

Research into the *Pragmatics of Human Communication*¹

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ABSTRACT

This article surveys recent research on interpersonal communication conducted by the Victoria Group and organizes the findings according to the "axioms" proposed in *Pragmatics of human communication* (Watzlawick, Beavin Bavelas, & Jackson, 1967). The topics include: establishing when a nonverbal behaviour is a nonverbal communication; investigations of "one cannot not communicate", including disqualified (equivocal) communication; studies of the communicative context in psychological research; verbal and nonverbal relationship level communication; analogically encoded nonverbal acts; and interpersonal systems. Some of the original propositions have been supported, others modified, and some substantially changed. The relation of research to clinical practice is discussed.

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For almost two decades, and especially in the past 10 years, our Victoria Group has been active in experimental research into interpersonal communication. Our theoretical starting point was *Pragmatics of human communication* (Watzlawick, Beavin Bavelas, & Jackson, 1967), and by now we have accumulated a number of studies that support, expand, or modify the tentative axioms offered in that work. These studies will be summarized here, both to introduce them to the family therapy literature and to illustrate the possibilities for reciprocal influence and benefit between therapy and basic research. We made it clear in *Pragmatics* that our theory came primarily from clinical observation; here I will examine the potential impact of subsequent research on the theory. To the extent that the original theory has affected clinical practice, this research -- although conducted in the laboratory with normal subjects -- may also be of interest to clinicians. If so, the ideas have in effect come full circle, repaying a debt to their clinical origins.

The research to be described ranges from book-length programmatic research to unpublished exploratory studies (available from the author). The latter have been included in spite of their more tentative status because they illustrate what might be done and also in hope that some readers will want to take them a step further.

All behaviour is not communication,
but one cannot not communicate.

My view of our "first axiom" (which has been embraced and attacked in the literature with equal enthusiasm) has changed markedly because of subsequent theoretical and empirical developments (cf. Bavelas, 1990). To begin, it is essential to correct a logical flaw in the statement of the axiom. The two key statements, "all behaviour in an interactional setting... is communication" and "one cannot not communicate" (1967, pp. 48-49) should not have been treated as equivalent. The first says that, in an interactional setting, all behaviour is communicative. The second says that, in such a setting, some behaviour is communicative. (In formal terms, this is the difference between a universal quantifier and an existential quantifier; e.g., Quine, 1959.) These two propositions will be dealt with separately here.

Nonverbal behaviour versus nonverbal communication

Because no one doubts that verbal acts are communicative, any discussion of the first proposition must focus on whether and when nonverbal behaviours are communicative. Shortly after the publication of *Pragmatics*, Wiener, Devoe, Rubinow, and Geller (1972) presented convincing arguments against treating all nonverbal behaviour as nonverbal communication. They pointed out that to do so (1) confuses merely informative acts with truly communicative acts and (2) imposes a "receiver bias", that is, attributes a communicative intention (conscious or unconscious) to all actions that any receiver may wish to call communicative. I agree with their conclusion that communicative nonverbal acts are a subset of all nonverbal acts (i.e., all behaviour is not

communication) and that empirical criteria must be developed to establish the communicative status of any nonverbal behaviours.

We developed such criteria while working on the phenomenon historically called "motor mimicry" (to be described below). An act that is only a nonverbal behaviour occurs for noncommunicative reasons. An observer can make inferences from such a behaviour, but there is (1) no sender-receiver relationship and (2) no encoding and decoding by means of a shared code. As an observer, you may infer the possibility of rain from dark clouds, but the clouds did not communicate this information to you. Similarly, if someone coughs, you as observer may infer she is ill, but she did not encode this as a message to you; the cough is an informative nonverbal behaviour but not a nonverbal communication. On the other hand, if someone enacts a cough to get your attention, he has established a sender-receiver relationship and used an understood code to convey a message; the stylized cough is a nonverbal communication.

