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CHAPTER 7

Some Problems with Linking Goals to Discourse

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This chapter is concerned with exploring, via the particular case of "goals," some general problems facing theories that use intrapsychic concepts to explain discourse behaviors. The use of the concept of goal to explain face-to-face interaction is an instance of a widely shared paradigm that assumes that the mind causes action, in other words, that an individual's observable behaviors must ultimately be explained by his or her mental processes. (Similar concepts include "intention," "motivation," "cognitive processes," "learning," "personality," and so forth.) This paradigm is typical of psychology as a discipline, including social psychology, but it can also be found in many other social sciences, including communication and linguistics. A minority in each of these fields, including many discourse analysts, focus more on overt communicative acts and generate descriptive rather than inferential theories. What I have to say here has much less relevance for the latter group.

The first modern goal theorists in psychology were Tolman (1932, 1959) and Lewin (1935; Cartwright, 1959). In their theories, goals were external to the organism (whether rat or human); they were "out there" (much like "goal *posts*" in idiomatic use). Although goals might create hypothetical states in the organism, the goals themselves were specifiable conditions or events that elicited and shaped behavior (e.g., Bavelas & Lee, 1978). Goals have more recently been brought inside; they are internal mental entities or processes. It is this shift that has created the theoretical issues discussed in this chapter.

THEORETICAL PROBLEMS

Because many readers who anticipate a critique of mental concepts will immediately free-associate to the label "behaviorist," some clarification in that direction is needed. I distinguish between substantive and methodological behaviorism by separating *what* a theory may include from *how* a theory is to be examined empirically (Bavelas, 1978, chap. 19). In reaction against the introspectionists of the early part of this century, substantive behaviorists such as Watson and Skinner aspired to exclude all nonobservables from their theories (e.g., "motivation" was eliminated in favor of "hours of deprivation," and "learning" was replaced with "prior reinforcement history"). The substance of the theory had to be behavioral. A gentler reaction was that of theorists such as Tolman, Hull, or the Gestalt psychologists (Wertheimer, Koffka, Kohler). In these theories, the explanatory concepts could be nonbehavioral, but they had to be capable of being translated into behavioral methods permitting observable empirical tests of the theory. Most of us are the heirs of these methodological behaviorists, free to generate theories and define concepts as we wish but obliged at some point to find "operational definitions" of concepts and to take these into the sobering domain of observables. It is safe to assume that theorists who are interested in goals are methodological, not substantive, behaviorists. I am not advocating that they become substantive behaviorists; rather, I am describing problems that are inherent in—but by no means insoluable for—methodological behaviorism that includes mental constructs. (Different criteria would be applied for the different problems inherent in a substantive behaviorist position.)

In a classic article, MacCorquodale and Meehl (1948) pointed out some of the obligations incurred by theory builders who choose to use concepts that are not, in themselves, observable. They pointed out that there are at least two kinds of such concepts: First, there are *intervening variables,* which are rather Spartan concepts that "merely abstract the empirical relationships. . . . the statement of such a concept does not contain any words which are not reducible to the empirical laws" (pp. 106–107). The second, more ebullient, *hypothetical constructs,* go much further:

> [they] involve the supposition of entities or processes not among the observed. . . . Concepts of the second sort . . . [involve] words not wholly reducible to the words in the empirical laws; the validity of the empirical laws is not a sufficient condition for the truth of the concept, inasmuch as it contains surplus meaning; and the quantitative form of the concept is not obtainable simply by grouping empirical terms and functions. (pp. 106–107)

Examples from other sciences are "gravity" as an intervening variable and "evolution" as a hypothetical construct.

