

ments about the world" and are epitomized by naming responses and descriptive statements. The second class of behavior, that behavior which varies not with entities but with situations, constitutes "mands." They are responses under the control of specific reinforcing contingencies such as might characterize a particular behavioral situation. Mand—*which include commands, demands, and requests*—are "comments" about the reinforcing properties of social situations as these have relevance to the needs of the actor.

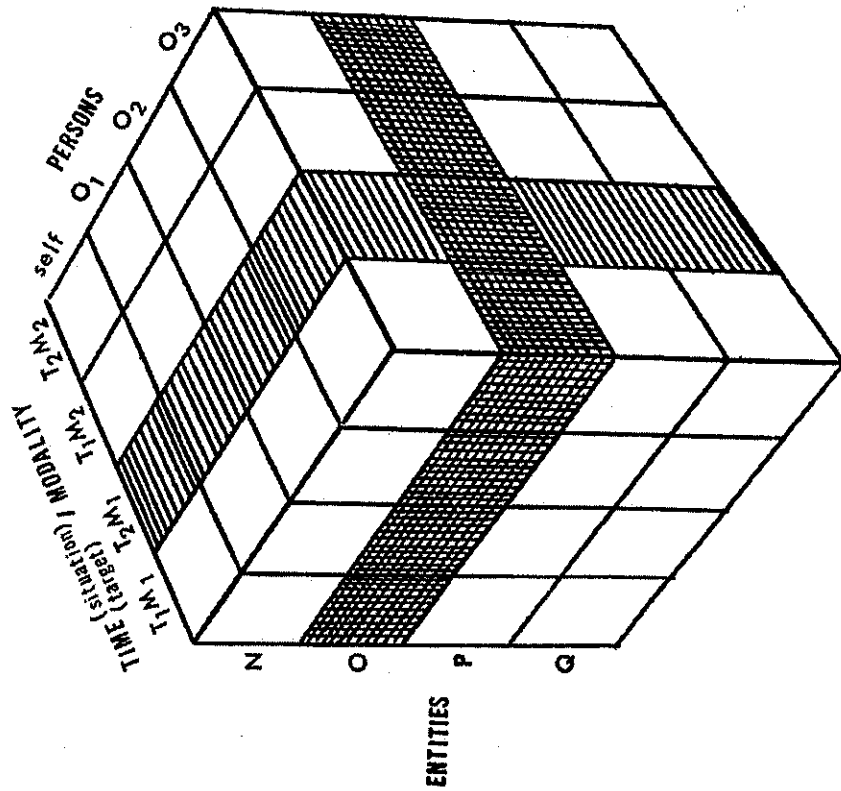


FIG. 4. Attribution interpretation of distinction between tacts and mandates.

In our data table, the distinction between tacts and mandates will appear as in Figure 4. Consistency within a horizontal slice of the table, corresponding to a given entity, would imply the response has tact properties. On the other hand, consistency of the response within a vertical slice corresponding to a given situation would indicate the behavior has mand properties.

Bem assumes (as I would, on the basis of this analysis) that tacts are considered by observers to reflect the actor's attribution to the entity—his true attitude or evaluation. Mand is considered (again, as they would be according to this analysis) to convey no information about the person's attributions to entities. They constitute attributions to the reward/punishment contingencies inherent in the response situation, but they are not relevant to entity properties.

Bem's (implicit) rule for determining whether a response is under cue or reinforcement control involves a simple presence-absence test. If a strong reward or threat contingency is present, the behavior is considered likely to have mand properties. If not, if the response situation is neutral or bland in terms of reinforcement consequences, the behavior is regarded as having tact properties. At this point, the reasoning strongly resembles that of Jones and Davis. Implicit in the judgment of *strong* reinforcement contingencies, as in the judgment of social desirability, is the notion that most or all persons in the same situation would make a response having the same effect. This is a significant implication to which we must return later in discussing Bem's critique of certain experimental studies of cognitive dissonance theory.

A number of other problems in self-perception are illuminated by attribution theory, but the problem of subjective volition is a particularly interesting one. Volition means (according to Webster) "the act of willing or choosing; the exercise of the will." Brehm and Cohen (1962) have considered problems of *subjective* volition, meaning the perception of control over one's own behavior, or perceived degree of choice. In attribution terms, subjective volition means that the behavior is attributed to the self rather than to external forces. Brehm and Cohen's examples suggest a series of conditions under which subjective volition will be high.

Let us first consider three conditions which seem to fall into the same pattern. Subjective volition is high first, when the constraints against leaving a situation (of neutral or negative attractiveness) are low and you remain in it (p. 200); second, the (legitimate) forces producing compliance are low and you comply (p. 204); and third, alternatives are equal in attractiveness and you choose one (p. 203). These three all imply the same pattern of "data" implied by the Jones and Davis analysis. The reason, of course, is that both analyses are oriented toward identifying personal sources of causation. Specifically, both analyses imply actions or effects deviating from what most persons would do and characterized by low consistency over time and modality. You can be said to exercise choice only if you do something different from what everyone else would do in the same situation, or if other times and under other circumstances. The similarity between the Brehm and Cohen examples and the Jones and Davis analysis suggests the interesting possibility of defining volition in terms of perception of acting according to one's own intention. A fourth condition is generally similar to the first three. Volition is high if the amount of pressure to make a choice (as between equally attractive alternatives) is low and you make a choice (pp. 209-210). With high pressure the choice, whatever it may be, is externally or situationally determined. In the absence of such pressure, any choice you make is attributed to yourself.

A fifth condition involves a twist on the factor of social desirability. Volition is high if the strength of *illegitimate* forces is high and you comply (pp. 205-208). This is a case in which conformity provides more information than does *nonconformity*. The implication presumably is that *most* people would resist illegitimate coercion and therefore your compliance must be attributed to your own choice.

