the scorn, derision, or indifference of others.

13. *Nurturance*. To give sympathy and gratify the needs of a helpless object.

14. *Order*. To achieve cleanliness, arrangement, organization, balance, neatness, tidiness, and precision.

15. *Play*. To act for "fun" without further purpose.

16. *Rejection*. To separate oneself from a negatively cathected object.

17. *Sentience*. To seek and enjoy sensuous impressions.

18. *Sex*. To form and enjoy sensuous impressions.

19. *Succorance*. To have one's needs gratified by the sympathetic aid of an allied object.

20. *Understanding*. To ask or answer general questions. To be interested in theory.

Murray's list and his accompanying definitions were not based on empirical research. Rather, they represented his conceptualization of what internal states govern human behavior and were generated from his clinical experience and observation. Notice also that almost every "need" appearing in twentieth century literature of organizational psychology appears on this list.

### Maslow's Hierarchy

As everyone knows, Maslow (1954) postulated a *hierarchy* of human needs incorporating several levels. Basic to Maslow's theory is the notion that needs at a particular level of the hierarchy must be "largely" satiated before the needs at the next higher level become operative. This is not to say that two levels cannot be operative at the same time, but the needs at the lower level take precedence. It follows that if lower level needs are substantially satisfied in our society, they may never actually be very important for energizing and directing behavior.

The basic outline of Maslow's hierarchy from the lowest level to the highest level is as follows:

1. *Physiological needs*. As one might expect, these include such things as hunger and thirst. Maslow also goes on to talk about a number of specific hungers.

2. *Safety needs*. These refer primarily to freedom from bodily threat and in our culture are probably most active for young children.

3. *Belongingness or social needs*. These include the need for friendship, affection, love, and perhaps something akin to affiliation as that term is used by Murray and McClelland.

4. *Esteem needs*. These represent an individual's need for self-respect, for the respect of others, and for a stable, positive evaluation of himself.

5. *Self-actualization*. At the top of the hierarchy is the need level most existential in nature and most difficult to define. A succinct definition is simply that an individual's need to self-actualize is the need to be what one wants to be, to achieve fulfillment of one's life goals, and to realize the potential of one's personality.

Self-actualization is similar to the construct of need achievement except that *n* Ach is a more normative concept and is intended to have a similar meaning across a wide range of people, situations, and cul-

tures. Self-actualization must be defined individually. As noted by Lawler (1973) many different personality theorists have given a self-actualization type variable a prominent place in their theories. If the consensus of "experts" is any guide then outcomes which are instrumental for self-actualization are a pervasive influence in the lives of most people.

As was true of Murray's theory, Maslow's characterization of human needs is not based on any empirical foundation, but was derived primarily from Maslow's clinical experience.

### Alderfer's ERG Model

Alderfer (1969, 1972) has attempted to reformulate the Maslow hierarchy into a more meaningful set of three basic needs labeled *existence* needs, *relatedness* needs, and *growth* needs.

Existence needs consist of desires for material substances that are in finite supply. That is, one person's gain is another person's loss. Food, shelter, and money would fall in this category. Luckily there is a way out of the zero sum dilemma. Existence needs are not insatiable and individuals have a certain degree of satisfaction they regard as "enough" and in time of plenty no one need be a loser.

Relatedness needs have as their object the mutual sharing of thoughts and feelings with other people. That is, the theory says that people desire to tell other people their thoughts and feelings and to have the other person(s) reciprocate. That is not a zero sum situation but one in which the satisfactions of the parties are highly correlated. Notice also that no positive or negative connotations are given to the exchange. The open and accurate nature of the communication is what's important, not the pleasantness or unpleasantness involved.

The definition of growth needs is as slippery as ever and Alderfer presents no major conceptual breakthrough. Such needs are said to involve the interaction of the indi-

vidual with the environment in such a way as to develop whatever abilities and capacities the individual feels are most important for him or her. Thus, satisfying growth needs involves a fairly fundamental change in individual capacities.

Although one important thrust of the theory is the taxonomic one, the theory speaks to certain processes. However, it is not intended to explain dependent variables such as the direction or amplitude of behavioral responses. Rather, the process side of the theory concerns the dynamics of two subjective states labeled *satisfaction/frustration* and *desire*.

Alderfer (1972) lists a number of propositions dealing with interrelationships between the desire for the objects incorporated by a certain need and the satisfaction/frustration with those objects. In general, these propositions suggest that: (a) the less a need is satisfied the more it is desired; (b) the less a "higher order" need is satisfied the more lower order needs are desired; and (c) the more a need is satisfied the more higher order needs are desired. The term higher order is not used in the Maslow sense but refers to the level of concreteness in the need objects. Existence needs simply have more concrete referents than relatedness needs and relatedness need objects are less ambiguous than growth need objects.

A rationale for (a) above is older than psychology. The explanation for (b) is not quite so self-evident and is based on the notion that if one type of need desire is frustrated, the individual will seek to satisfy desires with more concrete referents. The progression up the hierarchy, as in (c), occurs because satisfaction of existence or relatedness desires frees the individual from the effort required to satisfy and he or she can then turn to relatedness or growth.

Thus, contrary to Maslow's notion of prepotency, the need is always there and consciously recognized. It is the means to pursue it that is at issue here.

### Data Relevant for Need Theories

There are perhaps three major kinds of available data that have been generated by content theories of needs.

1. Descriptive and correlational studies pertaining to which needs are strongest for which people.

2. "Tests" of the hierarchical or prepotency elements of need structures.

3. Tests of the taxonomic adequacy of a proposed model using multivariate psychometric methods.

We do not wish to discuss the available findings in detail but would like to highlight the basic issues.

### Correlates of Need Satisfaction

Although not a lot of data exist in this category, Vroom (1965) and Campbell et al. (1970) review studies pertaining to the relationship between job performance, job duties, organizational level, and need satisfaction. The now classic study in this area is the national survey by Porter (1964) of 2,000 managers drawn at random from the membership of the American Management Association (AMA). A thirteen-item questionnaire based on Maslow's need hierarchy asked the respondent to assess the manifestation of each need relative to (a) how much there is now, (b) how much should there be, and (c) how important is it.

In general, higher level managers placed greater emphasis on self-actualization and autonomy needs, but there were no differences across levels for the other three need categories. In terms of specific item content, the higher level managers rated as more important the opportunity for personal growth and development, the opportunity for independent thought and action, the opportunity for participation in the setting of goals, and the authority connected with their management position. Using the same questionnaire, similar small but significant hierarchical differences have been found in a

military organization (Porter & Mitchell, 1967). It is interesting to note that within Porter's AMA sample, neither the size of the company nor the distinction between line and staff had any appreciable effect on the importance ratings for various needs.

### Tests of the Hierarchical or Prepotency Mechanism

There are at least three possible explanations for the Porter-type findings. First, some mechanism such as Maslow's prepotency notion could be operating and for lower level jobs at lower levels in the organization the lower order needs may not yet be satisfied. Second, the differential results could be due to individual self-selection. That is, people with certain kinds of needs wind up in certain kinds of jobs. Third, certain jobs might provide certain kinds of outcomes that both stimulate and satisfy a particular kind of need, and a hierarchical or prepotency mechanism is not necessarily involved.

A few studies have tried to test the prepotency notion empirically. Alderfer (1969) used questionnaires and interviews to measure both the importance and satisfaction of existence, relatedness, and growth in a sample of managers. The study was cross-sectional in nature and the crucial statistic is the correlation between the reported satisfaction of a particular need and the reported importance of the next higher need. This correlation should be positive while the correlation of satisfaction with importance reported for the *same* need should be negative. That is, as Maslow's model would predict, as a lower level need is satisfied the importance of that need decreases while the importance of the next higher need increases. The correlations did not support the prepotency predictions, but were in the opposite direction.

Hall and Nougaim (1968) examined the same kind of relationships in a longitudinal study. They were able to secure five yearly

follow-up interview protocols from each of fifty participants in A.T.&T.'s Management Progress Study. The interviews covered numerous aspects of both work and nonwork related experiences and a panel of judges was used to infer the satisfaction and potency of Maslow's needs. The correlation of satisfaction with potency for the same need and with potency for the next higher need confirmed the Alderfer findings and again disconfirmed the Maslow-type predictions. Further, changes in satisfactions over a year period were correlated with changes in potency of the next higher need and the coefficients were zero or negative rather than positive.

While the above studies cast doubt on the prepotency notion, they do not permit a choice between the self-selection explanation and the job itself explanation.

A longitudinal study by Festinger (1964) bears on this issue and points up the complexity of the relationships that are encountered when such a global variable as management level is used as a correlate of need satisfaction. Festinger and his associates interviewed 175 "promotable" managers and then followed them for approximately four years, at which time about half had been promoted and the entire sample was re-interviewed. In general, the motive and value orientations seemed to be unchanged by promotion. However, the varying nature of the promotions suggested an interactive effect. If it was not perceived by the individual as a dead end job and in fact raised his level of aspiration, then work achievement tended to take on more importance. However, if the promotion attenuated the individual's level of aspiration, then family and other outside considerations took on greater importance.

Further support for the self-selection hypotheses is provided in a study by Vroom (1966), which suggested that MBA students tend to select jobs that will complement their expressed need preferences. In sum, the major thrust of what little data there are

is that expressed motives do not seem to change a great deal as an individual rises in the organization. However, we should not forget that the data are scanty and the ability of certain jobs to "teach" people that self-actualization or growth needs are important has not really been closely examined.

### Tests of the Taxonomy

A number of studies have attempted to determine if questionnaire items written to reflect the importance of or satisfaction with various needs intercorrelate the way they should when administered to samples of jobholders. In general, the items generated by the Maslow classification have not been able to reproduce the expected factors or clusters with any degree of clarity (Herman & Hulin, 1973; Payne, 1970; Roberts, Walter, & Miles, 1971). In contrast, Alderfer (1972) has been able to write items on the basis of his ERG model that do apparently exhibit significant convergent and discriminant validity. In the same series of studies, a set of items designed to tap the Maslow categories did not exhibit as much psychometric clarity.