Given this conceptual distinction, it is possible to set up experimental tests of whether a nonverbal behaviour is a nonverbal communication, which is what we did when we began to suspect that motor mimicry might be communicative. Motor mimicry is a (typically fleeting) nonverbal behaviour by an observer that is appropriate to the other person's situation, e.g., wincing at another's injury. It has historically been seen either as a reflex or as a sign of vicarious emotion or experience (see reviews by Allport, 1968; Bavelas, Black, Lemery, MacInnis, & Mullett, 1986; or Bavelas, Black, Lemery, & Mullett, 1987). In other words, motor mimicry has been treated as a nonverbal by-product of the actor's intrapsychic state, having nothing to do with the other person as a

receiver or decoder of the behaviour. In contrast, we proposed that motor mimicry is an analogically encoded nonverbal communication conveying, quickly and eloquently, the sender's sympathy for and identification with the other person. If so, then it would be less likely to occur when the other person would not see it, that is, when there is no sender-receiver relationship. Two experiments have shown this to be true. In the first, when the "victim" of an apparently serious injury did not make eye contact with the observing experimental subject, there was little likelihood of motor mimicry; when there was eye contact, wincing and other motor mimicry occurred (Bavelas, Black, Lemery, & Mullett, 1986). In the second study, when one subject listened to another (seated across the table) telling about a "close call", the listener displayed motor mimicry, but when there was a partition between them or when the story was told on the telephone, there was virtually no facial mimicry by the listener (Chovil, 1991).

We also conducted two tests of our second criterion, that is, whether motor mimicry is an encoded act. Videotapes of subjects in the first experiment above (Bavelas et al., 1986) were rated by a new group of subjects, and these "decoders" rated those who displayed motor mimicry as more aware and concerned about the injured person than those who did not. Next (Bavelas, Black, Chovil, Lemery, & Mullett, 1988), we found a kind of motor mimicry that could take two different forms and predicted that the form that was most decodable as involvement with the other person would be the most common form; this was confirmed. Thus, it is possible to establish empirically whether a behaviour is a nonverbal behaviour or a nonverbal communication, and I think we will find that, not all, but a surprising number of subtle nonverbal behaviours are encoded,

communicative acts.

Trying **not** to communicate

In *Pragmatics*, our main application of "one cannot not communicate" was the phenomenon of disqualification (1967, pp. 75-78), which was described as "saying nothing by saying something", that is, as contradictory, tangential, or obscure statements. We also proposed that the cause of this apparently "poor" communication is not individual pathology or incompetence but rather a situation in which all direct communication is impossible. Our research group (Bavelas, Black, Chovil, & Mullett, 1990) studied the phenomenon for over 10 years, during which we made the original theory more explicit, tested it experimentally, and confirmed it. (In the process, we also changed the name to equivocation, partly because of the wide variety of other meanings subsequently attributed to the term "disqualification" in both the communication and family therapy literature.)

We took our working definition of equivocation from Haley's (1959) analysis of schizophrenic communication, which drew on four essential elements of communication: sender, content, receiver, or context. That is, a perfectly clear message conveys, explicitly or implicitly,

I am saying this to you in this context.

An equivocal message (whether schizophrenic or normal) obscures at least one of these elements, for example,

Sender: "They say it's for the best" does not make clear what the sender thinks.

Content: "I agree, but on the other hand ..." obscures the meaning by contradiction.

Receiver: "People like you sometimes annoy me" does not clearly include or exclude the recipient.

Context: "That's an interesting question" does not answer the question.

Notice that, following Sluzki, Beavin, Tarnopolsky, and Veron (1967), we defined context as the previous question or statement, so not answering the question that was asked is equivocal in terms of context. These four elements became the basis of our quantitative measure of equivocation, which has proven to be both highly reliable and capable of subtle discriminations among equivocal messages.

Next, we became more specific about our situational theory, proposing that equivocation arises in a communicative avoidance-avoidance conflict, in which both direct options would lead to negative consequences and are therefore avoided in favour of an indirect, equivocal reply. Life is full of these situations; for example, when caught in a conflict between lying and telling a hurtful truth, the person is likely to avoid both and equivocate instead: "You always give the most unusual gifts!" If the conflictual situation is the cause, it follows that equivocation is not an idiosyncratic or pathological response. This was confirmed by over 20 different studies in which normal individuals equivocated or not depending solely on whether the experimental situation put penalties on direct communication -- just as we had originally proposed in *Pragmatics*.