The sixth and seventh points are interesting because they refer to the *process* of choice rather than the *conditions* of choice. Volition is high if you consciously concern yourself with which action to choose and give the choice much consideration (pp. 202-203), and if you experience uncertainty, conflict, and the potentiality of alternative responses (p. 168). These are intimately

related to the notion of temporal and modality stability. If an attribution (say, about the relative merits of two entities) does not readily attain consistency upon successive and different views of the problem, the person presumably continues to process the decision (that is, he gives it further consideration). If instability continues, any response he finally makes is likely to be self-attributed rather than externally attributed.

BIASES, ERRORS, AND ILLUSIONS IN ATTRIBUTION

Like all other perceptual and cognitive systems, attribution processes are subject to error. Heider suggests that errors can be traced to instances in which, first, the relevant situation is ignored; second, egocentric assumptions are made; third, the relevant effects have affective significance for the observer; and fourth, the surrounding situation is misleading. I will discuss the first three very briefly and then consider the fourth in more detail. Finally, as a fifth point, I wish to consider briefly some means for the deliberate induction of attributional errors that have appeared in the recent experimental literature.

1. With regard to *ignoring the relevant situation*, Jones and Harris (1966) report evidence which they suggest may indicate a tendency to attach too much significance to the behavior and its effects, and too little to its situational context. In a situation in which a person was observed to express an unpopular and unexpected opinion, the judgment of his true opinion was very much influenced by his expressed opinion *even when the expression had been elicited by strong, legitimate external pressure*. Jones and Harris note that although there is some ambiguity in the interpretation of this effect, it may support Heider's point that "behavior in particular has such salient properties it tends to engulf the total field rather than be confined to its proper position as a local stimulus whose interpretation requires the additional data of a surrounding field..." (Heider, p. 54).

In some cases, as in complex causal environments, the relevant causal factors in the situation are not ignored but are simply not perceptible to the person. Under these circumstances, there may occur quite accidentally a covariation between actions and consequences which will create a strong impression of self-control

over events that are really externally controlled. For example, if the reinforcing environment gradually becomes increasingly beneficent (e.g., during an inflationary period when the profits of certain businesses increase almost without regard to what they do), an action a person happens to take early in the trend will be reinforced and increase in its frequency. By this effect, the rate of that particular response (or effort, amount of trying, etc.) may rise along with the rising rate of reward. The resulting covariation would provide compelling evidence for attributing the environmental changes to the self. (Experimental evidence for somewhat similar phenomena comes from studies of "superstitious" behavior. See Kimble, 1961, pp. 197-198). The reader will note that an attribution error of this type, in which a self-attribution is made for what is actually an externally caused change, is likely to be asymmetrical, occurring only in situations of increasing environmental benevolence. If the external causal system is declining in its rate of reward, the person is likely to make repeated adjustments in his behavior in an attempt to stem the decline. Since it is unlikely that he will experience any covariation, one way or the other, between his actions and his rewards, he will probably attribute the decline to external factors and not blame himself.

2. *Egocentric assumptions* are important when the evidence for the attribution is incomplete. For example, Heider suggests that attributions are often made on the basis of what he refers to as the "minimum data pattern" which is the simple presence-absence test: one instance of presence of both object and effect coupled with one instance in which both are absent. This simple experience leads to an attribution to the object which then often leads to assumptions that the other attribution criteria will also be fulfilled: What I now enjoy I will enjoy again and other persons will also enjoy. It is not clear to what degree assumptions of this sort are likely to be in error in such domains as those of desire and pleasure (where Heider makes this point), but at least they make it possible for there to be systematic differences in attributions between an individual and a person observing him. As Heider puts it, the individual may have only the minimum data pattern, but an observer of his reactions will also have (at

the very least) the additional evidence of his own reactions. Under these circumstances, the individual's *external* attribution of his reaction will often be in contrast to the observer's *personal* one. Specifically, if the observer fails to share the individual's reaction, he will attribute it to personal characteristics of the individual himself.

But the egocentricity extends also to the observer. When the reaction he observes the individual to make differs from his own, he is inclined not only to attribute the former to personal characteristics but also to attribute the latter to the object world. "The person tends to attribute his own reactions to the object world, and those of another, when they differ from his own, to personal characteristics in [the other]" (Heider, p. 157). This, in brief, is Heider's theoretical account for what he refers to as "a tendency for attribution in general to be leveled at the environment" (p. 156).

Yet this problem of egocentric assumptions has not been fully analyzed. Against examples given above, in which the individual erroneously assumes his reaction to be common, we can pose counter-examples in which the individual erroneously assumes his reaction to be unique. This seems to lie at the basis of the phenomenon of *pluralistic ignorance* where each person assumes that everyone except himself accepts and conforms to social norms. (The attributional implication is that everyone else is assumed to find the norms to have the properties of objective standards rather than arbitrary social rules, but each individual finds evidence in his own experience contrary to this external attribution.) The phenomenon of pluralistic ignorance exists when there is a strong consensus in the public labeling of the relevant behavior (as if it reflected external attributions) but careful concealment of the behavior itself. The consequences of these two circumstances is that people are led to draw an erroneous equation between what is socially desirable and what is common. Intuitively, it seems probable that the condition underlying this state of affairs is a social norm that is based on assumptions about attributes of external objects that are highly nonveridical, is too weak to prevent individuals from interacting directly with the

objects, and is therefore repeatedly disconfirmed in the individuals' own experience.