We should keep in mind that there could be a number of alternative explanations of the above results and the correctness or incorrectness of the underlying taxonomy is only one. Nevertheless, what data there are suggest that the Maslow hierarchy is not as powerful and robust a notion as some people assume.

### Content Models of Performance Outcomes (Rewards)

The second major class of content models is comprised of attempts to specify taxonomies of the job outcomes, or rewards, that are important for explaining job behavior.

Since the content of most multifaceted job satisfaction scales deals with outcomes that are the direct result of task (job) performance, the primary source of such taxonomies

is the job satisfaction measurement literature. Therefore, it seems reasonable to examine the available job satisfaction measures with an eye toward their adequacy as taxonomic models of job outcomes.

After looking at the available information it was surprising to realize that there have been relatively few comprehensive programs of research that have tried to define and sample systematically the overall domain of possible job outcomes; and that most of what is available has used some variant of the factor analytic procedure. One exception is the original study of satisfiers and dissatisfiers by Herzberg, Mausner, and Snyderman (1959).

### Herzberg's Two-Factor Theory

On the basis of an extensive review of the earlier literature on job attitudes (Herzberg, Mausner, Peterson, & Capwell, 1957) and their well-known study involving a series of interviews with engineers and accountants (Herzberg, Mausner, & Snyderman, 1959), Herzberg and his colleagues have postulated the existence of two classes of work motivators—extrinsic and intrinsic factors which are in effect a taxonomy of first level outcomes (rewards). They are listed below.

#### Extrinsic Factors

1. *Pay*, or salary increase
2. *Technical supervision*, or having a competent supervisor
3. The *human relations* quality of supervision
4. *Company policy and administration*
5. *Working conditions*, or physical surrounding
6. *Job security*

#### Intrinsic Factors

1. *Achievement*, or completing an important task successfully
2. *Recognition*, or being singled out for praise

3. *Responsibility* for one's own or other's work
4. *Advancement*, or changing status through promotion

The intrinsic factors are viewed as being derived from the individual's relation to the job itself. Alternative labels are job *content* factors or motivators. Extrinsic factors are rewards or sources of need satisfaction that stem from the organizational *context* and are thus somewhat divorced from the direct influence of the individual. In later versions of the theory they are called hygiene factors. Since the behavioral implications of the theory are not stated in terms of energizing, sustaining, or directing effort, but are concerned with changes in job satisfaction, we will have no further concern with that aspect of the two-factor theory. It is the taxonomic question that is of interest here.

Note that Herzberg's use of extrinsic and intrinsic rewards does *not* conform to our previous distinction between internal and external outcomes. For example, recognition could be granted by the individual to himself or more formally by someone else in the organization. Similarly, either the individual or someone else could label something he or she did as an "achievement." This confusion of the event and the agent mediating the event has the effect of reducing the clarity of the content analysis of the interview protocols (Schneider & Locke, 1971).

To its credit the Herzberg taxonomy was not produced by generating an item pool, casting the items in a summated ratings (Likert) format, administering the items to a sample, and factoring or clustering the resultant correlation matrix. Unfortunately, rather than going on to explore systematically the potential of the free response procedure, the same procedure has been repeated over and over. For taxonomic purposes, it would have been more fruitful to explore the effects of systematic changes in certain parameters, such as the nature of the episode to be described, the time frame

of the recollection, the mode of data collection (e.g., oral versus written), type of job, alternative procedures for the content analysis, etc. If this had been done, we would now have a much more complete picture of what people consider to be the important outcomes of work.

### Vroom's Summary of Factor Analytic Studies

Vroom (1964) reviewed much of the factor analytic work on measures of job satisfaction and outlined the following seven factors as seeming to be those which have appeared consistently across a number of studies.

1. Company policies and management
2. Promotional opportunities
3. The job content
4. Supervision
5. Financial rewards
6. Working conditions
7. Co-workers

Interestingly enough, most of the studies which Vroom reviewed used the Science Research Associates job satisfaction questionnaire (e.g., Baehr, 1954).

On the basis of much of this same literature, Smith, Kendall, and Hulin (1969) decided to pursue a job satisfaction measure comprised of four factors: supervision, the work itself, pay and promotions, and co-workers. Subsequent correlational analyses indicated that the pay and promotions factor should be split into two separate factors. The result was the five job satisfaction factors currently measured by the *Job Description Index* (Smith et al., 1969).

### ISR Survey of Working Conditions

If a factor analytic type procedure is used to determine the facets that people see as the "basic" outcomes of work, a distinction can be drawn between the individual's satisfaction with an outcome and the importance attributed to an outcome. We do not wish to jump into the question of whether

weighting an individual's satisfaction of an outcome with the rated importance of the outcome will yield a "better" overall measure of job satisfaction. That is again a matter for theories of job satisfaction, not motivation. Our interest here is in whether ratings of importance will yield a different factor structure than ratings of satisfaction.

One example of such a study using importance data is reported by Quinn and Cobb (1971). Importance ratings for a list of twenty-three outcomes included in the 1969/1970 *Survey of Working Conditions* administered by the Institute for Social Research (ISR) were factored. Data were collected from a national random sample of employed adults ( $N = 1,500$ ) and the resulting five factor solution is shown in Table 1.

Table 1 illustrates one of the obvious pitfalls of the empirical factor analytic procedure. What comes out must go in, and it would be possible to quibble with the content of the Table 1 list. For example, where are outcomes having to do with opportunities for promotion?

#### Minnesota Studies On Work Adjustment

Imbedded in another theory of job satisfaction (Dawis, Lofquist, & Weiss, 1968) is another taxonomy of outcomes which is the culmination of an attempt to be extremely thorough in the sampling of the population of job outcomes. A series of factor analyses across several different occupational groups produced twenty relatively homogeneous factors, each measured by five items, which constitute perhaps the longest list of outcomes generated by this method. The factor labels and the highest loading item in each factor are given below.

1. *Ability utilization*. The chance to do something that makes use of my abilities.
2. *Achievement*. The feeling of accomplishment I get from the job.
3. *Activity*. Being able to keep busy all the time.
4. *Advancement*. The chances for advancement on this job.

TABLE 1

#### FACTOR ANALYSIS OF IMPORTANT RATINGS FOR 25 JOB FACETS INCLUDED IN 1969/1970 ISR SURVEY OF WORKING CONDITIONS

Factor I: Resources	
I receive enough help and equipment to get the job done	
I have enough information to get the job done	
My responsibilities are clearly defined	
My supervisor is competent in doing his job	
Factor II: Financial Rewards	
The pay is good	
The job security is good	
My fringe benefits are good	
Factor III: Challenge	
The work is interesting	
I have enough authority to do my job	
I have an opportunity to develop my special abilities	
I can see the results of my work	
I am given a chance to do the things I do best	
I am given a lot of freedom to decide how I do my work	
The problems I am asked to solve are hard enough	
Factor IV: Relations With Co-Workers	
My co-workers are friendly and helpful	
I am given a lot of chances to make friends	
Factor V: Comfort	
I have enough time to get the job done	
The hours are good	
Travel to and from work is convenient	
Physical surroundings are pleasant	
I am free from conflicting demands that other people make of me	
I can forget about my personal problems	
I am not asked to do excessive amounts of work	

5. *Authority*. The chance to tell other people what to do.
6. *Company policies and practices*. The way company policies are put into practice.
7. *Compensation*. My pay and the amount of work I do.
8. *Co-workers*. The way my co-workers get along with each other.
9. *Creativity*. The chance to try my own methods of doing the job.
10. *Independence*. The chance to work alone on the job.

11. *Moral values*. Being able to do things that don't go against my conscience.

12. *Recognition*. The praise I get for doing a good job.

13. *Responsibility*. The freedom to use my own judgment.

14. *Security*. The way my job provides for steady employment.

15. *Social service*. The chance to do things for other people.

16. *Social status*. The chance to be "somebody" in the community.

17. *Supervision—human relations*. The way my boss handles his men.

18. *Supervision—technical*. The competence of my supervisor in making decisions.

19. *Variety*. The chance to do different things from time to time.

20. *Working conditions*. The working conditions.

Interestingly enough, second order factor analyses of these twenty scales tend to produce two higher order factors which look very much like Herzberg's intrinsic versus extrinsic dichotomy (Weiss, Dawis, England, & Lofquist, 1967). Regardless of the outcome of the arguments surrounding the specifics of the two-factor theory it is apparent that the notion of two higher order factors has considerable meaning, even though its precise nature has not been specified.

#### An Emerging Structure

In view of the large number of studies in which measures of job satisfaction have played an important part, it is surprising to find that so little attention has been devoted to its basic structure. The information that is available suggests that a hierarchical picture might fit the current data best. That is, the overall judgment about the "job" is made up of two sub-general factors corresponding roughly to the intrinsic versus extrinsic breakdown originally identified by Herzberg. Each of the two sub-general factors can be broken down further into more specific factors, such as those measured by

the Minnesota Satisfaction Questionnaire (MSQ).

Obviously, labeling such a structure begs several questions. One concerns whether the taxonomic structure should be pursued to more specific levels still. Certainly it should make sense to break the compensation or pay factor into the major components of pay. The same may or may not be true for other factors. For the purpose of providing a taxonomy of potential outcomes to use in motivational theories, we do not think that lists of outcomes, such as those provided by the MSQ, are specific enough. It is an empirical question whether more specific facets that still possess significant discriminability can be defined and measured, but we think it is a useful avenue to pursue.

