Western Journal of Speech Communication, 54 (Fall 1990), 593-602

FORUM:
CAN ONE NOT COMMUNICATE?

Behaving and Communicating: A Reply to Motley

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Two propositions that have been treated as equivalent ("All behavior is communication" and "one cannot not communicate") are separated, on logical grounds, into two separate questions: "Is *all* nonverbal behavior communicative?" and "In an interactional setting, is there always *some* communicative behavior?" I suggest that both should be treated as hypotheses, not axioms, and outline empirical tests for both. The essay also specifies agreement and disagreement with other points in Motley's (1990) article.

I WOULD LIKE TO SORT OUT some of the tangle of meanings derived from our "first axiom" in *Pragmatics of human communication* (Watzlawick, Beavin, & Jackson, 1967, pp. 48-51) and to propose a framework that permits these issues to be settled empirically. In the process, I will give some of the historical context in which that axiom arose and will describe my current view, which is that all behavior is *not* communicative, but that one probably cannot avoid communicating in a social setting.

First, it is essential to separate two statements that were entangled then and have continued to be treated in the literature as equivalent. The formal statement of the axiom, "One cannot *not* communicate" (p. 51) has been treated as identical to another statement from which the first was informally deduced, "all behavior in an interactional situation has message value, i.e., is communication" (pp. 49-49). These statements are not even approximately the same, because—to use the language of formal logic—one has a universal quantifier while the other has an existential quantifier (Quine, 1959). That is, the statement, "All behavior in an interactional situation . . . is communication" is a universal statement ascribing communicative properties to *all* behavior occurring in

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such a situation. On the other hand, "One cannot not communicate" means that, in the presence of others, *some* behavior is communicative. If the first were true, then the second would have to be as well, but if the first were false (i.e., all behavior is not communicative), the second could still be true. They will be treated separately below.

NONVERBAL BEHAVIOR VERSUS NONVERBAL COMMUNICATION

To understand why we (and others) proposed to treat all behavior in an interactional situation as communicative, an historical context must be re-constructed, however sketchily. Since the origins of formal rhetoric in Classical Greece, communication had been equated with effective public speaking. Implicitly, communication was limited to *planned, formal, persuasive* acts. In the 1950's information theory provided a broader definition, namely, the transmission of information by use of a shared code. This approach took communication out of the forum and into any other setting in which a source used a transmitter to send an encoded message over a channel to a receiver to be decoded at a destination (Miller, 1951, p. 6-8). Powerful as this model is, it too set implicit limitations on what could be considered communication: Explicit, logical material, produced by a deliberate, formalized encoding process, and leading to successful mutual understanding.

Those of us fascinated by interpersonal communication did not find this definition congenial. We were interested, as Weakland (1967, p. 1) described, not in communication as it "should" be but in communication as it was actually occurring between people. Information theory seemed good for handling communication that was

- *mediated* (e.g., electronic) but not face-to-face;
- *goal-oriented* (e.g., exposition or narrative) but not phatic (e.g., small talk);
- *organized* (e.g., debate) but not naturally occurring (e.g., conversation);
- *formal* (e.g., speeches) but not informal (e.g., gossip or slang);
- *deliberate* (i.e., planned) but not spontaneous (ad libs or joking);
- *logical and clear* but not illogical or ambiguous (e.g., equivocation);
- *explicit* but not implicit (e.g., indirect speech acts);
- *successful* but never unsuccessful or frustrating;

and most of all,

- *verbal* but not nonverbal.

The last exclusion was very important to us. All verbal behaviors had at least the possibility of being seen as communicative (although often as "poor" communication), but nonverbal behaviors were not even second-class citizens. By the criteria of the 1960's, nonverbal behaviors could not be considered communicative unless they had the above characteristics, including intentional encoding and successful decoding. At the time, the only means by which such mutual understanding

could be established seemed to be introspection (Watzlawick et al., 1967, p. 49). Yet our very goal was to propose an alternative to intrapsychic models! Given the choice between abandoning nonverbal behaviors and accepting an introspective criterion, we chose nonverbal behaviors, brashly enfranchising all of them. (I have never regretted this youthful excess, first, because I will always choose what I am observing over what I am told I should be seeing and, second, because research requires taking positions, being wrong sometimes, and changing.)

In 1972, Wiener, Devoe, Rubinow, and Geller wrote an incisive criticism of those (including us) who had equated nonverbal behavior with nonverbal communication. Their bases for distinguishing between nonverbal behavior and its subset, nonverbal communication, were essentially the same as Motley's (1990). I was, and remain, completely convinced by their distinction between *informative* and *communicative* acts (i.e., behaviors with "message value" may be informative but not communicative). I agree also that by calling all behaviors communicative, we had imposed a "receiver bias" that necessarily–but ironically–led to the imputation of "unconscious" intention. However, their methodological recommendations were disappointing, because they reinstated the old criterion of intentionality. (It is amazing to think how long we have let ourselves be dominated by a concept that its proponents admit they cannot adequately define or measure.) Thus, the times have cast the study of nonverbal behavior adrift. Even when called nonverbal communication, it is not usually treated (by communication researchers, much less by linguists) as *real* communication. Rather, it is treated as a separate and considerably lesser communicative "channel," studied for what it reveals rather than for what it conveys.