Obviously, most of the mental concepts used in our theories are hypothetical constructs. The problem is that they have many *surplus meanings*—connotations beyond the empirical measures used in any particular study. They are, in Underwood's (1957) phrase, more an "artistic or literary conception" (p. 55) than a scientific specification. We cannot underestimate the role of the theorist's subjective experience (and his or her appeal to others' subjective experience) in theories that invoke hypothetical constructs, including goals. In our own consciousness, we are aware ourselves of having goals, so we do not question the use of the term; its surplus meanings may even be an advantage in its initial acceptance. The same surplus meanings, however, can create a drastic imbalance between the hypothetical construct as used in the theory and the particular method of its measurement, which is usually much more specific. At worst, there is an inverted pyramid with a large theoretical and conceptual superstructure supported by a narrow data base. The danger is greater the more appealing the construct, because creative speculation and generalization will certainly exceed the empirical base.

(The majority of social scientists seem to treat theories with intervening variables as mere descriptions, rather than proper theories. "Black-box" theories [e.g., Watzlawick, Beavin, & Jackson, 1967, chap. 1], which deal solely with input–output relationships or patterns of behavior, are often seen as incomplete because of an unacknowledged and therefore unquestioned assumption that explanations of behavior must be mental.)

When goals are invoked to explain the production, comprehension, or patterning of discourse, another problem arises. Not only must the concept of "goal" be defined explicitly and clearly, but a model of *the process by which goals are connected to discourse* must be explicated. Just as we all "know" what a goal is, we can all imagine how they affect discourse; intuitive plausibility becomes a disadvantage in the longer term, because the "mere formalities" of tight theoretical connections are likely to have been neglected. For example, a minimum theoretical specification is whether goals operate consciously on discourse ("I have this goal; therefore, I will talk this way."). If so, is this awareness verbal? Do subjects' open-ended self-reports confirm the model? The theory need not equate awareness with verbalizability; there are cognitive models that tap unverbalized cognitive processes and test these by ingenious techniques. But some such model must be chosen and tested.

If awareness is not invoked, another route must be proposed. Moreover, the theorist in this case cannot appeal to introspective experiences, even in examples, when elaborating the theory. It is quite striking how often scholarly discussions of important theoretical issues are advanced by personal authority, such as anecdotes or appeals to how we (the scholars) experience the world. It is as if our standards for a theory are that it describe the world as a group of social scientists see it and that it conform to anecdotes that those social scientists can adduce in support of it. This is *not* the same as a legitimate phenomenological or ethnomethodogical approach because of the inordinate weight given to the opinions of a small group of scholars.

The possibility of multiple goals presents a further requirement, namely, that a subsection of the theory must describe the nature of the *interaction between goals.* Lewin (1938) had such a model, which included detailed predictions for the process and outcome expected when goals conflict. As I describe later, our Victoria group adapted Lewin's model to discourse in the face of conflicting goals (Bavelas, 1983, 1985; Bavelas, Black, Chovil, & Mullett, 1990a, 1990b) and obtained substantial empirical confirmation. The way in which the concept of goal was defined in this work may not match the definition or interests of another researcher, but one lesson is clear: The process was, as it should be, far from a dreary duty. The details that link goals to each other and to discourse should be of great intrinsic interest to the theorist who proposes such a connection. He or she should regard the requirement for details as a welcome one, permitting intimate exploration of a chosen territory. The most interesting questions should be: How would we know that goals affect discourse? What consequences would this lead to? What difference would it make?

A final major theoretical problem is that the essential nature of a mental goal, however defined, is *monadic:* It refers to some process, disposition, motivation, or awareness in an individual. Yet, if such goals are then connected with face-to-face *interaction,* a fundamental disparity of units arises. Goal as a construct located in an individual mind might explain monologue, but even the cleverest and bravest reductionist does not have the alchemy to produce the creative spontaneity of dialogue out of two goals, in separate minds. The problem, of course, is a general one, not limited to theories about goals. Any intrapsychic model of dyadic (or group) discourse faces a chasm between the mind of an individual on the one side and the behavior of a social unit on the other. This does not mean that the problem is insoluable, just that it exists and is too little recognized. Mentally driven theories can hypothesize a start to the interaction, but they must also account for the reciprocity and accomodation that characterize face-to-face interaction. Otherwise, the goals of the two individuals would run parallel, never affecting each other. (The nonmentalistic alternative is to shift the level of analysis to dialogue as a social system whose pattern is its own explanation.)