The above point is related to the question of whether the job outcome domain has been representatively and comprehensively sampled as yet. Recall that by means of interviews with a sample of jobholders, Dachler and Mobley (1973) were able to generate a list of forty-five outcomes thought to be important for people on relatively simple production jobs. Perusal of their list suggests that the existing multifacet measures of job satisfaction may not be as comprehensive as we think they are. One disturbing element of this literature is the tendency for everyone to borrow everyone else's items. The content is thus rather self-perpetuating.

If a full definition and more complete sampling of the job outcome domain is actively pursued, then questions surrounding how to stratify the population of such outcomes become extremely important. Possible strata (i.e., facets, parameters, etc.) to consider are those having to do with the *job* (e.g., job level, job function, "prestige" rating, etc.), and those having to do with the *individual* (e.g., age, education, career expectations, etc.). A useful definition of facets or parameters with which to stratify the sample would greatly facilitate sampling operations. So far, such questions concerning the job outcome population have suffered from considerable benign neglect.



### Intrinsic/Extrinsic Interactions

So far, we have discussed intrinsic and extrinsic job outcomes as two major taxonomic classes. Additional questions could be asked concerning the differential effects of these two classes of rewards on behavior and whether they interact in some form other than simple summation. As already noted, the Herzberg two-factor theory is a theory about job satisfaction not job behavior. The processes spelled out by the expectancy model imply that the total effect of all rewards is derived by simple summation.

However, deCharms (1968) has postulated that intrinsic and extrinsic outcomes may interact in non-additive ways. For example, if intrinsic rewards are being obtained for an activity, the introduction of additional extrinsic rewards may lower the value of the overall package, rather than enhance it. Deci has tested these notions in a series of laboratory studies (Deci, 1971, 1972; Deci & Cascio, 1972) designed to determine the interaction between extrinsic rewards and the intrinsic rewards associated with "doing" the task. Typically, subjects were asked to work on interesting puzzle games for specific time periods and then were given rest periods during which they could do anything they liked. The experimental treatment had to do with whether or not external rewards (e.g., money, verbal reinforcement, punishment) were also provided and whether or not they were contingent on performance.

The general conclusion derived from this series of studies is that extrinsic rewards which are contingent on performance decrease the valence of the intrinsic rewards. This is inferred from the fact that extrinsically rewarded subjects tended to spend less time working on the puzzles during their free time than subjects who were not extrinsically rewarded. Unfortunately, as noted by Calder and Staw (in press) no actual performance data are reported and we do not know how the two kinds of rewards interacted to influence performance itself.

Deci's explanation as to why introducing an extrinsic reward decreases the valence of an intrinsic reward is that it "teaches" the subject that performance should be motivated by the extrinsic reward and when that reward isn't present, performance should cease. In effect, the organization is disagreeing with the subject that the task is interesting in its own right. However, Calder and Staw (in press) also point out that the perception of *why* the reward is being offered is important. For example, if a financial reward is perceived as a bonus for good work rather than as an inducement to keep people on the job, it may not have a deleterious effect on the valence of intrinsic outcomes.

In sum, the available research on the differential effects of intrinsic versus extrinsic rewards is still sparse and full of loopholes. However, it is a step in the right direction. We need to know much more about how the available job outcomes combine to influence behavior.

### SUPPLEMENTARY MODELS

Relative to adult individual behavior in organizations, it is no secret that there is more to motivation theory than the process and content models that we have just outlined. We will discuss briefly four additional theoretical notions: equity theory, the influence of goals and intentions, the theory of need achievement, and attribution processes. These four topics vary considerably in breadth and theoretical scope; however, all of them seem narrower in focus than the valence-instrumentality-expectancy model. Consequently, we will treat them as sub-theories which appear to either supplement or contradict the basic expectancy notions at various points.

#### Equity Theory

Equity theory is also a cognitive theory concerning individual perceptions. It is based on cognitive dissonance and social

comparison processes. It deals with exchange relationships and the fairness or equity of these exchange relationships. Of the models dealing with equity concepts (Homans, 1961; Jaques, 1961; Patchen, 1961) the formulation presented by Adams (1963a, 1965) is the most explicit and has stimulated by far the greatest amount of research. In his most complete statement, Adams (1965) considers (a) the nature of a person's inputs and outcomes in an exchange relationship, (b) the social comparison process that is used by the individual, (c) conditions leading to perceptions of equity or inequity, (d) possible effects of inequity, and (e) behavioral and cognitive actions a person may take to reduce feelings of inequity. The exchange process under consideration is that which takes place between the organization and the individual within the relevant organizational role.

Inputs include any and all factors (e.g., effort, education, age, beauty, etc.) perceived by a person as relevant for getting some return. In contrast, outcomes include any and all factors perceived to be returns on the individual's job investments. The net "value" of the exchange to the individual may then be expressed as a ratio of inputs to outcomes, in which the specific outcomes and inputs are each weighted according to their perceived importance.

Perceived equity or inequity results when a person compares his or her outcome/input ratio, either consciously or unconsciously, to what is perceived to be the ratio of another person or persons. This comparison object need not necessarily be any one individual; it may be an abstraction based on a broad class of others seen to be relevant for comparison purposes. Adams refers to this generalized comparison as "Other," and to the one who compares as "Person."

Perceived equity results when Person perceives the two ratios to be equal. This is true regardless of the absolute level of inputs and outcomes for either party. For example, equity is said to exist in a situation where

Person's inputs do not match his outcomes but Other is perceived to be in an identical situation. Perceived inequity arises whenever Person's ratio differs from Other's ratio.

Adams postulates that the consequence of inequity is an induced tension (drive?) with motivating properties impelling Person to reduce or eliminate this tension. Moreover, the magnitude of the tension should be proportional to the magnitude of the inequity. Thus, the strength of the behavioral tendency (effort, choice, etc.) toward reducing inequity is determined by the magnitude of the perceived difference between the two ratios.

Adams lists several things Person can do to reduce or avoid inequity. One or both of the two ratios can be changed by (a) cognitively distorting inputs or outcomes, (b) acting on Other to get him or her to change inputs or outcomes, (c) actually changing Person's own inputs or outcomes, (d) changing the comparison Other, or (e) leaving the particular exchange relationship. Adams notes, of course, that all these modes of inequity reduction are not equally available to Person either behaviorally or cognitively, and he offers tentative hypotheses about the most likely modes for Person to choose in seeking to reduce inequity. Adams suggests that Person will seek to maximize positive outcomes, will seek to minimize effortful or costly inputs, and will resist both behavioral and cognitive changes in those inputs and outcomes which are most central to his or her self-esteem or perceived self-concept. In addition, Person will be more resistant to altering cognitions about his or her own inputs and outcomes than to altering cognitions about the inputs and outcomes of Other. Leaving the field or retreating from the exchange relationship is viewed as a last resort, occurring only when inequity is great and other means of reducing it seem to be unavailable. Finally, if comparisons with a particular Other have stabilized over time, Person will be highly resistant to changing comparison persons.



### Relevant Data

Equity theory literature has dealt primarily with financial compensation as an outcome. The majority of studies have dealt with the effects of overpayment and underpayment on job performance and, to a lesser extent, on job satisfaction. Since the data have been reviewed extensively elsewhere (Goodman & Friedman, 1971; Lawler, 1968c; Opsahl & Dunnette, 1966; Pritchard, 1969), we will attempt only to summarize the highlights.

The major research hypotheses have been those directed at quantity and quality of performance as a function of over- and underpayment within both incentive and hourly pay systems. The job or task at hand is usually simple enough to permit the assumption that performance, defined as either quantity or quality of output, is largely a function of effort. Under these conditions, equity theory would make the following predictions, if other means of equity resolution are controlled:

- A. For the overpayment condition
  1. Under an hourly system either quantity or quality should increase depending on the "set" given the individual since either one would increase Person's inputs.
  2. Under an incentive system where the payoff is contingent on the quantity of items produced, quantity should stay the same or decrease. Producing more simply increases the financial outcome and increases inequity even more; however, quality should increase since this would allow an increase in inputs to redress the inequity.
- B. For the underpayment condition
  1. Under an hourly system quantity and quality should both decrease depending on which would reduce the individual's input the most.
  2. Under an incentive system quality should go down and quantity may

or may not increase depending on the cost versus benefit (input/output) ratio for each additional unit of production.

Predictions of equity theory concerning the effects of *underpayment* have consistently been supported. From studies by Clark (1958), Homans (1953), Lawler and O'Gara (1967), Andrews (1967), and Pritchard, Dunnette, and Jorgenson (1972) one can conclude that when discrepancies in outcome/input ratios exist, inequity is felt and thus inequity leads to behavioral attempts to balance outcome/input ratios. In the case of hourly payment, this inequity reduction takes the form of decreased productivity, and under piece rate payment, increases in productivity are accompanied by decreases in quality.

At first glance, the research on the effects of *overpayment* on productivity seem to support equity predictions. Studies by Arrowood (1961), Adams and Rosenbaum (1962), Adams (1963b), Adams and Jacobsen (1964), Andrews (1967), Friedman and Goodman (1967), Lawler, Koplin, Young, and Fadem (1968), Lawler (1968b), Moore (1968), Goodman and Friedman (1968, 1969), Weiner (1970), Wood and Lawler (1970), and Pritchard et al. (1972) have generally supported the prediction that hourly overpayment leads to increases in productivity and piece rate overpayment leads to decreases in quantity of production and increases in quality.

However, three problems have plagued efforts to study overpayment. First, as Lawler (1968c) and Pritchard (1969) have pointed out, it has been difficult to manipulate perceived inputs and outcomes without at the same time threatening Person's self-esteem. The typical overpayment manipulation has consisted of informing the subjects, usually in a rather gruff fashion, that they are not qualified to do the task and thus are to receive more money than they are worth. Such an initial contact with the "employer" may not only have induced conditions of

overpayment inequity in the subject, but also to threaten self-esteem. If these were indeed the feelings aroused by this type of manipulation, it seems quite plausible that the subjects would attempt to do an especially good job to convince both experimenter and themselves that they were not as poor as they were made out to be.