There was another deduction from "one cannot not communicate" in *Pragmatics*, which is that even strangers who wish not to communicate will have to communicate this

wish (1967, p. 49). That is, the mere presence of others requires a statement about whether one is going to communicate. Mullett (1986) tested this hypothesis by creating three different conditions, in which strangers were asked (i) to work together on a task, (ii) to work individually, or (iii) to work individually under time pressure. She hypothesized that the third condition would make it necessary for the pair not to communicate with each other and that they would have to signal this to each other when they first came together. As predicted, nonverbal behaviours in the first 10 to 15 seconds were systematically different in the three conditions. In the third ("noncommunicative") condition, the participants used distinct patterns of eye contact and other nonverbal behaviours to signal their mutual wish not to communicate.

Bryson (1985) was interested in public situations in which an individual needs to make his or her behaviour interpretable by others but cannot address strangers directly (e.g., Goffman, 1964). For example, while rushing down a busy sidewalk you realize that you have forgotten your briefcase and must suddenly rush back in the other direction. Most of us will engage in what Bryson called explanatory behaviours, such as making a frustrated face and perhaps enacting a stylized halt and turn, hitting the forehead with the heel of the palm, or even exclaiming to oneself (for the benefit of others). These actions indicate to the observing public that there is a reason for the sudden about-face, probably that something had been forgotten, yet they do not break the rule against verbally addressing strangers in public. In a pilot study, Bryson showed that people who had been asked to engage in apparently meaningless actions in the presence of a stranger would stylize (encode) their actions as if to convey "I am doing something that makes

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As can be seen, we have been especially interested in issues centering on the "first axiom", that is, in precisely when and how people communicate. Although we have on the one hand conceded that all behaviour is not communicative, we have on the other hand been trying to push the limit in this regard -- to show that a great deal more is communicative, even by a conservative criterion, than has been previously believed.

We have even examined our own method, which is experimental research in the laboratory, from this point of view, following the suggestion in *Pragmatics* that "in general, psychological testing must consider the communicational context of these tests" (1967, p. 79). Consider "creativity tests", which traditionally assume that creativity is an intrinsic trait of individuals, moreover a trait that is directly tapped or measured (but never affected) by objective tests with items such as "List all the uses you can think of for a tin can" or "Name all the things you can think of that are hard, white, and edible." We (Bavelas & Lee, 1978) simply varied the number of blank lines (5, 10, 15, or 20) that appeared after the question, with the same amount of space for writing in every case. The number of items that subjects listed varied directly with the number of lines! It is unlikely that an intrinsic trait of creativity was affected by the number of blank lines on the test. Rather, the lines must be acting as an implicit instruction about the quantity and quality of responses the experimenter wanted. We confirmed this hypothesis by examining the qualitative aspects of the responses: For example, in the "hard, white, and edible" task, subjects loosened their criteria for these three qualities quite precisely as a function of the number of lines given. The objects that they listed in response to 5 lines

were very hard, definitely white, and good to eat. As the number of lines increased, the objects listed moved away from these strict criteria until, at 20 lines, the objects were "not soft, not dark, and not poisonous". It appears that experiments are indeed informational contexts in which subjects make precise and systematic inferences about what is expected.

A more ambitious study of the experimental context is being conducted in collaboration with Peter Stephenson (an anthropologist) and Allan Wade (a family therapist), in which we varied the experimenter's behaviour in a study of nonverbal communication. In one condition, the experimenter was formal, impersonal, and "objective"; in the other condition, the experimenter gave the same instructions but was informal and friendly, acting as if this were a normal social interaction. Even though our analysis is not yet finished, it is already obvious that the behaviour of subjects was greatly affected by the independent variable of experimenter behaviour. This contradicts the usual view, which treats the experimenter as somehow not part of the situation. It is important to stress that our goal is not to disparage experimental or any other kind of research by promoting vague notions of experimenter bias or silly interpretations of the Heisenberg principle. Instead our goal is to demystify the experimental setting by examining it as we would any other social interaction: Context always matters, so it is our job to study what its effects are and to take them into account (see also Bavelas, 1984).