So far, I have sketched out some minimal requirements for theories that link goals to discourse. Even so, it may seem like a great deal to ask of a theory even before it moves on to data. Perhaps this suggests a more modest course, in which a narrower but specifiable conception of goal is linked to an equally limited aspect of discourse—spending within one's means, so to speak. The alternative is to spend on credit with a broadly defined construct without paying the theoretical or empirical bills.

EMPIRICAL PROBLEMS

The most serious evidential risk for theories that invoke internal constructs is *hidden circularity.* For example, goals determine the discourse, and the discourse is the evidence of the inferred goals; there is no independent evidence for the extra conceptual baggage being carried on board. It is crucial to provide independent, collateral evidence for the concept invoked. To do so, it is necessary to explicate the theory enough that it is possible to say: If goals are driving behavior in this particular way, then when X happens, Y should follow. In other words, the hypothesis must be falsifiable; it cannot account for all conceivable outcomes without being meaningless. Circularity can be found remarkably frequently in theories of mental processes, because the more complex the theory, the harder it is to identify potential circularities. Indeed, the broader the theory appears to be in its application, the more likely that this breadth is being bought with hidden circularity. Even the substantive behaviorists are vulnerable on this point: Why did a behavior occur? Because it was reinforced. How do we know the behavior was reinforced? Because it occurred. To assume that behavior is goal-driven risks the same trap. This problem can be diagnosed, first, by explicit logical formulation and, second, by early attempts at falsification.

All of this implies a pressing methodological necessity to *identify goals empirically,* whether by experimental manipulation, indirect measurement, or subjects' reports after the fact. There are, however, obvious dangers with the last approach. If we provide subjects with our terminology, it may not be possible for them to reply in a disconfirming manner. Too often, the researcher assumes that the subjects have goals and only asks about which ones. This is like offering a ballot with several candidates, but all from the same party. It might seem that this problem could be avoided by manipulating goals or measuring them indirectly, but it must, of course, be established that what was manipulated or measured was really "goal," that is, that there is no alternative interpretation of the independent variable. A tight definition of the term will make all of these tasks possible.

A related issue is the problem of differing operational definitions. If various researchers use various methods for studying goals, are they all really studying the same thing? *Convergent operations* (a variety of methods for measuring the same concept; Campbell & Fiske, 1959) are important, because they establish the breadth of the concept being measured. But this is only true if the measures are used *together* in the same studies often enough to show that they are functionally similar. That is, in the desirable form of multiple operations, measures of the same concept correlate with each other in the same setting. It is quite a different matter when different measures are used (alone) in various studies. In the latter case, we have no evidence that the researchers are, in fact, studying the same concept. Conceptual and operational breadth are assumed but not demonstrated. The remedy in this case is straightforward: Researchers with different measures should include each other's measures in their studies.

Finally, there is the anomaly of labeling some discourse, namely, *methodological discourse,* as not-discourse. When a researcher asks subjects about their goals, whether by interview or questionnaire, this interaction is also discourse; yet it is often seen, instead, as a direct route to the mind. I do not wish to raise here the specter of recursiveness and self-reflexivity; the fact that subjects' replies are discourse does not make them invalid as a source of information. It does mean that we should remain attuned to the context in which the replies are made, instead of treating them as context-free "truth."

ALIENATION FROM DISCOURSE

It is essential to keep in mind the fundamental *difference between (mental) goal and discourse:* One is a construct, the other is observable behavior, and some strange things can happen when they are juxtaposed. Goals in the sense of hypothetical, intrapsychic entities cannot occur *in* discourse. They can affect discourse, or they can be inferred from discourse. To the extent that we begin to "see" goals in discourse, we have pushed discourse aside and replaced it with an inferred construct. We have lost track of what is observed and what is inferred and have begun to believe in the literal reality of hypothetical mental constructs. This reified construct becomes what we think we are actually seeing, rather than the discourse.