The second problem in the overpayment literature is that some studies appear to have induced a set toward increased quality. Subjects made to feel over paid by the "typical" method (attacking the subjects' qualifications) are told to "pay close attention," etc. Thus, as Opsahl and Dunnette (1966) point out, they may tend to concentrate on doing high quality work at the expense of quantity.

A third problem, noted by Lawler (1968c), is that subjects made to feel overpaid due to their poor qualifications may also believe they are in danger of being fired, and thus seek to do especially good work to assure their job security. Lawler points out that this could take the form of increased quantity under an hourly pay system, and increased quality under piece rate, since quantity and quality may be more salient under hourly and piece rate systems, respectively.

Probably the most serious of the three is the self-esteem problem and several studies have dealt with it directly. Andrews and Valenzi (1970) had eighty subjects role play an overpayment manipulation which threatened the "employees'" qualifications and express what their reaction would be in that situation. They found that none of their subjects responded to open-ended questions about how they would feel in the situation in terms of wage inequity. However, fifty-nine of their subjects responded in terms of self-image as a worker. Subjects were also asked to rank order several possible reactions to the "employment interview" in terms of how plausible each reaction would be. Wage inequity reactions were chosen as least plausible while self-image reactions

were chosen as most plausible. Studies by Evans and Molinari (1970) and Weiner (1970) also offer data supporting the contaminating effects of threats to qualifications in overpayment research. The latter study also demonstrated that the effect due to threats to self-esteem was greater when the task seemed to involve highly valued abilities than when it was portrayed as not involving skills that were central to the individual's self-concept.

If one considers only those studies which do not manipulate overpayment by threats to qualifications, a somewhat different picture emerges. The data produced by Andrews (1967), Lawler (1968b), Moore (1968), Weiner (1970), and Pritchard, Dunnette, and Jorgenson (1972) suggest that overpayment has a small effect in the predicted direction, but it frequently falls short of statistical significance.

One explanation for the asymmetry between over- and underpayment deals with the type of exchange relationship present in most organizational settings and in the experiments on overpayment. Exchange relationships can be ordered on the basis of the amount of psychological contact in the relationship (Pritchard, 1969). At one extreme is an intimate emotional relationship between two people. Less intimate would be two partners working in cooperation with each other; then two co-workers (friends) in an exchange relationship with a third party, their employer; and finally, a worker and an unknown generalized comparison group (e.g., lathe operators in general) who are both in exchange relationships with employers. An important characteristic of this intimate-impersonal continuum is that differences in inputs or in outcomes are much easier to perceive and may be more salient at the intimate end than at the impersonal end. It should be less likely that Person will experience inequity relative to Other the closer the relationship is to the impersonal end of the continuum and vice versa. Aside from these direct determinants of inequity,

it would also seem easier to distort cognitively one's inputs and outcomes at the impersonal end, and thus decrease the chances that inequity would persist for very long.

In addition to the argument that inequity will be less likely to occur and easier to eliminate in an impersonal exchange relationship (e.g., an organizational compensation setting), there is another reason for not expecting over-reward effects in real world employment roles. The basic assumption of equity theory is that if someone perceives a situation of inequitable treatment, forces are generated within the person to alleviate the situation. It is proper to speculate on whether or not these forces are the same for over- and under-reward.

In the organizational situation, Person may react to underpayment with decreased effort, demands for more pay, or by generally addressing attempts to achieve equity directly toward the company. In the case of over-reward, however, Person may indeed feel too highly paid relative to Other, but the cognitive and behavioral effects of this inequity may occur only if Person somehow comes to believe that he or she is treating someone else *unfairly*. However, it may appear that the organization does not feel it is being taken advantage of or it would lower Person's pay, fire the individual, or take some other action to increase its own outcomes, increase Person's inputs, etc. Thus, it seems likely that even if feelings of overpayment could be generated in Person, he or she would not behave in a way that could be construed as reducing feelings of overpayment, if indeed no one else seemed to think Person was being overpaid.

In sum, there are a number of reasons why overpayment might produce less behavioral change than underpayment. The identity of the comparison Other, the mode of inequity resolution, and the nature of the "feeling" of inequity are all items of interest. As was the case with the VIE model, we again see a dearth of research dealing with these component questions in favor of "tests" of the overall model. Again, we

would argue that the enterprise would derive more benefit if a greater proportion of our resources were devoted to finding out more about the model's components. For example, the process of inequity resolution seems so complex in its own right that to bypass it seems self-defeating.

A study of Tornow (1971) indicates another area of concern that is virtually ignored by equity theory and research. Reasoning that individual differences may indeed play a part in the intensity of feelings of over- and underpayment, Tornow constructed a questionnaire designed to measure whether an individual was typically input or outcome oriented. For example, some people view "responsibility" or "hard work" as something they must put into a job. Others view these as outcomes that the job provides for them. Tornow began by generating a large list of such items and then asked samples of subjects (students) to indicate whether they considered each item to be an input or outcome relative to working at a job. The items for the final scale were those which yielded the most ambiguous splits (i.e., 60/40 to 40/60), but which still possessed internal consistency reliability.

The Pritchard et al. (1972) study yielded somewhat uneven support for equity theory predictions. The original study ran for six days, involved the creation of a simulated organization in several different cities, hired students on vacation to work, and used two different manipulations for over- and underpayment. Tornow tracked down virtually all the subjects who had participated in the experiment and gave them his input/outcome checklist. The subjects within each of the original experimental treatments were then classified further into input oriented versus outcome oriented individuals. Supposedly, a manipulation of overpayment should have more of an effect on someone who is outcome oriented than input oriented since such individuals should feel even more over-rewarded. For each of the eight cells defined by type of payment (hourly ver-

sus incentive), type of inequity (over-versus under-reward), and type of manipulation (experimental versus natural), the difference in performance on a simple repetitive task between the input oriented and outcome oriented subjects was in the predicted direction. Individual differences were having an effect. One person's input is another person's outcome. Tornow's study is well worth emulating.

### *The Relation of Equity Theory to Expectancy Theory*

A final issue concerns how equity theory relates to the VIE model. Is it in direct opposition? Is it completely consistent? Where does it fit? One feature of equity theory that

narrows its focus is that it has come to be used almost exclusively as a theory of financial payment rather than dealing with the complete spectrum of outcomes. One advantage of this restriction in range is that in the context of financial payment it is possible to make predictions about the quantity and quality of performance under the four conditions specified by crossing overpayment versus underpayment with incentive versus hourly payment; and we have tried to summarize the evidence in each of these four cells. Would similar predictions be made by the VIE model or are the two models in conflict? A comparison of the two models within each of the four cells might look something like the following below.

	UNDERPAYMENT	OVERPAYMENT
INCENTIVE	<ul style="list-style-type: none"> <li>Equity theory predicts more quantity and lower quality.</li> <li>The VIE model predicts the same thing depending on the costs associated with a minimal increase in effort balanced against the increased financial reward. Equity theory must take the same trade off into consideration.</li> </ul>	<ul style="list-style-type: none"> <li>Equity theory predicts higher quality and lower quantity.</li> <li>Presumably the VIE model would predict higher quantity and similar quality, depending on what was really perceived as being rewarded; and thus, the VIE prediction would conflict with equity theory. However, this assumes the valence of money doesn't change, and that other outcomes don't become salient.</li> </ul>
HOURLY	<ul style="list-style-type: none"> <li>Equity theory predicts a decrease in quality and/or quantity depending on which is the "cheapest."</li> <li>The VIE model (assuming we are trying to explain a change in payment on a difference between two groups) would predict the same if the individual was trying to avoid the outcome of being fired. Presumably, the performance required to avoid firing would be less for a lower-paid situation.</li> </ul>	<ul style="list-style-type: none"> <li>Equity theory predicts higher quality and/or quantity depending on which reduces the imbalance more efficiently.</li> <li>The VIE model might predict the same thing if the individual perceives that a higher level of performance is required to "keep the job."</li> </ul>

Based on these comparisons, and a similar analysis by Lawler (1968c), several generalizations seem appropriate. The two models are not really in conflict. In part this is due to the looseness of the two theories. It is almost always possible to conjure up

previously unrecognized outcomes that will reconcile conflicts in predictions. In addition, it seems reasonable to hypothesize that individuals have a concept of fair payment (Jaques, 1961) and departures from "fairness" changes the perceived value (valence)

of money. Thus we agree with Lawler (1973) that equity theory can be subsumed under the general umbrella of the VIE model as an explanation and definition of the need for equity which in turn influences the valence of outcomes.

### Goals and Intentions

Without being very explicit, expectancy models of the motivational process point to the task to be performed as an important intervening link between effort or choice behavior and the outcomes desired by the individual. For a cognitive model it is virtually axiomatic that individuals have intentions about what they will do in the face of certain task requirements and these intentions (goals) have an influence on the choices they make among task content and the effort they expend toward specific performance goals within tasks.

Locke and his associates (e.g., Locke, 1968; Locke, Cartledge, & Knerr, 1970) have suggested a cognitive model which tries to make more explicit how goals and intentions govern effort and choice. Their basic notions are quite consistent with an expectancy model and can best be viewed as an elaboration of the performance component. As it turns out, the evidence which has been subsequently gathered appears at first glance not to support one of the implicit hypotheses of the expectancy valence theory.