3. *The magnitude of the affective consequences* involved in an attribution may also bias the interpretation made. Heider suggests that a negative self-attribution may be avoided because it would undermine the individual's self-esteem. Uniformly critical action toward a person by his associates will be attributed by an observer to the person inasmuch as he is the constant factor in the situation. But to protect his pride the individual may attribute the common treatment to a conspiracy against him, perhaps based on jealousy of his superior attributes. Ego-protective attribution of this sort is illustrated by results from an interesting study by Johnson, Feigenbaum, and Weiby (1964). Their experimental subject attempted to teach a student some arithmetic materials. The student did not actually exist, but he was represented convincingly to the subject by a prearranged set of completed work sheets. He "did poorly" on the first unit of material and the instructor taught him a second unit. After this second instruction, the student improved for some instructors but for others he continued to do badly. The subject-instructors for whom the student showed no improvement tended to attribute the performance to the student, whereas those for whom the student improved tended to take credit themselves for the improvement, even to the extent of making more favorable evaluations of their own second presentations. The former attribution, that made to the student, would seem to be ego-defensive in character.

An interesting interpretational problem is posed by the latter type of subject who credits himself with the performance improvement. Apparently Johnson and his colleagues have succeeded in fooling him by means of their experimental procedure, arousing in him the impression of self-competency by simply giving evidence that the student improves after the second instructional attempt. Yet this subject may have compelling personal evidence indicating his effectiveness. Insofar as he tried harder or employed a different method on the second instructional attempt, the minimal data pattern available to him would strongly indicate his causal role in bringing about the improvement. At the same time,

this experimental procedure suggests a general way of playing attributional tricks on an unsuspecting person by bringing about a covariation between his actions (attempts, efforts) and the consequences.

Jones and Davis (1965) deal with affective consequences in terms of the *hedonic relevance* of the actions for the observer. They suggest that as the hedonic relevance increases, the observer is likely to assimilate various effects of the actor's behavior to a single theme suggested by the predominant hedonic value. The effect is, they suggest, to reduce the number of unique (noncommon) effects perceived to result from the behavior. This permits the observer to make a more confident inference as to the actor's intentions. We might also suspect that the important negative or positive consequences (and particularly the negative ones) of a person's action make the affected person keenly aware of the other actions that might have been taken with less extreme consequences. Thus, the set of alternative actions the interested observer considers in evaluating the chosen one is likely to be biased in a manner that makes the latter appear to be more unique than it would otherwise. There is, unfortunately, little evidence bearing upon either of these interpretations of hedonic relevance. In fact, the evidence that *any* bias results from hedonic relevance is less than adequate at present.

A study by Walster (1966) suggests that *magnitude* of consequences, whether relevant to the observer or not, may affect the attribution. She presents evidence that the worse the consequences of an accidental event, the greater is the tendency to assign responsibility to the person possibly responsible for it. This is true even though the evidence about the person's actions in relation to the accident are identical for the two samples of subjects told respectively about the mild and the severe consequences. Furthermore, the subjects told of the severe consequences of the accident (involving the runaway of an automobile parked on a hill) express higher standards with respect to factors contributing to the consequences of the accident (faulty brakes and lack of accident insurance). Again, we might suspect that the severe consequences make more salient actions that might have been taken which would have attenuated the consequences.

4. Let us now consider cases of attribution in which the *surrounding situation is misleading*. Here, the concern is with cases analogous to optical *illusions* where the field in which the figure is embedded exerts a distorting influence on the perception of the figure. Some examples in the area of social causation can be provided by returning to Bem's critique of cognitive dissonance experiments.

Bem uses his version of attribution analysis to interpret certain "forced compliance" experiments conducted as tests of cognitive dissonance theory. The reader will recall that in the classical experiment on this problem, by Festinger and Carlsmith (1959), the subject was induced, with either a \$20 or a \$1 payment, to misrepresent to another subject his views about a dull and boring task. Consistent with the hypothesis of insufficient justification (derived from cognitive dissonance theory), the smaller the incentive provided the subject for this misrepresentation, the more he was found subsequently to hold favorable attitudes about the task.

Given Bem's theoretical viewpoint, in which the processes of self-perception are considered identical with those in social perception, it occurred to him to ask what attitude an observer would attribute to subjects in the Festinger and Carlsmith experiment, given knowledge of the conditions and their behavior. Accordingly, Bem conducted what he calls "interpersonal replications" of several cognitive dissonance experiments. In his interpersonal replication of Festinger and Carlsmith (Bem, 1967), his subjects listened to a tape recording which described the *circumstances* of a subject in one of the experimental conditions (the boring task, the request made of him to describe the task to another subject as being interesting, the payment given him for doing so) and his *actions* (the comments he made in complying with the request). Bem's (observer) subjects were then asked to estimate that subject's attitude toward the task. The results closely paralleled those from the original experiment. "Observers" of the person who complied with the request when given the \$1 incentive, estimated his attitude to be more favorable than did those making similar judgments of a subject who complied in response to the \$20 offer. Presumably, in the latter case, the large

reward is taken to be the cause of the behavior (describing the task as interesting), but in its absence, the behavior is taken as having *tact* value. The same attribution interpretation is also made of the actual subjects in the Festinger and Carlsmith experiment. When they were asked about their opinions of the task at the end of the experiment, they reviewed their recent experience, their behavior in relation to the task, the conditions of its occurrence, and from this they inferred their attitude toward the task. When they recalled speaking highly of it in the presence of the large reward, they attributed this behavior to situational constraints and not to the task itself. The latter attribution was made when the incentive was small.

In brief, Bem interprets the post-experiment report as an attribution statement that reflects the individual's causal attribution of his earlier behavior—whether it was caused by irrelevant incentives (situation) or by the properties of the task (entity). In view of this interpretation, Bem concludes that such studies as that of Festinger and Carlsmith do not require the postulation of a post-decision dissonance reduction process.