Even when the distinction between goal and discourse is maintained, discourse is sometimes pushed aside as *"merely discourse."* In other words, there is often a kind of elitism in favor of constructs, in which goals are considered to be at a "higher" (i.e., theoretical) level, while the actual discourse is at a "micro" level—small and particular, only a means to a higher end. That is, discourse is of interest to the extent that it is a path to the construct; the particulars are not as important as the generality. This Platonic principle rejects the particular instance in favor of the idealized type. In my view, the importance of the emergence of discourse analysis was the legitimization of discourse itself as intrinsically interesting. To the extent that we favor explanatory constructs and mental models over discourse, we have stepped back into more traditional ways and left discourse analysis. It is true that researchers should always seek generality, but not simply by the use of general words. The general term *goal* does not have any real generality until it can be shown *empirically* that many particular instances of discourse can be explained or predicted by this construct.

One way of summarizing several of these problems is to return to the pyramid image. Do we want an inverted pyramid with a very small data base supporting

a highly elaborated theoretical superstructure or a stable pyramid with a large observational base supporting modest conceptual inferences? The first is tempting for several reasons, of which two should be emphasized: First, in many quarters, theory building is seen as more elegant, as a more important contribution, than mere observation. Second, the words with which theories can be built are more malleable than data, which can always prove us wrong. We may be able to make words mean anything we want, but data are not usually so cooperative.

My opinion about how we should proceed is probably obvious. We should be more like our natural science colleagues: Biologists, chemists, biochemists, astronomers, and the like pursue problems presented by phenomena; they describe the world first and then try to aggregate these descriptions into theoretical models. To me, the varieties of discourse are like the fauna of our planet—there to be examined, grouped, classified, and explained. The inner workings will come from intense observation and cautious inference.

BUILDING A STABLE PYRAMID: AN EXAMPLE

It is one thing to have an opinion about how research and theory should proceed; it is another thing actually to do it. In a particular project, our everyday choices are affected by many immediate factors that are never mentioned by nonscientist philosophers of science or by theory experts who do not do research. Our long-term project on equivocation (Bavelas et al., 1990b) illustrates these real-life pressures and confusions.

My interest in ambiguous, evasive, odd messages began when I was working with the Palo Alto group in the 1960s. We frequently noticed "disqualified" or "incongruent" communication in the families of schizophrenic patients and saw this as an important part of the situation with which the schizophrenic must cope. An example (from Sluzki, Beavin, Tarnopolsky, & Veron, 1967) is:

Adult son: You treat me like a child.

Mother: But you are my child. (p. 498)

The play on the word "child" seems not playful here but deadly serious, yet there is something perversely elegant about it. It is possible to see it, on the one hand, as a smooth, reasonable transition but, on the other hand, as a malevolent mystification by which the patient's meaning is taken from him and shaped into something else, or (on yet another hand) as mere simpleness or excessive literality.

Such messages kept occurring, and I kept being fascinated by their resonances and multiple meanings—but troubled by something else. Once I tuned in to them, I did not hear these equivocal messages solely from the families of schizophrenics, whom we were studying, but from my own friends and family and from all kinds of apparently "normal" people (including myself). Fortunately, so did my colleagues, and we were able to resist the temptation to label such communication as pathological, or pathogenic, and to see its more general import.

One clue was the observation (Sluzki et al., 1967) that, while the family puts the patient in an impossible position by their communicative style, so does the schizophrenic patient: Analyzed in the same way, his or her messages are equally problematic for the family. This insight was greatly assisted by a well-intentioned project, carried out very naively, in which we sought normal, control families with which to contrast and understand communication in schizophrenic families. In these interviews, we did *not* find the clear, straightforward, congruent standard of communication we expected but rather a good deal of incoherent, tangential communication. Fortunately, rather than concluding that these normal control families were undiagnosed "schizophrenogenics," we took seriously our commitment to the situation as an explanation and asked ourselves, what was the situation for these families?