In brief, the Locke formulation, which is referred to as a "partial model of task motivation," has the following basic ingredients: First, the most immediate motivational determinant of effort or choice is the individual's goal or intention vis-à-vis some task. By implication, such a conscious intention, or goal, is defined as a goal the individual *has consciously decided to pursue*. That is, the individual has already made certain conscious decisions about the goal in question and this is an extremely important boundary condition in terms of a comparison of this model with other models. In terms of

their behavioral influence, such goals have two important properties. One has to do with the fact that stating a goal or intention is synonymous with giving behavior a direction. This is really a non-motivational consideration in that the existence of a goal reflects knowledge of what is to be done rather than reflecting which goal is the more attractive. The second property is a motivational consideration and has to do with the energizing function in that effort is concentrated on particular goals and some goals require more effort expenditure than others. Thus, research should be concerned with finding goals that elicit the most effort once they become conscious intentions; that is, once the individual makes a commitment to the goal.

The second major ingredient of the model is that changes in the value of incentives (i.e., valence of first level outcomes) can only have effects on behavior insofar as a change in valence is coupled with a change in the individual's goals. This is quite consistent with saying, as does expectancy theory, that changing the availability (instrumentality) and/or magnitude (valence) of incentives changes the valence of specific task goals, such that a particular performance goal now has a high enough valence to elicit effort in that direction.

Third, an individual is satisfied or dissatisfied with performance to the extent that actual performance matches the individual's performance goals. If it does not, a condition analogous to Adams's (1963a, 1963b) inequity results and the individual tries to reduce his or her dissatisfaction. However, Locke et al. (1970) speak only of the case where actual performance falls below the previously accepted goal and the method used to reduce the resulting dissatisfaction is to set a goal to increase output by the necessary amount. Other methods of reducing dissatisfaction, à la equity theory, such as refusing to accept the original performance goal and adopting a lower one, are not really considered. In any event, if we stick to their

circumstances, Locke et al. (1970) predict that dissatisfaction will be significantly correlated with changes in performance via the setting of improvement goals.

Locke and his associates are careful to point out that their model is meant to concentrate on the goals component and is not overly concerned with what influences the establishment and acceptance of goals in the first place. Vroom and Porter and Lawler are criticized for their lack of attention to the controlling properties of goal setting. However, to the extent that Locke et al. do discuss goal instrumentalities and the value of ensuing rewards their discussion is consistent with an expectancy model.

Research by Locke and his associates has tended to support their basic notions. Virtually all their data has been generated in the laboratory, with undergraduates as subjects, and using simple tasks such as reaction time, addition, unscrambling letters in scrambled words, and building windmills from tinkler toys as the experimental vehicle. Subjects typically work on several blocks of trials during a session, and, depending on the objective of the experiment, may either be assigned goals (external), be asked to state their goal (internal) before working on a block, or be asked to verbalize their goals after a block is finished. The rewards investigated to date have been money and knowledge of results.

Based on a large number of individual studies, several conclusions seem warranted.

1. The effects of rewards do seem to be mediated by changes in goals. If offering an individual an opportunity for higher pay does not commensurately increase performance goals then most likely behavior will not change. By implication, assessing goal changes should be one way of predicting the effects of newly instituted incentives.

2. Specific performance goals (above a certain critical magnitude) elicit higher performance on the above type of tasks than do instructions to "do your best."

3. In general, the more difficult the per-

formance goal, so long as it is accepted by the individual, the higher the performance. This is true no matter if the goal becomes so difficult that virtually no one can achieve it.

4. In this restricted kind of laboratory situation, the anticipated satisfaction attached to a changed performance goal (i.e., the valence of a specific performance level in our composite model) does correlate significantly with future performance.

Support for the second or third conclusions also can be found in at least four field studies. Meyer, Kay, and French (1965) compared a traditional kind of superior-subordinate performance review with a mutual goal setting approach and found that the crucial aspect was not whether goal setting was mutual or superior initiated, but whether specific goals were set at all. Looking at goal difficulty, Stedry and Kay (1966) asked production foremen to achieve either the 50th or 75th percentile of their work groups' distribution of weekly quantity and quality scores over the previous twenty-six weeks. Over a thirteen-week test period the results showed that work groups whose foremen perceived the higher goal as "challenging" produced 28 percent fewer defective parts while groups whose foremen saw the goal as impossible experienced a 35 percent increase in defective units. Similar, but less pronounced results were produced for the quantity criterion. If a perception of "challenging" is taken as goal acceptance, then Locke's results are supported. The prediction that difficult specific goals lead to higher performance, other things being equal, than do the best goals has also been supported in field studies by Latham and Kinne (1974) and Latham and Baldes (in press).

A study which tends not to support the first conclusion in the above list is reported by Pritchard and Curtis (1973). Student subjects worked on a card sorting task in the laboratory. After a base trial subjects were assigned to one of six conditions de-

finied by performance goal versus no goal and three financial incentive levels (i.e., none, low, and high). The results suggest that not all the effect of a change in incentives is mediated by a change in goals. However, sample sizes were small and the determination of the actual operating goals used by the subjects was difficult.

### Comparisons with Composite Expectancy Model

As we noted above, there is little in the arguments of Locke and his associates that is inconsistent with the composite expectancy model. The Locke model could be best viewed as an elaboration of the task goal component. However, at first glance one might interpret the above results as contradicting the composite model's prediction that the *higher* the perceived relationship between effort and task accomplishment (expectancy) the greater the expenditure of effort. Their data suggest that the harder the goal and thus the *lower* the probability of success, the greater the effort (given the assumption that performance on the task was almost entirely a function of effort). Unfortunately, it is not possible to know precisely what the subjects in the laboratory studies regarded as the performance goal. Was it actually the task goal specified by the experimenter, in which case the contradiction is real; or was it "demonstrating to the experimenter an ability to work very hard?" In the latter case, the relationship between effort and demonstrating hard work to the experimenter would be greater for the more difficult goal condition and there is no contradiction. In general, perhaps the act of choosing to commit effort to a particular goal changes the definition of the goal. Also, the act of commitment might serve to introduce a new set of highly valent outcomes that have an influence on behavior. If nothing else, this "confrontation" between the Locke formulation and the expectancy model points up the ambiguous nature of the task goal component and the rather

abysmal looseness with which it has been treated by most motivational models, including expectancy theory. We need much more investigation into the motivational properties of tasks and the way in which tasks are defined and redefined by people under various conditions.

### The Theory of Need Achievement

McClelland, Atkinson, and their associates (Atkinson, 1957, 1964; Atkinson & Cartwright, 1964; McClelland, 1951; McClelland, Atkinson, Clark, & Lowell, 1953) have proposed a very tightly constructed expectancy/valence theory of task behavior which is firmly grounded in two specific but pervasive motives—the need for achievement and the fear of failure. Their basic model has gone through several revisions since the early fifties and we will try to portray the flavor of its evolution.

A motive is regarded as a label for a class of incentives (outcomes in our terminology), all of which have much the same effect. Motives are viewed as relatively stable behavior dispositions, or learned tendencies, which are not presumed to operate until aroused by the presence of one or more of the relevant incentives, or situational cues that have become associated with the incentive. Need achievement is defined as a predisposition to strive for success. The fear of failure is defined as a predisposition toward the minimization of failure and pain. The strength of the achievement need is typically measured via a prescribed method of content analysis for responses to the Thematic Apperception Test (TAT). The scoring method was empirically constructed by comparing the responses of people under benign conditions with those in situations where the need was presumed to be operating. Other methods have sometimes been used (Aronson, 1958; French, 1958; O'Connor, 1962) and very recently paper and pencil test *n* Ach has been developed by Hermans (1970). According to Weiner (1970), the strength of the motive to avoid failure is

usually measured by the Mandler-Sarason Test Anxiety Questionnaire (Mandler & Sarason, 1952).

In addition to the strength of the achievement need ( $M_s$ ) and the need to avoid failure ( $M_{AF}$ ), the theory includes four other independent variables:  $P_s$  = the subjective probability of success, or the expectancy that action will result in accomplishing the goal;  $P_f$  = the subjective probability of failing to accomplish the goal;  $I_s$  = the incentive value of success or goal accomplishment; and  $I_f$  = the incentive value (negative) of not accomplishing the goal. The dependent variable is the strength of the tendency to approach the task ( $T_A$ ), which is similar to the notions of force and effort as they appear in other expectancy/valence models.

$T_A$  represents the resultant of the approach and avoidance vectors and is specified by the following equation:

$$T_A = (M_s \times P_s \times I_s) - (M_f \times P_f \times I_f)$$

Unlike the composite picture of expectancy theory presented earlier, this expression contains some important dependencies. According to the theory, the incentive values of success and failure have no operational meaning themselves. Rather,  $I_s = 1 - P_s$  and  $I_f = 1 - P_f$ . The rationale is that for  $I_s$  the satisfaction or pride in successfully accomplishing a task is inversely related to its perceived difficulty. The more difficult the goal the greater the sense of pride in accomplishment if it is reached. Note that this conceptualization of incentive restricts the theory to one particular first level outcome of an internal nature. Note also that the product of  $P_s \times I_s$  is at a maximum when  $P_s = .50$ . Relative to  $I_s$ , the feeling of failure or shame generated by not reaching the goal should be inversely related to the subjective probability of failure. That is, the greater the probability of failure the less shame or guilt the individual should feel if she or he does indeed fail.

By definition, it is further stipulated that

$P_s + P_f = 1.0$ . As pointed out by Weiner (1970), this reduces the degrees of freedom in the basic formula to one and the expression can be transformed to the following:

$$T_A = (M_s - M_{AF}) [P_s \times (1 - P_s)]$$

This form of the model indicates that it is only the relative strength of the two motives that determines the direction of behavior toward the task (i.e., approach or avoidance); and given a particular motive level, the tendency to approach or avoid is greatest when  $P_s = .50$ .

The above expression is really the second stage in the overall evolution of the model. The beginnings of the theory did not deal with the motive to avoid failure but it is made necessary by subsequent research findings. A third stage can be identified in Atkinson's (1964) later inclusion of extrinsic, or what he calls non-achievement oriented, outcomes. They were included because of the assumption that any response is overdetermined; that is, it most likely is controlled by a number of incentives besides achievement oriented ones. In any experiment designed to test the model these motivational determinants are regarded as error and the basic expression becomes:

$$T_A = \text{Resultant achievement tendency} \\ + \text{extrinsic motivation}$$

Thus if for one individual  $M_{AF} > M_s$  but he still approaches rather than avoids the task, the explanation is that the individual has a high valence for money, grades, or whatever.