I do not intend here to consider the appropriateness of Bem's critique as a general alternative to cognitive dissonance interpretations of experimental results. Rather, I wish to subject both Bem's analysis and the procedures of certain cognitive dissonance experiments to close scrutiny from an attribution theory point of view. The analysis has two problems even as applied to the forced compliance experiments, and both of these problems, when carefully analyzed, suggest the possible existence within experimental procedures characteristic of cognitive dissonance research of *attributional illusions*. I shall refer to these two problems as (a) the illusion of freedom, and (b) the effects of unanticipated consequences.

(a) *The illusion of freedom*. Although Bem does not make it explicit, the high- and low-incentive conditions would seem to imply different degrees of consensus in compliance with the experimenter's request. That is, the \$20 incentive would induce all or most subjects to comply and this would be one indication of its role as a strong situational cause. The \$1 incentive would induce compliance only from subjects whose private attitudes are

more or less favorable toward the task. And it does not seem likely that all subjects would feel favorable, in view of the boring nature of the task. Thus, the observer's judgment that the attitude of a \$1 complier is more favorable than that of a \$20 complier is probably associated with assumptions (unchecked in Bem's work, as far as I know) that there is a distribution of opinion toward the task, and only the more favorable subjects complied in the \$1 case and almost all, favorable or not, complied in the \$20 case.

But at this point we encounter a problem. Any assumption of differential rates of compliance in the Festinger and Carlsmith experiment (and in similar, well-conducted experiments of the cognitive dissonance genre) would be quite unwarranted. Virtually all subjects in both incentive conditions complied with the request to describe the task as interesting and exciting. Thus, an observer to the *actual* Festinger and Carlsmith experiment, seeing all subjects comply in both conditions, would not have this basis for making different attributions regarding the cause of their behavior. Of course, this fact of uniform compliance in both conditions does not vitiate Bem's procedure because insofar as possible, he gives his "observer" the *same* information about the circumstances and the behavior as the corresponding subject has in the actual experiment.

If, then, we are to accept an attribution interpretation of the Festinger and Carlsmith experiment (or a Bemian one), we must deal with the problem that the attribution made in the low incentive condition is one that assumes less than total compliance to the situational demands existing there, when in fact the compliance is virtually complete. (I need hardly point out that it is the complete compliance which makes the result non-obvious and interesting to us, as observers of the experiment with total knowledge about the conditions and effects. If only some of the subjects complied under conditions of low incentive, the results would be quite trivial, readily interpretable in terms of subject self-selection.)

A resolution to this puzzle is achieved by assuming that it is possible to induce a person to feel he has total freedom to express himself when in fact he has none. That is, it is possible to induce

a person to state a particular opinion under special misleading conditions such that he and all other persons will do so, but on looking back at it he will feel he was free to express his own opinion. If this line of argument is at all plausible, it leads us to examine typical procedures in forced compliance experiments for clues about how to create this particular attributional illusion, the illusion of freedom.

(I should point out that Brehm and Cohen's line of thought seems to have been heading toward the same point. They came to the conclusion that feelings of volition or freedom of choice are necessary if subjects are to experience high dissonance. As I have pointed out in an earlier section, Brehm and Cohen's description of the conditions necessary for such feelings implies that if such conditions were in effect, not all subjects would act in the manner required of them. And yet, in the well performed experiment, very few subjects are lost, and the loss is no greater in high dissonance conditions than in low. All of which raises the question of how these incompatible requirements are reconciled.)

Certainly a key feature of the typical cognitive dissonance study is concealment of the fact that everyone conforms. In fact, there is often the implication that the individual is the *only* person of whom the request is being made. This would seem to divert his attention, at that moment and later, away from any consideration of whether other persons did (or would) comply.

A second common procedure is to label the decision a free choice: "It's up to you to decide what you want to do." There may even be contained in this kind of comment an encouragement for the person to think about the problem, to process the pros and cons, and even to have some feelings of wavering, indecision, and hesitancy such as are associated with the impression of voluntariness (cf. the earlier discussion of Brehm and Cohen).

The Jones and Davis analysis suggests that the "socially undesirable" action will be judged by the person as reflecting his own decision, and in a similar vein, Brehm and Cohen imply that compliance to illegitimate pressure is self-attributed. A number of the forced compliance experiments contain the direct suggestion or at least the strong implication that the subject's compli-

ance will be an exception to the social rule. (Brehm & Cohen, 1962, p. 75: After indicating that other subjects had written anti-police essays, the experimenter says, "What we really need now are some essays favoring the police side." Davis & Jones, 1960, p. 404: "Surprisingly enough, most of my recent subjects have been choosing to read the flattering evaluation of the person, so... I'd like to ask you to read the negative evaluation...." Davidson, 1964, p. 50: "Thus far we've run some 52 pairs of subjects and, well, quite understandably, we've had 46 cases where the subject has chosen to read the positive evaluation." The subject is then asked to read the negative evaluation.)

In some cases, the nature of the compliant behavior itself is fairly clearly socially undesirable, as for example, telling a lie directly and with seeming sincerity to some person who is innocently involved in the situation, as in the original Festinger and Carlsmith experiment and in one of the conditions of the Carlsmith, Collins, and Helmreich (1966) experiment.

Finally, the experimental procedures seem to include much pressure and constraint which is exerted so diffusely that the subject's attention is never called to its presence. The usual appeal is to the needs of the experimenter or experimental design, but these are mentioned in mild terms and always with a qualifier or offsetting statement. ("As far as I'm concerned you may read either... but I would like to ask you... Do you think that you'll be able to do this for me?" Davis & Jones, 1960, p. 404). There is often a complicated justification in terms of experimental design that probably serves to befuddle the subject, exerting pressure on him but leaving him unable to recapture the reasons or justification for his decision.