In brief, they had been asked by their family doctors to be interviewed by a famous psychiatrist because they were normal. What a responsibility! What a set-up! They dared not make a mistake: They could be neither unhappy nor unrealistically happy; they could neither admit to problems nor say that they had no problems; they had to be perfectly honest, natural, and, above all, *normal.* As they tried to thread their way through this mine field (which we had not intended), they sounded very strange indeed (e.g., Watzlawick, 1964; Watzlawick et al., 1967, pp. 76–78). So, by the mid-1960s, we at least understood that such imperfect communication was the product, not of an imperfect mind, but of an impossible situation, a situation in which no direct response would be satisfactory.

Although I then left clinical research for an academic career, these "bad" communications that were not really bad did not entirely fade from my mind. About 10 years later, during an individual study course with a mature student who had adult children, I was challenged to address the issue of always blaming the parents (of schizophrenics or anyone else), and I finally had to take my beliefs seriously: If communication is situational, then it is necessary to give up blaming. If the schizophrenic is not defective but only reacting to the subtle complexities of his or her situation, then so are the parents and family. It cannot be that different laws operate for one side than for the other.

Thus began the "disqualification project." Starting in 1977, we sought to identify the situations in which perfectly normal people would choose or produce messages that were considerably less than perfect. After first finding a method with which to identify and measure such messages (Bavelas & Smith, 1982), we began to list the many everyday situations that leave no alternative

other than unclear communication. The result was a list of "binds" in which all communicative alternatives seemed impossible, yet communication was required. For example:

- You have to write a thank-you note for an awful gift from a well-liked friend or relative.
- You are asked for a letter of reference about a friend who was an incompetent employee.
- Two friends who disagree intensely about an issue ask you for your opinion on it.

In all of these cases, it is necessary to reply, yet all of the direct response options would have bad consequences. A disqualified response, which "says nothing while saying something" avoids the worst consequences. At this point, there was nothing worth calling a theory, just a notion about normal, transient, "benign binds" (as opposed to double binds; Bateson, Jackson, Haley, & Weakland, 1956).

Fortunately, I happened to describe our work to Professor Tamara Dembo (who had trained and worked with the German Gestalt school and also with Kurt Lewin). She suggested immediately that our situations were, colloquially, ones that involve "tact" and, more technically, appeared to be avoidance–avoidance conflicts. At last, it was possible to make more than a descriptive statement:

> A bind in our terms is an *avoidance–avoidance conflict,* in which two unappealing choices repel the individual, who will leave the field if possible—in this case, communicationally, by evasive or indirect communication. . . .
>
> Three premises can be applied to the case of conflict: (1) Situations are represented as valences [i.e., goals] attracting or repelling the person, that is, as eliciting approach or avoidance. (2) The force of the valence, whether positive or negative, is stronger if closer; this is the "goal gradient": A positive valence becomes more attractive as one approaches it, and a negative valence becomes more repellent as one comes closer to it. (3) There is a force or tendency towards movement—either the valences vary slightly, though randomly, or the decision region itself becomes negative. (Bavelas, 1983, pp. 138–139)

This theory accounted for previous results and predicted new ones, for example, that approach–approach conflicts (with two positive goals) would *not* elicit evasive communication. The social consequences of messages became goals with valences that could be positive or, more interestingly, negative. These external goals induce a psychological decision process that includes tension, vacillation, and finally, resolution.