The need for still further modification of the model resulted from its inability to handle phenomenon such as the Zeigarnik effect and other data which indicated that behavior persisted even when the incentives thought to be necessary for its support were absent (Atkinson & Cartwright, 1964). Atkinson and Cartwright postulated the concept of inertial tendency ( $T_{Gi}$ ) to account for the persistence of goal-directed behavior

and defined it as the strength of the previously aroused but unsatisfied motivation to strive for an achievement related goal. Thus, the full need achievement algorithm became:

$$T_A = \text{Resultant of time} \\ \text{specific achievement} \\ \text{motivation} \\ + \text{inertial tendency} \\ + \text{extrinsic motivation}$$

However, the original definition of the inertial tendency could not account for the experimental finding obtained in several studies (summarized in Weiner, 1970) that when the expectancies are unknown high need achievement subjects who experienced failure tended to persist in achievement oriented responses and low need achievement subjects who experienced failure tended to be inhibited even further. Without going through the mechanisms of it all, the above expression will predict the former but not the latter result. To account for this interaction, Weiner (1972) has proposed that the inertial tendency also be thought of as a *resultant* force. Thus, for individuals with high need achievement, greater approach than avoidance is aroused in an achievement-task situation and this persists after failure and supplements the resultant achievement motivation in the next situation. However, for people low in need achievement, greater avoidance than approach is aroused and if failure is experienced the result will be to inhibit behavior in the next similar situation. The strength of the inertial tendency is a function of the magnitude of the difference between  $M_S$  and  $M_{AF}$  and the original task difficulty. However, the direction of the carry-over effect is the same, regardless of the original expectancy. The original model which did not incorporate  $T_{OI}$  would have predicted this result for low  $n$  Ach—high fear of failure people only if they failed at an easy task. If they experienced failure at

a difficult task the original model would predict an increase in performance.

Thus the full-blown model sees the tendency to approach or avoid a task as being a function of:

1.  $(M_S \times P_S \times I_S) - (M_{AF} \times P_f \times I_f)$  = the resultant achievement motivation stemming from current stimulus conditions.
2.  $T_{OI}$  = the result inertial tendency.
3. Extrinsic incentives.

### Comparisons with Expectancy Theory

Please note that this model of achievement motivation is consistent with the composite expectancy model except for one important discrepancy. It is confined to one particular first level outcome defined in a very specific way (i.e.,  $I = 1 - P$ ). That is, by definition the valence of a performance goal is completely dependent on the perceived probability of being able to achieve it. For this particular kind of outcome, this may indeed be a more reasonable assumption than the composite expectancy model's blanket assumption that all outcome valences are independent of expectancy. For externally mediated outcomes, Atkinson and his colleagues use assumptions consistent with the composite model.

The trade-off for the restriction in focus to one outcome is that more specific and finely differentiated behavioral predictions can be made and the persistence of behavior can be accounted for, given that we stay within the boundaries of the model.

Many examples of such predictions can be found in the excellent monograph by Weiner (1970). To mention just one, consider the effects of programmed instruction in which 100 percent reinforcement schedules are created. Under the condition of repeated successes the tendency of low achievement oriented people to engage in further achievement oriented activity is maximized; however, the converse is true for people high in achievement orientation and the PI tech-

nique will suffer. According to the model, high need achievers should receive reinforcement only 50 percent of the time.

Although Locke (1968) sees his experimental results as being contradictory to the need achievement notion that the behavioral tendency is maximized when  $P_S = 50$ , the two formulations seem to be quite different in operational terms. Again, the crucial element is the commitment of the individual to the goal. Almost by definition, if an Atkinson subject accepts a goal, he or she has interpreted it as one which provides achievement oriented outcomes, and would, if asked, describe the goal as "challenging" or some other adjective signifying "moderate" difficulty. In sum, the two models seem to operate on opposite sides of the commitment point and the subject's own definition of the task is again crucial.

### Attribution Processes and Motivational Dynamics

The notion of expectancy, or the subjective probability that effort will lead to goal accomplishment, pervades contemporary human motivation theory. However, as we pointed out at the beginning of this chapter, the actual level of performance on a task or in an organizational role can be a function of a number of things besides effort. Thus, it is reasonable to view an individual's expectancy estimate as being influenced by his or her explanation of how the various components influence performance in a given situation. If we could describe an individual's "theory of performance" it would do at least two things for us: First, on a priori grounds it would simply be a good thing to understand the antecedents of the expectancy variable, and other variables as well. Second, it would also provide a means for hypothesizing what happens to behavior (e.g., effort) over the long run as an individual experiences successes and failures under various conditions. So far, motivation theories pertaining to em-

ployment behavior have been woefully silent as to what happens to behavior over time.

One body of theory that provides such leverage has been labeled attributional analysis, or the processes by which individuals attribute characteristics or causal relations to various objects as explanations for their appearance or behavior.

As pointed out by Kelley (1967) and Weiner and Kukla (1970), two general kinds of attributional processes have been brought under consideration. The first can be attributed to Heider (1958) and Jones and Davis (1965), and emphasizes the environmental factors or stimulus conditions that affect attribution. The second is most closely identified with Rotter (1966), and focuses on an individual differences variable labeled "internal versus external" control. That is, there seems to be stable individual differences in the degree to which people feel they control or are controlled by their environment.

The first way of looking at attribution has implications for the justifications used by the organization to reinforce success or failure and the resulting alteration of the individual's instrumentalities. For example, an individual's task success or failure might be attributed either to ability or to effort (Heider, 1958). Suppose someone fails, and the people in the organization who control the external rewards attribute the failure to a lack of ability rather than effort. In organizations such as schools, or other training institutions, this may result in greater reinforcement ("that was a really nice try") for failure rather than for success, if task success were in turn attributed to high ability with little effort. Conversely, in a business organization, when failure is attributed to a lack of ability rather than effort it may result in a dismissal, whereas failure due to a lack of effort (in the presence of high ability) may put the responsibility on the organization's back and set management to wondering where it went wrong and how it could improve the "motivation" of capable

employees. The individual himself can affect his internal outcomes in much the same way by attributing his performance to skill, effort, or external factors. In the case of goal achievement, attribution via effort may not lead to as great a feeling of accomplishment as attributions of the form, "I succeeded because I had a great deal of ability to bring to bear on this task." If failure is experienced, it might be much less negatively rewarding if the individual can attribute it to environmental factors or the fact that he or she just didn't feel like trying. Considerable data support this kind of defensive attribution (e.g., see Weiner & Kukla, 1970).

Weiner and Kukla (1970) also report the results of several of their own experiments, albeit with elementary school children, that show how differential rewards are produced by experimentally induced differences in attributions of effort versus ability. Their data also suggest that self-generated attributions and internally mediated negative outcomes have a much more severe effect than externally mediated negative outcomes. Thus, while punishment administered by an independent party may have little effect on behavior, as argued by many reinforcement theorists, it may be quite potent when self-administered.

Given the importance of self-generated attributions, it might be wise for the motivational models to consider the possibility of stable individual differences in these processes. For example, Rotter (1966) suggests that individuals vary in the degree to which they believe they control events (internal control) or events control them (external control) and he has developed a paper and pencil inventory to assess this characteristic. Within our valence-instrumentality-expectancy model, such a variable has important implications for the expectancy component. Someone scoring toward the external end of the continuum may seldom see a connection between either ability or effort and task accomplishment. It is possible that such attributions are associated with being at the very bottom of the socioeconomic ladder

and, therefore, has a very deleterious effect on both academic and employment behavior. Conversely, someone scoring toward the internal control extreme may always interpret the attainment of outcomes as dependent on the individual's own behavior. However, if the situation is really such that no matter what the individual does rewards cannot be brought under self-control there would surely be long-term behavioral effects. For example, self-esteem could steadily erode.

Looking at the same phenomenon in a somewhat different way, Weiner and Kukla (1970) hypothesized that individuals high in resultant achievement motivation (i.e.,  $M_S > M_{AF}$ ) are more likely to attribute success in achievement oriented situations to themselves than people low in resultant achievement motivation (i.e.,  $M_S < M_{AF}$ ), which would in turn enhance the valence of first level outcomes and result in a higher probability of greater effort. They present a number of experiments on grade school children to support this hypothesis.

The data relative to attributions of failure are not quite so clear. However, a provocative suggestion from their studies is that both individuals who are high and low on resultant achievement motivation tend to attribute failure to themselves, but high scorers attribute their failure to a lack of effort while low scorers attribute theirs to a lack of ability. A practitioner attempting to use an expectancy/valence framework to deal with organizational behavior would do well to take these suggestions into account.

In subsequent statements concerning the interaction of need achievement and attribution theory concepts, Weiner (1972, 1974) introduced another major parameter pertaining to the perceived causal agents of success and failure. The four major causal factors considered are effort, ability, luck, and task difficulty; and a distinction is made between stable and unstable agents. Ability and task difficulty are viewed as stable causal agents, while luck and effort are causal elements that can vary a great deal from time to time. What results is a  $2 \times 2$  classification

of causal factors where the two major parameters are internal versus external control and stable versus unstable factors. The four-fold table is as follows:

	Stable	Unstable
Internal	Ability	Effort
External	Task Difficulty	Luck

Weiner (1974) is critical of the Rotter-type distinction between internal and external control because it is confounded with the stability dimension. Attribution in each of the four cells may have different antecedents and different behavioral consequences.