Unfortunately, much of the interchange between experimenter and subject that occurs in these experiments—the social process by which the pressure is exerted—is left ad lib—to the experimenter. The published experimental instructions often indicate where the experimenter paused for the subject's reaction, the implication being that the experimenter dealt with these reactions as he saw fit at the moment. As a consequence, many details of the process by which pressure may be exerted without being made salient are not available to us.

In view of these common elements in experiments on forced compliance, it seems not too farfetched to believe that the successful experiment is one in which strong situational demands, entirely sufficient to produce total or near-total compliance, are successfully camouflaged by a network of cues as to self-determining the behavior is misleading. The central attribution assumption in Bem's analysis, then, becomes tenable—that when the subject in the low-incentive condition reviews his recent behavior, it does not occur to him to attribute it to the situation and it is possible for him plausibly to make an entity attribution. Apparently having been free to speak his mind at that point, and having addressed himself to the entity, the subject views his response as having been entity-caused. In the high-incentive condition, the salient fact of the large payment suggests to him a situation-attribution for his behavior.

(b) *Effects of unanticipated consequences.* A different type of cognitive dissonance experiment avoids the problem of noncompliance by introducing the dissonant factors and consequences *after* the action has been taken. Such an experiment raises a different problem for an attribution analysis. Under these conditions, in which the dissonant consequences of the behavior are known to the person at the time of his action, they could not be considered to have been among the causal factors conditioning the behavior and, therefore, could not logically affect the attribution of the behavior. And yet, these experiments yield differences in reported attitudes which are consistent with a dissonance reduction model.

An example will illustrate the type of experiment and the problem it poses for attribution theory. In Brehm's (1959) study known as the "spinach-eating" experiment, a youngster was induced to eat a disliked vegetable (e.g., spinach) and was then told that a letter would be written to his mother describing his action. Presumably this would create negative consequences for him ("If you can eat spinach for those doctors, you can eat it at home for me"). These consequences were thus dissonant with his action. The dissonance-reduction interpretation affords an explanation for the fact that the subsequently measured degree

of liking for spinach was higher for children in this condition than for similar children for whom no letter was to be written home. Similar evidence on the dissonance-generating effects of unanticipated (or unanticipatable) consequences is provided by the excellent experiment by Jecker (1964).

In terms of attribution concepts, if the threat of the letter in Brehm's experiment had been present when the youngster decided to eat the spinach, he could later have attributed his eating to the entry. Choosing to take an action in the face of negative consequences tends to disengage it from situational determinism and leaves it available for attribution to entry properties. But the fact of the letter became apparent only after the eating occurred. The problem is, then, whether it is possible for a person's attribution of his behavior to be influenced by consequences that were not anticipated at the time of the behavior.

Note that Jones and Davis have eliminated this problem from their consideration by dealing only with cases in which the actor may be assumed to have foresight of relevant consequences. Similarly Bem bypasses the problem by reference to the stimuli that were "in control" of the person's behavior which would presumably include only factors present (physically or psychologically) at the time of the behavior.

The question of unanticipated consequences is not a clear one. To an observer of a person's choice behavior, there will obviously be much ambiguity as to what consequences are anticipated at the choice point. And Walster's results might mean that there is a bias toward assuming that all *important* consequences were anticipated. Similarly, a person observing his own behavior in retrospect may be quite uncertain as to what effects he anticipated, and he may, under certain circumstances, suffer considerable confusion among factors objectively present before and after the action.

We may then ask what the conditions might be under which a person can be led to believe his behavior was under the control of information or cues which were in fact present (known to him) only *after* the behavior occurred. One factor would seem to be the similarity between act and consequence: the appropriateness or fittingness of the consequence, its figural properties—

whether it provides good continuation of, or closure to, the action. This is probably what we mean when we refer to consequences that *should* have been anticipated. Uniqueness or non-commonness of a consequence may dominate its temporal properties (especially when it has high hedonic relevance) and supersede the details of *when* it became known or considered. We have fewer clues on this point from cognitive dissonance experiments because unanticipated consequences have not been thoroughly studied, but from an attributional viewpoint, their analysis seems exceedingly important.

5. The attributional analysis of the procedures employed in certain cognitive dissonance experiments suggests that without intending to do so the investigators may be inducing their subjects to make attributional errors. In recent years, an increasing number of experiments have involved the *deliberate* attempt to induce such errors. These methods do not readily fit Heider's four categories, considered above, so they are briefly considered separately at this point.

Hidden causal factors affecting the subject's actual responses.

The general principle here is similar to that described in the above example in which the situation is misleading: the subject's attention is diverted away from an important causal factor. This is illustrated by the work of Schachter and Singer (1962) on social determinants of emotional states. The procedure is to create in the subject through administration of a drug a state of physiological arousal. When the material injected into the subject is described by the experimenter (and accepted by the subject) as having no effects, the subject's "evaluation" of his emotional state (how he behaves and reports himself as feeling) is subject to influence by the example of another subject in the same situation. Apparently, the subject attributes his response (the arousal state) to the situation and then takes his cue as to the nature of that situation from another person who is responding to the same setting. The social example has this influence upon the subject's behavior and subjective reports only when he has no other explanation for his experience. When he attributes it to the injection, either at the suggestion of the experimenter or by

seeing the possibility himself, his susceptibility to the other person is lessened.