For our Victoria group, the breakthrough came when we began studying motor mimicry in the early 1980's. Motor mimicry (such as wincing at someone else's pain) began to seem very communicative to us, specifically an eloquent, analogically encoded relationship message about caring and involvement. We needed to develop a method for testing whether motor mimicry was a nonverbal communication or a nonverbal behavior *without requiring that intentionality be established.* Put simply, the reasoning we used was that "if it looks like a duck and walks like a duck, then it's probably a duck; you don't have to ask it." More formally,

> We propose that a strong prima facie case can be made by experiments focused on sender-receiver variables; that is, if motor mimicry is communicative, then (a) the probability of its being seen should affect the sender's display of facial mimicry, and (b) receivers should make consistent interpretations of such displays. (Bavelas, Black, Lemery, & Mullett, 1986, p. 323)

We went on to establish that motor mimicry was very precisely controlled by the sender's eye contact with an available receiver and that it was systematically decoded by naive receivers. In subsequent studies (Bavelas, Black, Chovil, Lemery, & Mullett, 1988), we demonstrated

that, of otherwise equivalent possibilities, the form in which senders spontaneously enact motor mimicry is the one decodable by receivers. Chovil (1990b) varied physical channel availability (e.g., face-to-face versus telephone) and found that facial mimicry occurred only when it would be seen.

We are now (Bavelas and Chovil, 1990) generalizing this method for identifying nonverbal communication. Even by a stringent criterion definition ("transmission of information by use of a shared code"), there are already several studies available in the literature that have successfully demonstrated that some surprising nonverbal behaviors are nonverbal communication (e.g., facial "disgust" in reaction to 110% salt-saturated sandwiches was displayed only when there was another person present; Brightman, Segal, Werther, & Steiner, 1975).

McNeill (1985) has presented different empirical criteria and evidence for demonstrating that gestures in particular are not only communicative but linguistic. For example, different kinds of gestures disappear in different kinds of aphasia, namely, those gestures with linguistic functions similar to the specific verbal loss. Chovil (1990a) has also shown many linguistic functions served by facial displays.

There are without doubt innumerable nonverbal actions (e.g., self-adaptors such as scratching a mosquito bite or object-related actions such as picking up a mug of coffee) that will be demonstrated by these methods not to be communicative. But *some* (many) nonverbal behaviors can be demonstrated empirically to be communicative in the fullest sense of that term, and I see this as an exciting new research area.

HOW'S NOT TO COMMUNICATE?

It is, as noted at the outset, an entirely different logical proposition that, in the presence of others, there will be some communicative behaviors ("One cannot not communicate"). Our main application of this axiom was to the phenomenon of *disqualification* (1967, pp. 72-78), which we proposed was an effort to "say nothing by saying something" when the situation made direct or clear communication impossible. We began testing this specific hypothesis in the early 1980's (Bavelas, 1983) and now have extensive experimental evidence for it (e.g., Bavelas, Black, Chovil, & Mullett, 1990).

The second application of this axiom in *Pragmatics* was what might be called the "minimalist" hypothesis:

> The man at a crowded lunch counter who looks straight ahead, or the airplane passenger who sits with his eyes closed, are both communicating that they do not want to speak to anybody or be spoken to, and their neighbors usually "get the message" and respond by leaving them alone. (Watzlawick, et al., 1967, p. 49)

In other words, the mere presence of others may require the individual to communicate about his or her relationship with them, even if that relationship is overtly going to be one of noncommunicating strangers.

N.B.: This does not mean that *all* of the individual's behaviors are communicative, just that *some* must be.

I think this second hypothesis, too, can be empirically tested, although it will be more difficult, because it needs both further refinement and appropriate research methods. What constitutes "the presence of others" and how minimal can communication get? How can we as researchers have access to data from putatively non-communicating individuals?

Some members of our research group have begun attacking these problems. Mullett (1986) videotaped the first few seconds of behavior occurring when one stranger walked into a room with another stranger, in three different experimental conditions: (1) They were going to work together on a task; (2) they were going to work separately, under no time pressure; or (3) they were going to work separately and were already under time pressure. Mullett assumed that the participants would easily infer that the first condition would require interaction; the last would preclude it (because it would be a distraction, and they would fail at their tasks); and the second would fall in between. The question was, how would they get this across to each other? She found significant and very precise differences in several nonverbal behaviors (such as linear changes in latency and duration of eye contact and of smiling) consistent with this prediction. In less than 15 seconds, the two people signalled differently depending on the relationship they had to establish. In the third condition, this relationship was "no communicating, please," and that is what they successfully communicated to each other.