For example, Weiner and his colleagues have conducted a number of laboratory-type studies (e.g., Weiner, Heckhausen, Meyer, & Cook, 1972) which suggest that attribution of causality to stable factors results in a much greater change in subsequent expectancy judgments than attributing causality to unstable factors.

If we were to consider the full range of interactions posed by the above conceptual and empirical analysis, the situation would appear as in Figure 3. Given that an individual is high or low on need achievement we can ask whether task success or task failure is experienced. For either success or failure we must next ask whether the individual attributes the cause of the performance outcome to internal or external con-

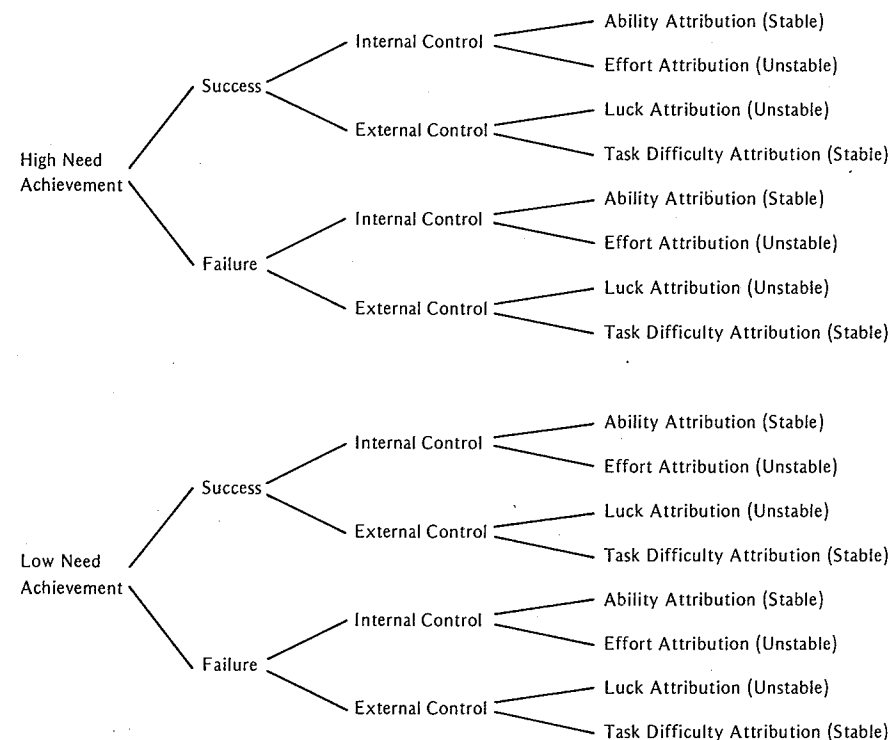


Figure 3. Possible interactions among IDs ( $n$  Ach), performance outcomes, and attributions of causes for success and failure.



trolling agents. If the outcome is seen as being under internal control then the final question is whether the outcome is perceived as resulting from ability or effort. If the performance outcome is perceived as under external control then the final question is whether the cause is attributed to luck or task difficulty.

Such an analysis has several major implications for any attempt to develop a more precise expectancy model of individual motivation in organizations. First, the cell an individual is in has implications for how the perceived value (valence) of accomplishing or not accomplishing a goal might change over subsequent periods. Previous research (e.g., Osipow, 1972) suggests that, in general, people who experience success on a task tend to value future accomplishment of the task more highly than people who experience failure. However, Figure 3 suggests that such a finding masks some important interactions. For example, data summarized by Weiner (1974) suggest that attributions of internal causes increase the subsequent variability in the valence assigned to task success and task failure. External attributions should decrease it. The change in the variance of valence should be further increased if only the "stable" causal elements are considered. Other things being equal, attribution of unstable factors should decrease the difference in the valences of success and failure. For example, high need achievers who experience success but attribute it to their effort rather than their ability may devalue such task accomplishment in the future.

The second major implication pertains to how attributions affect future expectancies. One obvious example is that task failure may influence future expectancies differently depending on whether failure is attributed to internal or external causes.

Third, the above discussion deals only with attributions pertaining to one event, task success or task failure. It would be profitable if we also had an attributional analysis for how individuals explain why

they were granted or not granted rewards by the organization or why someone else was granted or not granted the same rewards. In general, the need achievement model does not make a distinction between expectancy and instrumentality as discussed previously. However, in the organizational situation, it may be profitable to explore how individual differences in causal attribution affect subsequent judgments of instrumentality, as well as expectancy.

The above touches only on some of the implications of the attribution process and our discussion only in a somewhat ahistorical sense. That is, we have not dealt with how attributions develop or can be altered. It is beyond the scope of this chapter to do so. However, Kelley (1967) and Weiner (1972) present an excellent summary of these considerations and the reader is enthusiastically referred to these sources. A deeper understanding of these processes and further research on the issues they suggest seem mandatory. This domain of research and theory is one of the few that speaks directly to how the motivational antecedents of organizational behavior might change over time. Perhaps the best summary statement of how this causal attribution model for performance relates to expectancy theory is to say that it provides part of the missing link that explains how expectancies, instrumentalities, and valences change from time 1 to time 2.

### Some Conclusions

This chapter has spanned a broad range of motivational theory and research. Based on this brief examination, what can be concluded? Is motivation theory alive and well in organizational psychology? Or should we again decide that whatever ails motivation, it's incurable, and while the patient should be kept comfortable, its life should not be prolonged unnecessarily? Neither extreme seems palatable, and as an assessment of where we are and where we ought to go we would like to: (a) offer some gen-

eral conclusions about the state of the art in I/O (Industrial-Organizational) psychology; (b) ask whether issues raised by the experimentalist, but ignored by the organizational literature, might profitably be re-examined; and (c) suggest some directions that future research on adult motivation in organizations might take.

### *The State of the I/O Art*

1. Conceptually, motivation theory in organizational psychology has come a long way since 1960. Previous to that time there was very little for the practitioner or scientist to fall back on when a systematic set of questions was needed to analyze a real situation or a potential research domain. Of course, there was the soon-to-be-shot-down notion that satisfied people are productive people, and a certain amount of attention was devoted to the notion of fatigue and its determinants as well as to the benefits of incentive payment. However, it was not until the 1960s that a much more systematic set of models evolved to provide more comprehensive specifications for the practitioner or researcher who wishes to make a "motivational" analysis of a situation. During the last fifteen years motivational models have come of age, or at least have moved through the pubescent years. Whether optimum use is being made of the available theory is another story. We think not.

2. In spite of the potential usefulness of the available models, there is a strong suggestion that industrial and organizational psychology is repeating a historical mistake in its quest for an overall, all encompassing model such as valence-instrumentality-expectance. Previously, Hull attempted to formulate a general model of behavior, and failed. As a result, the experimentalists seem to have given up the objective of a motivational "grand design." Whether the proposed function is  $sE_R = sH_R \times D \times K$  or  $F = E(\Sigma V \times I)$ , the behavior domain involved is too complex and its bits and pieces are too poorly understood to permit one

algorithm to do the job. To make such an algorithm serve this purpose, either the right side of the equation will have to be made impossibly complex or the left side (i.e., the dependent variable) must be defined so narrowly that all resemblance to organizationally meaningful behavior is lost. Also, we have already discussed the more specific problems created by invoking multiplicative relationships among poorly understood variables. These compound the overall problem.

For these reasons we suggest that I/O psychology abandon attempts to predict behavior using the quantity  $F = E(\Sigma V \times I)$ . This is not to argue that the function can't serve as a useful heuristic for identifying specific variables or relationships of interest and for guiding future research. What we are counseling against is seeking one overall grand design that will produce one summary index that has a high correlation with a highly important dependent variable.

3. Another aspect of the grand design syndrome is the expectation that a relatively simple operationalization of the function, "performance = ability  $\times$  motivation" will yield meaningful and significant correlations. Again, this equation may have a certain amount of heuristic value but to take it literally and multiply an aptitude test score by some composite of expectancy, instrumentality, and valence for the purpose of predicting performance is to come very close to jousting at windmills. We could better spend our time asking less cosmic and more useful research questions.

By outlining the schematic representation of task performance attributions in Figure 3 we certainly were *not* trying to offer up a grand design of attributional processes that should be "tested" by one overall study. Such studies would meet a predictable fate, the results would be confusing and inconclusive. Rather, it should function as a heuristic device for trying to sort through a wide range of potential research questions which would yield the most useful information about performance attributions.

4. By implication, the above arguments



also point an accusing finger at the dependent variables used by motivation theory and research. There is almost universal agreement that what we are trying to predict is choice behavior, effort expenditure, or the persistence of behavior. Effort expenditure is probably the most popular. However, we know virtually nothing, in a measurement or construct validity sense, about the dependent variables we are trying to explain. The "criterion problem" is no less important here than in personnel selection. Why has it been so largely ignored? For example, how should effort be defined? How should it be measured? How should the construct validity of an effort measure be established? Even if we could successfully predict self-ratings of overall effort, we do not at this time know what we would be predicting. Research on performance measurement took a step forward when it backed away from an ill-defined global rating measure and began to look at performance in terms of multiple factors defined by concrete observables. Perhaps research on effort should do the same thing.

5. The measurement of motivational independent variables is also in a very primitive state. We have been much too content with one-item measures couched in Likert formats. We share Guion's (1973) strong misgivings about this procedure as well as his advice that many of the criteria for judging the usefulness of psychological tests should be applied to these variables as well. There is virtually no research that has attempted to develop measures of instrumentality or expectancy such that they possess demonstrated construct validity. Items are simply written down and the investigator goes on to test a series of "hypotheses."

6. In spite of the above problems of execution, we think that motivation theory has exhibited a great deal of progress on the conceptual level. There is considerably more clarity in the motivational domain vis-à-vis industrial and organizational psychology than there was before 1960. One qualifica-

tion to this point is that the taxonomic models described in the section on content theory do not yet seem to provide very much guidance concerning the potential outcomes that should be considered in a particular situation. In terms of desired job outcomes, or rewards, we still have very fragmentary knowledge about why people work.