Erroneous information about the subject's responses. While Schachter's procedure is intended to control the subject's actual responses, other experiments provide the subject with erroneous but apparently convincing evidence as to what his responses are. This method was developed independently by Bramel (1962, 1963) and Gerard (Gerard & Rabbie, 1961; Gerard, 1965). The former provided male subjects with false information about their reactions to pictures of other males, described as indicative of their level of sexual arousal, and the latter (Gerard, 1965) provided subjects with false information as to their "first impulse" (i.e., pre-action) muscle potentials, described as indicative of their choice tendencies with regard to alternatives placed before them. The purpose of the procedure is to influence the attribution the person makes about the stimuli he is shown or his attribution about himself. By providing feedback indicating that his preferences covary in a particular consistent manner with successive stimuli, the subject may be led to conclusions about the relative merits of the external entities (cf. Valins' experiment, 1966, described below). On the other hand, Bramel's purpose (1963) in leading a subject to believe he had a consistent tendency to react with "arousal" to handsome nude males was to induce the self-attribution of homosexuality. Whether the attribution is made to self or entities presumably depends upon what assumptions the person makes; e.g., about how other persons would react to the same stimuli. Yet, Bramel's interpretation of his data suggests that these assumptions may serve ego-protective functions rather than simple attributional ones. Specifically, the personal significance of the "arousal" reactions and, hence, their esteem-threatening implication can be attenuated by assuming other persons would experience similar reactions.

The success of this method in producing the above effects presumably depends upon the subject's not having very good direct evidence about his own reactions. In Gerard's (1965) experiment, this was not the case, but he was using the method to induce self-attributions of "conformer" vs. "deviate." The subject made his alleged "first impulse" responses to simple visual judgment

problems after observing the responses of two other persons. Some subjects observed their responses to be incorrect but conforming, and others observed them to be correct and nonconforming. The former would, of course, constitute evidence of a tendency to make *mand* responses in this situation and the latter, evidence of a tendency to make *tact* responses.

Perhaps the most memorable study of this type (at least to date—this procedure provides an appealing challenge to experimenters' ingenuity) is that of Valins (1966). Subjects were shown a series of slide pictures of seminude females and at the same time heard what they believed to be the amplified sounds of their hearts beating. Variations in rate changes of the sounds were coordinated with presentation of the slides. Thus, in one experimental condition, each subject heard his heart rate increase soon after the presentation of each of five of the slides (and this occurred on both of two successive exposures to the series of slides), but he noted no such increase after five other pictures. (Other subjects heard decreases in rate rather than increases.) The effect of the covariation procedure was that subjects rated as more attractive the pictures with which the changes had been associated and more frequently chose them as remuneration for their participation in the experiment. The implication is probably that the subject makes attributions about the various stimuli; i.e., about their attractiveness. But to rule out the possibility of a self-attribution, additional data would be required; e.g., subjects' expectations about how others would react to the same pictures. Valins presents anecdotal evidence that some of the subjects found the alleged changes in heart rate to be sharply inconsistent with their immediate reactions to a picture. In these cases, the changes were attributed to other factors and presumably (although Valins does not present the relevant evidence) the expected attributions of attractiveness were not made. This serves to emphasize the point noted above, that the success of this procedure depends upon the subject's not having very good direct evidence as to his (in this case) differential reactions to the various stimuli.

Erroneous information about the consequences of the subject's responses. Experimental psychologists have long used the

procedure of giving the subject false information about the success or failure of his efforts. The experiment by Johnson, Feigenbaum, and Weiby (1964), described above, employs this method to examine an attributional problem. Some of the research on social reinforcement of verbal behavior can be viewed in this light, particularly that on noncontingent reinforcement where the rewards or punishments are provided on a predetermined, more or less arbitrary basis. For example, Bavelas, Hastorf, Gross, and Kite (1965) show that arbitrary reward of a particular group member's actions and punishment of the actions of his co-members can result in an increased frequency of his participation and also in higher evaluations of his contributions, both by himself and by the co-members. One of the experiments reported by these authors shows the importance of temporal consistency of the rewards and punishments. They do not have the effects just noted, either on behavior or evaluations, if they are delivered at arbitrary times during the interaction. They must be delivered in proper temporal juxtaposition with the various actions.

The study by Bavelas and his colleagues is basically a demonstration of expert influence. The rewards and punishments inducing the behavioral and evaluational changes were thought by the subjects to reflect the opinions of experts as to the quality of the several members' contributions. In a further study by Hastorf, Kite, Gross, and Wolfe, (1965), a more explicit attributional factor is introduced. These investigators test observers' reactions to an increase in behavior brought about by social feedback that is being used to *manipulate* the behavior rather than evaluate it (as in the earlier study). The group member whose participation is observed to increase under these circumstances of external control does not receive a corresponding increase in evaluation of the quality of his behavior and the increment is discounted in judging what his typical level of participation is. The point is, of course, that the social feedback is no longer seen to be a consequence of the person's contributions (as it is when it is represented as an evaluation of his behavior). Thus, the feedback presumably varies with the inten-

tions of the "controllers" rather than with the behavior and contains no information about the attributes of the latter.

CONCLUSION

In this paper, I have explored the parallels that exist among the ideas of persons who, from various different theoretical backgrounds and for different purposes, have been dealing with what are essentially problems of causal attribution. I believe there is a genuine convergence of concepts in this area, despite the fact that some investigators begin with Heider, others with Skinner, and still others with Festinger.