I think that a wide variety of settings could be studied with this strategy (which is quite like the Luft, 1962, study originally described in *Pragmatics,* p. 49): Impose non-communicative conditions on strangers or non-strangers, and look for patterns of nonverbal behaviors, that is, choices of some behaviors rather than others. If patterns emerge, these may be communicative. Confirmatory evidence could then be obtained by turning to the sender-receiver criteria described earlier. For example, vary the availability of visual communication; if no facial communication could be observed by the other, there should be "channel-switching" (e.g., rigid posture, sighing or "groaning" in concentration, etc.). Later, the decodability of the behavior, as judged by the other person's reactions and by outside observers, should be tested.

In Mullett's task, the participants in the key third condition were not forbidden to communicate; they simply had to convey quickly that they were not going to do so thereafter. The empirical task becomes more challenging when the person cannot appear to be communicating even that message. Bryson (1985) described "explanatory behaviors," whereby individuals solve the Goffmanesque problem of making their public behavior explicable without breaking the rule of not addressing strangers. For example, while standing alone in an odd place, an individual may glance at her watch frequently, in order to convey—without

appearing to do so—that she is waiting for someone. We reasoned that such a "smuggled message" would differ from real time-telling by its stylization, that is, by changes in form necessitated by decodability rather than by its ostensible function. In a pilot study, Bryson asked people to walk into a room and turn to look for 10 seconds at each of four cameras (in the corners). When a stranger was studying in the room, the behaviors were qualitatively different (more stylized) than when the room was empty.

Another promising lead is Lawrie's (1988) proposal that *gaze avoidance* is an important means of communicating to strangers that one is not going to interact with them. For example, Kendon (1967) paraphrased Goffman's (1964) proposal that

> *where* an individual is looking is an important indicator of his social accessibility. . . . Whether or not a person is willing to have his eye "caught". . . is one of the principal signals by which people indicate to each other their willingness to begin an encounter. (Kendon, 1967, p. 23, italics added)

In other words, we look not just to see but *to be seen looking.* So the individual who rigidly "looks straight ahead" may be selecting and enacting a behavior from an established code, one that says, however paradoxically, communication is not occurring. Motley proposed that noncommunicative acts such as "spontaneous yawns, stomach growls, and facial blushing"[1] occur on their own, as a result only of their "natural instigators" (1990, p. 7). There is no natural instigator to unbroken gaze at the display panel in an elevator, so we can hypothesize that this choice is made in order to indicate that one will *not* look at the strangers on board. Empirically, this could be studied by comparing eye movements in overtly non-communicative settings with those where communication is appropriate and also with the pattern of eye movements when an individual is alone in the same setting.

Notice that the last two suggestions explicitly question Motley's (p. 10) implication that there can be completely "noninteractive episodes" in the presence of others. We hypothesize that the mere presence of others is a constraining state for humans; they must indicate their degree of availability for communication. Stated this way, we have a horse race: Data can be gathered that would support one position or the other, and we would all be more informed for the effort. My research proposals can only be suggestive, because these are newly emerged questions and there is little so far to guide us. However, they raise important issues worth investigating. We should now refine both hypotheses (minimal communication and noninteractive episodes), test them, and decide which to accept.

COMPARISON WITH MOTLEY'S POSITIONS

The reader will have noticed by now that I agree with some aspects of Motley's (1990) article and not with others. First, I agree with and

am grateful for his criticism of "blind acceptance" (p. 13) of *Pragmatics,* as well as his implication that the authors are not responsible for everything said or done in its name. My own view of several of the "axioms" has been changed both by logic and by data over the past 25 years, and I would be very worried about my scientific practices if this were not so. What is important is not to be right all of the time but to be open to seeing what people are really doing when they communicate.

Motley stresses the importance of consistency and of constantly examining exactly what we are saying, and again I agree heartily. It is essential to question everything, especially one's own position, to strive toward articulating a clear and consistent position, and to examine that position both logically and empirically.

I also find the first three "traditional postulates" (emphasizing symbols, encoding, and interactive process) to be a fruitful and interesting way to conceptualize communication. In contrast, Skinner's (1957) treatment of communication as mere "verbal behavior" does not, in my view, help us see its richness. Emphasizing the interchange of symbols is a better starting point, one that lets us see new phenomena. It is important to bear in mind, though, that "traditional postulates" are only chosen perspectives and not eternal truths. They may lead us to confirmable hypotheses, but they are not themselves confirmable. (As will be seen below, I disagree passionately about the utility of the fourth, "fidelity" postulate.)