Content and Relationship Communication

A second proposed axiom (1967, pp. 51-54) was that there were both content and relationship aspects of communication, the latter being a (usually implicit) statement

about the immediate relationship between the interactants (for example, the difference between "Would you mind very much not talking too loudly just now?" and "SHUT UP!").

For this axiom to be plausible or useful, it must be shown that ordinary people (not just experts) can use content to embed and retrieve (i.e., encode and decode) relationship meaning. Chovil (1980) asked people to write dialogues that would portray certain specified qualities of relationships (e.g. "friendly but formal, superficial, and unequal") without overt reference to the nature of the relationship. She showed that other people could accurately infer the relationship from these written transcripts. So an implicit relationship level does exist in communication; how it operates is an important and neglected area of research.

There has been a good deal of research by other groups on one particular relationship dimension (described in *Pragmatics* as the fifth axiom; 1967, pp. 67-70), namely, symmetry-complementarity. Rogers and her colleagues (e.g., Rogers, 1981) have shown that symmetrical or complementary relationship definitions can be conveyed through what is said and how it is said. Building on the work of Sluzki and Beavin (1965/1977), Rogers's scoring system uses principally grammatical form (e.g., interrogative versus imperative sentence structure) to infer definitions of the relationship.

As described above, we found that motor mimicry acted as a nonverbal means of conveying a relationship of involvement and "togetherness". However, when we tried to generalize this relationship message to nonverbal mirroring in general (e.g., similar postures or arm positions), the data said No. In three years of research, we could find

no evidence that competitive versus cooperative (together vs. against) relationships affected nonverbal mirroring. There was one considerably narrower finding, namely, a tendency for symmetrical dialogues to produce mirrored torso positions (principally, both people leaning forward) and for asymmetrical monologues to produce asymmetrical postures (e.g., one forward, one back), but that was all we found. Contrary to a widely held clinical and popular belief, mirroring of nonverbal postures is not connected to relationship definition in any way that we can presently understand.

One final note on the content/relationship distinction. I no longer think it is useful to call the relationship level "metacommunication", as both Bateson (1951) and *Pragmatics* (1967, p. 54) did, for two reasons: the term "relationship communication" will do just as well, and the term metacommunication is picking up too many different meanings to be useful (Bavelas, 1985; Sluzki, 1991; Wilmot, 1980). I would reserve the term metacommunication for one of Bateson's more specific meanings, namely, explicit communication about the process of communicating itself (e.g., "Are you kidding?" or "Don't interrupt!"). Here the issues of logical type and even possible paradox can actually be found in everyday communication, where they have not yet been fully appreciated.

Digital and Analogic Encoding

The fourth axiom distinguished between digital and analogic codes, that is, between symbols that are arbitrarily assigned versus those that in some way resemble what they represent. This principle has been very important for our studies of nonverbal communication. A major problem in the literature on nonverbal behaviour is that,

because the encoding is usually analogic, nonverbal acts are often seen as not encoded at all. That is, they are seen as behaviours caused (often involuntarily) by the actor's intrapsychic state rather than as behavioural messages about something (a classic confusion of the symbol with what it represents). For example, motor mimicry was historically equated with vicarious experience (feeling the other's pain) whereas it should be recognized as a symbolic representation: "It is as if I can feel your pain." Most nonverbal communicative behaviours are analogically encoded, but they are still encoded acts and not simply nonverbal behaviours.

Building on the concept of analogic encoding, and on McNeill's (1985) analysis of hand gestures as manual symbols with all of the properties of verbal language, Chovil (1991/92) showed that facial expressions, too, have many linguistic functions. Faces convey semantic information (e.g., illustrating one's own or another's reaction) and syntactic information (e.g., stressing words or underlining phrases with eyebrow actions). Our research group now routinely "translates" nonverbal communicative acts rather than merely tabulating them.