As lengthy as this paper has become, I have not been able to deal with many of the significant aspects of attribution theory and its implications. I should at least *mention* a few more:

1. Attributions about the self and the basis of need for information about the self. Investigation of this topic might involve an analysis of social situations (e.g., *T* groups or sensitivity training situations) in which unique information regarding the self is presumed to be gained.
2. The assignment of credit and blame. The understanding of the effects of unanticipated consequences, briefly discussed in this paper, may be of great importance in dealing with matters of unwarranted guilt. The other side of the coin is the defensive dismissal of anticipated consequences with the result that feelings of personal responsibility are inappropriately absent.
3. The interplay between language and attribution. The importance of labeling for attribution stability would seem to require analysis.
4. Problems of establishing trust in interpersonal relationships. Attribution theory has important statements to make about the conditions and dilemmas here, as illustrated by Strickland's (1958) provocative study of the matter.
5. The development of attribution processes, with analysis of the stages of attribution as a function of maturation, socialization, and language training.
6. Personality differences in attribution processes. Significant work has been done by Seeman (1963), Rotter (1966), and others on self- vs. external-attributions of causality. A different line of

investigation would be to examine individual differences in preferred modes of attribution, reliance on different criteria for stability, and egocentric biases in attribution.

7. The relation between the common man's attribution processes and the more systematic processes incorporated in scientific methods.

I hardly need to point out the potential importance of some of the problems considered within this framework. To take but one example from the present paper, the question of the illusion of freedom. This is a matter of high importance for a society that is dedicated to ideas of freedom but that seems to find it increasingly necessary to insist on conformity of behavior and expression. Understanding the conditions under which this illusion is created may enable us to avoid adopting the spurious solution it suggests to the problem of the tension between our ideals and the short-run advantages of uniformity.

REFERENCES

- Asch, S. E. *Social psychology*. New York: Prentice-Hall, 1952.
- Bavelas, A., Hastorf, A. H., Gross, A., & Kite, W. R. Experiments on the alteration of group structure. *J. exp. soc. Psychol.*, 1965, 1, 55-70.
- Bem, D. J. An experimental analysis of self-persuasion. *J. exp. soc. Psychol.*, 1965, 1, 199-218.
- Bem, D. J. Self perception: An alternative interpretation of cognitive dissonance phenomena. *Psychol. Rev.*, 1967, 74, 183-200.
- Bramel, D. A dissonance theory approach to defensive projection. *J. abnorm. soc. Psychol.*, 1962, 64, 121-129.
- Bramel, D. Selection of a target for defensive projection. *J. abnorm. soc. Psychol.*, 1963, 66, 318-324.
- Brehm, J. W. Increasing cognitive dissonance by a *fait-accomplis*. *J. abnorm. soc. Psychol.*, 1959, 58, 379-382.
- Brehm, J. W., & Cohen, A. R. *Explorations in cognitive dissonance*. New York: Wiley, 1962.
- Carlsmith, J. M., Collins, B. E., & Helmreich, R. L. Studies in forced compliance: I. The effect of pressure for compliance on attitude change produced by face-to-face role playing and anonymous essay writing. *J. Pers. soc. Psychol.*, 1966, 4, 1-13.
- Davidson, J. R. Cognitive familiarity and dissonance reduction. In L. Festinger (Ed.), *Conflict, decision, and dissonance*. Stanford: Stanford University Press, 1964. Pp. 54-59.
- Davis, K. E., & Jones, E. E. Changes in interpersonal perception as a means of reducing cognitive dissonance. *J. abnorm. soc. Psychol.*, 1960, 61, 402-410.

- Festinger, L. Informal social communication. *Psychol. Rev.*, 1950, 57, 271-292.
- Festinger, L. A theory of social comparison processes. *Hum. Relat.*, 1954, 7, 117-140.
- Festinger, L., & Carlsmith, J. M. Cognitive consequences of forced compliance. *J. abnorm. soc. Psychol.*, 1959, 58, 208-210.
- Gerard, H. B. Deviation, conformity, and commitment. In I. D. Steiner & M. Fishbein (Eds.), *Current studies in social psychology*. New York: Holt, Rinehart & Winston, 1965. Pp. 263-277.
- Gerard, H. B., & Rabbie, J. Fear and social comparison. *J. abnorm. soc. Psychol.*, 1961, 62, 586-592.
- Hastorf, A. H., Kite, W. R., Gross, A. E., & Wolfe, L. J. The perception and evaluation of behavior change. *Sociometry*, 1965, 28, 400-410.
- Heider, F. *The psychology of interpersonal relations*. New York: Wiley, 1958.
- Hovland, C. L., Lumsdaine, A. A., & Sheffield, F. D. *Experiments on mass communication*. Princeton: Princeton University Press, 1949.
- Jecker, J. D. The cognitive effects of conflict and dissonance. In L. Festinger (Ed.), *Conflict, decision and dissonance*. Stanford: Stanford University Press 1964. Pp. 21-30.
- Johnson, T. J., Feigenbaum, R., & Weiby, M. Some determinants and consequences of the teacher's perception of causation. *J. educ. Psychol.*, 1964, 55, 237-246.
- Jones, E. E., & Davis, K. E. From acts to dispositions. In L. Berkowitz (Ed.), *Advances in experimental social psychology*. Vol. 2. New York: Academic Press, 1965. Pp. 219-266.
- Jones, E. E., Davis, K. E., & Gergen, K. J. Role playing variations and their informational value for person perception. *J. abnorm. soc. Psychol.*, 1961, 63, 302-310.
- Jones, E. E., & Harris, V. A. The attribution of attitudes. Unpublished manuscript, Duke University, 1966.
- Kelley, H. H., & Thibaut, J. W. Group problem solving. In G. Lindzey & E. Aronson (Eds.), *Handbook of social psychology*. (2d ed.) Reading, Mass.: Addison-Wesley, in press.
- Kelman, H. C., & Hovland, C. I. "Reinstatement" of the communicator in delayed measurement of opinion change. *J. abnorm. soc. Psychol.*, 1953, 48, 327-335.
- Kimble, G. *Conditioning and learning*. New York: Appleton-Century-Crofts, 1961.
- Ring, K. Some determinants of interpersonal attraction in hierarchical relationships: A motivational analysis. *J. Pers.*, 1964, 32, 651-665.
- Rotter, J. B. Generalized expectancies for internal versus external control of reinforcement. *Psychol. Monogr.*, 1966, 80, No. 1 (Whole No. 609).
- Schachter, S., & Singer, J. E. Cognitive, social and psychological determinants of emotional states. *Psychol. Rev.*, 1962, 69, 379-399.
- Secman, M. Alienation and social learning in a reformatory. *Amer. J. Sociol.*, 1963, 69, 270-284.