Now we were at a crucial point in theory development. Lewin's theory is very appealing but also highly elaborate. It is a comprehensive theory of personality (cf. Lewin, 1935; Bavelas, 1978, chap. 17), and the conflict portion alone is a monograph (Lewin, 1938). Because Lewin was unusual among personality theories in including the interaction between the individual and the environment, his theory could be applied, more readily than most, to communication. However, the basic features of his theory were definitely intrapsychic; overt behavior was of interest because of what it revealed about intrapsychic processes. Even external features of the environment (such as goals) became part of the psychological life space and had their influence primarily by creating internal tensions toward resolution. We had no data on these internal tensions or, indeed, on any psychic structures or processes, and—being more interested in discourse—we were not keen to turn our attention to them. In short, had we adopted the larger structure of Lewin's theory, we would have created a classic inverted pyramid. It was clear from many colleagues' reactions that this would have been a popular choice, but, partly because a minority of other colleagues (such as Edna Rogers) urged us to "keep the faith," we began to back away from a full-fledged Lewinian theory and take another direction.

Essentially, we took an intervening variable approach. We explicitly labeled our theory as an *adaptation* of Lewin's and stripped it down to the bare, observable essentials (Bavelas et al., 1990b). We put goals back out there in the social environment, as consequences of message choices, and spent most of our time seeking equivocations (as we came to rename them) in the widest possible variety of situations. *Varied replication* became our main interest, because we wanted to show that our limited theory held firmly for its domain. We conducted avoidance–avoidance experiments with subjects writing their own messages (Bavelas & Chovil, 1986). Then we did the same with subjects responding on the telephone or face-to-face, in a total of about 14 different hypothetical scenarios; in some of these, we distinguished empirically between equivocation and deception (Bavelas et al., 1990a). The opportunity arose to do a field experiment that created a "real" conflict for politicians (Bavelas, Black, Bryson, & Mullett, 1988) and also to conduct some purely observational field work with politicians and reporters, with the aim of extending our theory into this particularly equivocal dyadic interaction. In other words, we sought generality by extending the data base for our relatively simple theory.

The difference between the route we took and the one we almost took became sharply clear to us when we examined a particular aspect of our data, namely, *latencies* (Bavelas, 1985, p. 205; Bavelas et al., 1990b). An avoidance–avoidance conflict is created for our subjects when they are actually asked the experimental question (e.g., "How do you like the gift I sent you?"). When the experiment is conducted with spoken (rather than written) communication, it is possible to measure response latency, which is the time between the end of the question and the beginning of the subject's reply. It is well established in Lewinian theory (Barker, 1942) that avoidance–avoidance conflicts produce

longer latencies than do approach–approach conflicts or nonconflict conditions. According to the theory, this is because of vacillation between the two alternatives, caused by the goal gradient. Imagine that you are the subject and are considering a brutal truth ("I don't like the gift you sent"). As you come psychologically closer to saying this, it becomes more negative, so you change to the other possibility, a lie ("I love the gift!"). But coming closer to that alternative inevitably makes it more negative (and the first choice less so), so you reverse again. Even though this would happen very fast, it should take measurable time, producing a longer latency, and indeed, this is what we obtained. In all such experiments, the latencies were in the predicted direction, and, in most cases, the difference was statistically significant. These "empty moments" are, in the full Lewinian theory, envelopes that hold the intrapsychic process of vacillation, and they are probably as close as we will ever come to seeing this process. It was very tempting to interpret them as such.

By this time, however, we had a rule of always looking for a discourse-focused interpretation when tempted by an intrapsychic one. This was partly to maintain theoretical consistency but equally to force ourselves to see new phenomena rather than just more instances of old theories. The alternative explanation for the latencies was suggested mostly by the lay judges who scaled our messages for equivocation. They noticed the pauses and interpreted them *as part of the message.* Hesitating before saying something negative is a way of encoding reluctance. Someone who unhesitatingly told a friend, "You look awful," would seem eager to hurt that friend, whereas appearing to have it dragged out of him or her conveys, "I hate to say this but. . . ." As a 19th-century writer observed, "Well-timed silence hath more eloquence than speech" (Tupper, 1854, p. 90).

In the end, what we say about equivocation is detailed but limited, compared to many other theories, but we are very confident about its empirical validity and replicability. On a personal note, that kind of solid confidence feels good—not as heady as high-flying speculation but more lasting.

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