#### *Issues from the Experimentalists*

If we go back to our listing of motivational issues taken from the experimental literature and compare them to the issues prevalent in the organizational literature there are some obvious discrepancies that deserve mention.

1. By and large the experimentalists have backed away from the concept of need or drive while need theories are still a viable topic in the organizational literature. Given the difficulties experienced in the infra-human laboratory, perhaps it would be wise to seriously consider concentrating on taxonomies of observable outcomes rather than underlying need structures. Sticking with observables would obviate many of the problems created by need theories.

To the extent that experimentalists have been concerned with the drive concept, the organizational literature has tended to parallel the experimental literature. That is, questions concerning the definition of needs, their dimensionality, and the dynamics by which they influence their effects have been prominent in the organizational literature. The frustrations involved in answering these questions have also been parallel.

2. In contrast, the notion of arousal level which became an important variable in the experimental literature has been largely ignored in the organizational setting, except for the work by Scott (1966). However, Scott and his colleagues have not anchored arousal in physiological measures, which is how Berlyne (1967) and others think it should be operationalized.

3. Experimentalists have been quite con-

cerned with the question of what mechanisms make a reinforcer reinforcing. While cognitive models pay lip service to the notion that job outcomes take on value because of their instrumentality for obtaining other valued outcomes, such data have not really been systematically collected. For example, we really do not have a systematic body of data pertaining to why money is or is not valent for some people.

4. The notion of adaptation level, and other mechanisms by which the "value" of an outcome might change have also been largely ignored in the organizational sphere. Equity theory addresses itself to this problem, but only in a very limited domain.

5. Some experimental issues have no real analog in the organizational setting. Primary versus secondary reinforcement and the consequent issues surrounding the parameters of secondary reinforcement is one such area. This one, as well as others, seem to be issues that arise almost as the result of the organism under study.

6. A number of experimentalists have worried a lot about whether the behavioral effects of a particular variable are of a motivational or learning sort. That is, to what extent is the acquisition of new responses involved, rather than the selection or implementation of already available responses? For the experimentalist this question occurs in the context of reinforcement and animal behavior, and thus appears to lack relevance for human behavior in organizations. However, the basic issue is quite real when the effects of goal setting or causal attributions are being considered. We would do well to pay much more attention to the distinction.

7. One glaring discrepancy in the organizational literature is the lack of a systematic concern for the Skinnerian or pure reinforcement point of view. Cognitive models have tended to overwhelm noncognitive ones. Unfortunately, it would be difficult to make any systematic comparison between the two in an organizational setting since each of them incorporates quite different

variables. A Skinnerian model makes no use of variables that pertain to internal states or otherwise involve surplus meaning, while such variables are the very foundation of an expectancy model. Thus, it is difficult to construct situations in which the two "models" would make contradictory predictions.

If we take Skinner at his word and think of the reinforcement orientation as a technology rather than a theory, the general principles that positive reinforcement is more effective than negative reinforcement and shorter latencies are better than long ones would not really be disputed by a cognitive model. What might be disputed are the notions that an outcome's incentive value is best inferred from an individual's past reinforcement history and that variable ratio reinforcement is a more powerful influence on behavior than other reinforcement schedules.

Any attempt to resolve the latter question must deal with the twin questions of what is a response and what is a reinforcement. In the employment situation, these are extremely difficult questions to answer and we have devoted little attention to them. Without any information concerning what the individual defines as a response and a reinforcement, it is always possible to explain away discrepancies between the expectancy and reinforcement models on the grounds that the subjects were defining responses or reinforcers differently from what we thought. For example, in the Yukl, Wexley, and Seymore (1972) lab experiment cited earlier we really do not know whether or not the subjects perceived the situation in the same way as the experimenters. They may have defined a response (i.e., task achievement) as, "whatever number of completed units it takes to get paid."

To put it bluntly, there is simply a fundamental difference between the reinforcement and cognitive models that gives them a qualitatively different approach to the explanation of behavior, and rather than try to pit them against each other it might be a

wise strategy to make intelligent use of what each has to offer. Thus, if we are expectancy theorists, there is really no reason why we should not carefully review the past history of individuals, attempt to document the outcomes that controlled their behavior, and use this information in conjunction with individual judgments about future preferences to account for effort or choice behavior. There is no denying that using proven reinforcers to provide immediate reinforcement for clearly defined responses is a powerful technology with which to affect behavior. When the reinforcers are known, can be manipulated (by the individual or some designated change agent), and the responses are clearly identifiable, the potential of the Skinnerian approach, for good or evil, is awesome. However, we must at the same time take into account the individual's perceptions of the situation or serious trouble can result. The rate restriction phenomena is one object lesson to keep in mind. Others are making the news almost daily.

#### *Future Research Directions*

Again, it is our belief that the available theory is too lacking in precision to make specific predictions about behavior that can be confirmed or disconfirmed by some overall "test." The principal virtue of the available models is their heuristic value for suggesting where we might better put our limited research resources. The following list constitutes our suggestions:

1. We still do not have much systematic data pertaining to the precise nature of job outcomes that people value. The taxonomic work is sparse and we need much more of it. For example, when we ask someone to rate the degree to which their job provides friendly co-worker relationships, what do they think about before answering? The outcomes incorporated in job satisfaction questionnaires have typically not been defined in terms of observables. As yet we can not really describe in concrete terms the

outcomes for which people work. We also do not have a great deal of faith that the outcomes used in laboratory and field research have been the most relevant set of such outcomes. For example, assuming that the financial piece rates used in laboratory studies in any way represent a sample of the real world outcome domain is a risky procedure in itself.

Determining the nature of relevant job outcomes is much in need of a multimethod approach. As noted earlier, Herzberg and his colleagues took one tentative step and then stopped. Why doesn't someone also ask subjects to think of a time when they felt like changing jobs or to think of the time when they did change a job, accept an offer, work overtime, etc., and then describe what led to that feeling or decision? We could also spend much more time trying to document the reinforcement histories of individuals as per the reinforcement view.

Since the available outcomes play such an important role in all the models we have discussed it is a bit startling that so little hard-nosed attention has been devoted to their description and definition. We are badly in need of both substantive and methodological innovation in this area.

2. In general, we think the entire spectrum of motivational concerns is in need of more descriptive or process type research. That is, besides attempting to predict effort or choice behavior with an *a priori* set of independent variables, we should also devote some of our resources to empirically analyzing the process by which individuals choose to expend a certain level of effort. An analog is the history of research on the employment interview. One frequent type of interview study was the investigation of the reliability and validity of interviewer judgments for predicting later job performance. That line of research was really something of a dead end. Subsequently, the understanding of the employment interview took a giant step forward when the actual processes by which interviewers arrived at a de-

cision began to be studied. We think similar benefits would accrue to an understanding of effort or choice behavior if we used a variety of methods to "recover" the processes by which individuals came to choose one task over another or change their level of effort expenditure.

3. We have already spoken to the need to develop better measures of the independent and dependent variables incorporated by the various models. For example, expectancy and instrumentality are constructs deemed to be important, and developing measures for them requires a substantial construct validity effort. Trying to build an understanding of motivation on unresearched measures of one item is a bad gamble. For example, we really don't know if subjects interpret the expectancy and instrumentality questionnaire items in ways that are consistent with the theoretical notions of the investigator.

4. In a similar vein, our science would benefit a great deal if a systematic program of research (or researches) could be devoted just to the criterion problem. As noted in the introduction to this chapter, what we should be trying to explain are effort and choice behavior and the dynamics thereof. These dependent variables must be defined in such a way that they are not confounded with ability, task understanding (i.e., learning), and the constraining influences of the situation. The experimentalists worry a lot about learning versus motivational effects; and we should share some of their anxiety. For example, much of the research on goal setting seems to confound learning and motivational considerations.

In pursuit of this line of research, we think it would be a mistake to seek a continuous criterion variable representing a "pure" measure of effort. Rather, it would be better to think of the dependent variable in terms of discrete decisions that individuals make. Thus, we should develop methods to assess whether individuals decide to work harder, adopt a certain goal, or stop

trying for a particular goal. This argument is based primarily on technological grounds. Development of a continuous pure measure of effort does not look all that feasible. Please note that we are not advocating predicting self-reports of decisions. The intent is to deal with observable behavior, but the developmental research for such an indicator must show that it measures observed behaviors that are consistent with individual intentions.

5. Atkinson, Locke, and others have produced enough evidence to convince us that the task goal and the way it is perceived are important determinants of the volitional side of behavior. Much more work on tasks and their important parameters is needed. Again, a certain amount of process research is called for. One important component of this research should be the separation of learning and motivational effects. For example, to what extent does increasing the specificity of goals result in "knowing what to do" versus "deciding to expend greater effort"?

6. Finally, it is surely obvious to all readers that there are now available a large number of practical motivational techniques designed to alter the effort or choice behavior of individuals. Incentive payment, flexible hours, employee counseling, management by objectives, supervisory consideration, participation in decision making, and job enrichment are but a few. In our opinion we really have very little systematic knowledge about the actual motivational processes involved in each of these. For example, why does participation work? A frequent answer is that participation increases commitment to the implementation of a decision. But what does that mean? Such an answer is still much too coarse to be very informative, and we need much more systematic information about the processes involved. For example, what valued outcomes are created and provided by participation? Does it influence expectancy and/or instrumentality? Does it involve equity considerations in any

way? A similar set of questions could be asked about job enrichment. Lawler (1973) posed a few of them but the list of process questions he suggested could be expanded considerably.

In sum, unless we use our available theoretical tools to examine systematically the processes by which a specific practical procedure works or does not work, we are not using motivation theory in industrial/organizational psychology to its best advantage.

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