- Strickland, L. H. Surveillance and trust. *J. Pers.*, 1958, 26, 200-215.
- Thibaut, J. W., & Kelley, H. H. *The social psychology of groups*. New York: Wiley, 1959.
- Thibaut, J. W., & Riecken, H. W. Some determinants and consequences of the perception of social causality. *J. Pers.*, 1955, 24, 113-133.
- Valins, S. Cognitive effects of false heart-rate feedback. *J. Pers. soc. Psychol.*, 1966, 4, 400-406.
- Walster, Elaine. Assignment of responsibility for an accident. *J. Pers. soc. Psychol.*, 1966, 8, 73-79.

Comments on Dr. Kelley's Paper

Irwin Katz

Professor Kelley's systematic statement of attribution theory, and of its relation to other recent developments in social psychology, should do much to raise it to the status of an explicit, hypothesis-generating, and research-provoking theory. The concepts he develops are rich in implications for a wide range of social phenomena, some of which I hope he will explore further. I should like particularly to see a fuller discussion of the influence of motivational factors on the attribution process based upon his suggestions in the section titled "Biases, Errors, and Illusions in Attribution."

The notion of attributional errors may prove very helpful in analyzing the kind of self-defeating behavior that I have observed in young Negro pupils. In the research reported in my paper it appears as though the academically unsuccessful Negro boys had a habit of unwarranted covert deprecation of their own achievement efforts. Assuming their behavior was a manifestation of low self-esteem, what sort of attributional experiences may have contributed to the development of their low self-esteem? One answer might be, experiences in which performances labeled as *adequate* were consistently attributed to environmental factors (for example, the easy nature of a task) and performances labeled as *inadequate* were consistently attributed to the self. This type

of attributional history would be created if two conditions prevailed: (a) the young child was extremely dependent upon information provided by others for making attributions of his own achievement behavior, and (b) the information provided by others exaggerated his responsibility for poor outcomes and understated his responsibility for good outcomes. The kind of naïve causal analysis that is required to disentangle which effects can be attributed to which factors obviously involves complex cognitive skills that are acquired gradually in the course of a person's development. Therefore, in their early environmental encounters children are likely to be heavily influenced by interpretive cues from adult models. The reliance on adult feedback should be particularly great with respect to self *versus* environmental attributions of achievement, because of the intrinsic difficulty of identifying relevant internal cues of effort, intention, anxiety, fatigue, distraction, and so on. Add to the subjective ambiguity of the child's internal state his ignorance of external achievement situations—of various entities (tasks), modalities (ways of attacking them), and degrees of consensus (performance of peers)—and it becomes apparent that the child is in no position to form confident independent judgments about the quality or causation of his achievement-oriented behaviors.

Various types of illusion may increase the child's susceptibility to the interpretations of adults. He may be subject to an illusion that to have power over him is to have knowledge of him, as suggested by a cognitive balance hypothesis. To the extent that adult models have a high degree of power over many of the young child's affective outcomes, the child may tend to perceive them as both expert and trustworthy in their interpretations of his behavior. This would lead him to adopt their interpretations as his own. Also, he may infer his intentions from the outcomes of the behaviors that are attributed to him by adults; so that if his performance is often judged inadequate, he may infer that he is lazy, academically disinterested, or deliberately uncooperative. Finally, his ignorance of the true determinants of his behavior may, in combination with adult persuasion, foster in him an illusion of freedom—that he "could do better if he really wanted to."

Not much is known about the attributional cues that had been given to the unsuccessful Negro pupils in my sample by adult models, though my data on the boys' perceptions of parents do suggest that the parents provided the kind of information that would produce biased self-attributions. The exact nature of the parental behavior is not clear, but what is known seems to be consistent with my assumptions. No information is available on the behavior of teachers.

If I am correct in my assumptions about the role of attributional factors in the development of motivational blocks to classroom learning, there would seem to follow certain suggestions for remedies. I will mention a few that have to do with training the child to make more accurate attributions of his classroom behavior. Clearly the child should be sensitized to relevant internal processes so that, for example, he can recognize when he is trying to do well on a task and when he is distracted by fear of failing. He should be provided with labels for these and other internal states that influence his performance. The training should be concerned as well with improving the child's ability to use information from the environment, such as the knowledge of the relative difficulty of tasks that can be gleaned from social comparisons. Enlarging the child's perceptual field for attributions will require in some instances reducing the affective consequences of success and failure, so that he will not be overwhelmed by the salience of his own behavior. In general, it might be easier to change the child's own bias-inducing assumptions by introducing as many new factors into the situation as possible—new tasks, learning methods, classmates, and teachers. Old teachers may prove to be relatively ineffectual as change agents, because their alteration of previously expressed evaluations and attributions may lower their apparent trustworthiness, especially if their previous interpretations fitted the child's self-image. New classmates, representing perhaps a broader range of abilities, could enhance the informational value of consensus. Finally, attribution theory suggests the desirability of exploring the characteristic biases of teachers, including a possible proneness to commit the error, mentioned by Kelley, of attributing learning failures to the child and successes to their own instructional efforts.