As for "all behavior is communication," I thought that this error was only in the literature for five years before being corrected by Wiener et al. (1972). I accept their insight that a "receiver only" perspective is not consistent with a symbolic encoding definition of communication and only obscures our thinking. All behavior is not communicative, although it may be informative, and Motley's "receiver/perceiver" distinction (p. 10) is a good way of putting this.

Finally, although we are obviously on different sides of the issue, Motley and I agree about the continuing importance of the notion of "intention" in defining and conceptualizing communication. Our research group has dealt with it by finding an alternative, empirically testable criterion; I will suggest below that proponents of "intention" have a similar task before them.

The first disagreement is one I share responsibility for, namely, the failure to distinguish between "all behavior is communication" and "one cannot not communicate." The distinction has been clear to me for several years, but I never took the time to say so in writing. I assume that there must be several swifter people who noticed it much earlier and probably said so in print or at a conference, but obviously neither Motley nor I knew of them.

Given this distinction (however late in the day), it becomes obvious that there is no contradiction between the four "traditional postulates"

and "one cannot not communicate." Indeed, a nonverbal message that limits the degree of communication in a situation is a very sophisticated, interactive, symbolic code. That does not mean that the "minimalist" hypothesis is true; it just means it is consistent with a view of communication with which I agree.

However, especially because he was not making the distinction, Motley's focus should have been entirely on nonverbal behavior. Because the instances in which verbalization might be considered noncommunicative are infrequent and trivial, his main thesis comes down to showing that all other (nonverbal) behavior is not communicative. Yet he ignored significant literature in the area (e.g., Wiener et al., 1972; McNeill, 1985) and did not commit himself to a principle by which this central issue could be resolved: how to establish when a nonverbal behavior is or is not a nonverbal communication.

I have major disagreements with Motley's methodological position. Science, in my view, is practiced through a constant effort at clear statements of coherent positions, which are rigorously examined both logically and empirically. I happen to do primarily experimental work, but I admire any good research that teaches me something—whether conducted in the field, in the lab, by ethnography, ethology, or experiment, qualitatively or quantitatively, with or without statistics. It seems to me that Motley consistently shied away from the possibility of empirical tests of his or others' ideas. Instead, he relied on four consensual "truths" from undergraduate texts—a standard that simply cannot be accepted as a reasonable criterion for theory development.

Another problem I have with Motley's method is his failure to define concepts so that they can be falsified. As I have discussed in detail elsewhere (Bavelas, 1990), it is perfectly reasonable to propose such intuitively appealing concepts as *goal* and *intention,* but the proponent takes on clear definitional and empirical obligations in doing so. If these obligations are not met, then there is the strong possibility that the apparent utility of the concepts is due solely to their slipperiness: Tautological concepts with surplus meanings and no empirical referents will always be "right"—and therefore meaningless. It is no loyalty to such concepts to leave them in this state.

I question the viability of Motley's goal-driven model of communication on the grounds that it cannot account for the rapidity and complexity of face-to-face interaction. If a communicative goal led to cognitive processing, which led to decisions and subplans, preparation of a message, encoding, and so forth, the sender would be lost in thought. In fact, microanalysis reveals that each speaker coordinates and responds to the other person with an integrated verbal and nonverbal message *in less than simple reaction time;* that is, complex cognitive processing as presently theorized is not possible. Dialogue is the fastest and most complex human activity, and even the most banal conversation leaves current cognitive models in the dust. Rather than downgrading

the phenomenon to examples such as "Pass the salt," it is the researcher's duty to upgrade the model to handle the full phenomenon plausibly. (And labelling the process as "unconscious" only sweeps it under the rug; if a theorist proposes "unconscious intentions," he or she must be prepared to tell us how they work.)

Finally, I reject the "fidelity postulate," which I read as a bias about what constitutes "quality." To me, it comes across as global, judgmental, and based on imposed ideals of persuasive speech and mediated communication (Motley, p. 11) that have historically dominated our definitions of communication. Just to stir up a new hornet's nest for the next 25 years, I propose that it is meaningless to say that anyone communicates poorly. *What we do mostly is face-to-face communication, and the most ordinary of us do that elegantly, precisely, rapidly, and with great subtlety and complexity.*

What I have learned from my experiences with all of these issues over almost three decades are two broad guidelines: First, we should not declare phenomena to be "poor" instances, much less non-instances; they might be interesting. Second, arguments from authority (whether mine or Motley's) must always be open to empirical examination and revision. A commitment to observe and learn from human communication should be the first fundamental postulate.

ENDNOTE

1. Actually, spontaneous yawning can be induced by a non-physiological, social stimuli (namely, seeing another person yawn; Provine, 1986), and blushing tends to occur only on parts of the body visible to others (Darwin, 1872/1965, pp. 215-217); so stomach growling may be the best example of a purely noncommunicative behavior.

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