Some of our observations have led us to reject certain propositions about analogic communication made in *Pragmatics* or in subsequent interpretations of that book. First, we had suggested (1967, pp. 102-105), that analogic codes lack crucial elements of logical syntax, especially abstract negation, so that one could not say "no" analogically. We have now seen unambiguous analogic negation in both facial and gestural communication. For example, a woman is explaining that, although she likes a food that the listener dislikes, she doesn't eat it often. As she says the last phrase, she very quickly enacts an

asymmetrical "disgust" expression (wrinkling one side of her nose with the *caninus* muscle). Remember that she likes the food, so she is not indicating disgust; instead, she is using the metaphor of rejection that is implicit in disgust to indicate negation: "I don't eat it very often." We have also seen people convey negation gesturally by making either an erasing or a sweeping-away motion when they have misspoken and want to negate what they just said.

The second change I would make is to revise our implied (and sometimes explicit) division of communication into

verbal communication = digital encoding = content information.

and

nonverbal communication = analogic encoding = relationship information

This division would mean that content is usually conveyed verbally and digitally, while relationship is best conveyed nonverbally and analogically. More broadly, it implies that there are separate verbal and nonverbal "channels", dedicated to different uses. We are accumulating a great deal of data in favour of an alternative, "integrated message model" in which verbal and nonverbal acts work together (e.g., Bavelas et al., 1990; ch. 7; Bavelas, 1992; Bavelas & Chovil, 1993). For example, recall that Chovil (1980) showed that relationship information could be encoded and decoded in purely verbal transcripts. Our studies of facial expression (Chovil, 1991/92) and gestures (Bavelas, Chovil, Lawrie, & Wade, 1992) have shown that a great deal of content is conveyed nonverbally and analogically. Moreover, we (Bavelas et al., 1990, Chs. 6 & 7) found no evidence at all for "nonverbal leakage" in either equivocation or lying. That is, equivocation did not consist

of an incongruence between verbal and nonverbal aspects of a message, and verbal lies did not "give themselves away" nonverbally. Rather, people produced tightly integrated, though complex and subtle, packages of meaning. In sum, whereas *Pragmatics* implied that nonverbal communication was a functionally separate channel, I now see nonverbal communication as a completely integrated part of language.

The Study of Interpersonal Systems

Even though Chapters 4 and 5 on systems theory were my primary responsibility in *Pragmatics*, our group has only recently begun to explore these concepts in research to any extent. The problem is undoubtedly methodological: The research methods of social and behavioural science are monadic in focus; they are specialized for studying the intrapsychic processes of individuals or, at most, the unilateral effect of one individual on another. It often seems as if one has to choose between embracing a systems theory and doing experimental research. However, there is a third alternative (although not an easy one), which is to invent new methods.

My first effort to do this was less than satisfying, although the logic was straightforward: One reason that traditional studies remain summative (i.e., cannot predict the interaction of the whole) is that the independent and dependent variables are not focussed on social behaviours relevant to the system. For example, in person-perception research, the dependent variable is the subject's perception of the other person, as indicated on a rating scale. But we do not interact with other people by rating scales; if the subject's perception is going to affect the other, it must lead to some behaviour that the other will actually experience. This suggests that, to study individuals

in the context of a system, the researcher must ensure that the stimuli to person A are the behaviours of person B, and vice versa. I obtained such data for a simulated teacher-student dyad (Beavin 1970/Bavelas, 1975), in which "teachers" saw performance scores for (hypothetical) students and then set goals for each student's new task. Then we turned around and varied goals for students and looked at their consequent performance scores (Bavelas & Lee, 1978). Finally, the two were brought together (although in separate rooms), and the input-output parameters for individuals predicted very closely an actual six-step sequence of goal --> performance --> goal --> performance, etc. (Bavelas, 1978). In other words, systems can be studied via their individual members if the system context determines the behaviours to be studied.

Still, the final result was somehow not at all exciting. This may have been because the tasks used were not intrinsically interesting or because the dyads were strangers who never met. If so, the same design could be applied to more interesting cases, for example, a parent teaching a small child to catch a ball; the child's performance should affect the parent's goal-setting (how hard the ball is thrown), which should affect the child's performance, etc. On the other hand, it may just be that this method of approaching an interpersonal system was too cautious and conservative, too concerned with holding down the number of exchanges so that they could be studied, step by step, by an adaptation of traditional, reductionist methods.

Black (1986/1988) was braver and studied the system as a whole. He was interested in how topical coherence is managed in two-person conversations, that is, how the participants manage the contradictory requirements of covering more than one topic

in a conversation while maintaining some coherence. To solve this puzzle, first he distinguished between local coherence (the degree of connectedness between adjacent statements) and global coherence (the overall coherence of the conversation considered as a whole). Then he proposed that conversations act as negative feedback systems, which maintain a particular level of global coherence by adjustments at the level of local coherence. His data on four brief but varied conversations confirmed this process. Plotted over time, local coherence oscillated in the wave form typical of homeostatic systems, and the sequence of topics maximized global coherence.

Our current research is testing directly a broad systemic theory of conversation. We (Bavelas, Chovil, Lawrie, & Wade, 1992) proposed that dialogues have both topical and interactive aspects. That is, in addition to talking about some particular topic, the participants must maintain the conversation as an interactional system -- taking turns, involving each other, responding to each other, etc. These systemic or "housekeeping" functions are content-free in the sense that they are required whatever the topic. One problem the system must handle is how to maintain the involvement of both people even though only one can talk at once. Put another way, whenever one person talks, the system is inevitably tilted toward monologue; how does it counteract this centrifugal tendency? We discovered a class of hand gestures specialized for this function. Using these interactive gestures, the speaker can refer to and include the listener without yielding the turn. Several experiments (Bavelas et al., 1990; Bavelas, Chovil, Coates, & Roe, 1993) have supported our hypothesis by showing that the rate of gestures of this kind increased significantly (a) for dyads compared to monads, (b) when members of a

dyad talked face-to-face compared to when they could not see others' interactive gestures, and (c) when dyads who were face-to-face talked together rather than in sequential monologues. Moreover, interactive gestures are, as predicted, less redundant with the accompanying words than are topic gestures, and they elicit predictable responses from the person to whom they are addressed.

We are now looking at all other means, verbal and nonverbal, by which the interactive or dialogic aspect of a conversational system is accomplished. This goal is consistent with our view, described above, that verbal and nonverbal behaviours should be studied according to function ("what for?") rather than physical source ("where from?"). The same conversational function can be served by different verbal or nonverbal means. Indeed, my next book will probably be with Nicole Chovil, presenting our integrated message model of language in face-to-face dialogue. We propose that language as used in natural conversation (a) includes both verbal and nonverbal elements and (b) serves both the topic at hand and the systemic aspects of dialogue. All of these elements are seamlessly integrated in performance, which we are only beginning to appreciate.

Summary

As can be seen, we have not been doing research on therapeutic practice but rather basic research on human communication. Still, many therapists find our studies worthwhile for two reasons. First, many of these communicative phenomena and principles are encountered by family therapists every day, so therapists may have developed the same interest in them as we have, in which case the detailed examination

that research provides would be intrinsically interesting. Second, to the extent that therapists make certain assumptions about how people communicate and what their communication means, then evidence that confirms, modifies, or rebuts any of these assumptions is relevant to therapeutic practice. Some of the original propositions in *Pragmatics* have been confirmed and expanded, others have been substantially modified. Moreover, our microanalysis of human interaction has brought entirely new processes to our awareness; ordinary people still have a lot to teach us about communication. Indeed, our present work is focussed much more on careful observation of what people are doing than on any preconceived principles such as the axioms. The latter possibility -- learning from the data -- points out a deeper similarity between research and therapy. The best research, like the best therapy, is "hands on", with the practitioner intimately immersed in the events of the moment and how those events are proceeding. Both kinds of practitioners follow what people do, learn from them, and only then intervene. Good research, in my view, is much more inductive and intuitive than formal research reports convey (Bavelas, 1987).

Because of these similarities and affinities, I am puzzled and saddened to hear, occasionally, a certain stereotype of research in family therapy circles. Researchers are sometimes cast as cold-eyed positivists who believe that they can extract Truth from nature while maintaining an antiseptic distance from the objects of their study. For me, the actual process of research is best captured by writers such as Polanyi (1958/1964), Medawar (1979), and Keller (1983), who write freely of intuition, passion, commitment, and craft. Most of us have eluded the twin extremes of positivism and constructivism and

are engaged in an activity that, like therapy, is a balance of observation, involvement, and careful action, all in pursuit of a clearer view of human behaviour.

Footnotes

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