

THEORETICAL AND METHODOLOGICAL PRINCIPLES OF THE EQUIVOCATION PROJECT

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The theoretical framework of this long-term research project on equivocation includes three essential principles: (a) An interesting and unexplained phenomenon is worth studying for itself, by inductive methods; (b) communicative acts are part of a communicative sequence; and (c) the methods must keep the phenomenon in its communicative sequence. The article explicates these principles and applies them to other research, including the studies in this special issue. The broader issue is the cumulative nature of research, that is, how to judge when a new study adds to, confirms, or disconfirms a body of work versus when new studies take such a different direction that they do not bear on previous work.

When a topic such as equivocation piques the interest of a number of researchers, the subsequent studies give us the opportunity not only to clarify assumptions but also to consider broader issues such as tolerance and the cumulative nature of research. I am going to explore these issues for the particular case of equivocation to develop the general thesis that terms such as *equivocation* are always implicitly or explicitly grounded in a set of theoretical and methodological assumptions that ultimately define them. I hope to make clear two different but complementary points: Our research group does not own the term equivocation, and all uses of that term do not bear on our research.

In the following, the essential principles that derived from and were supported by our program of research on equivocal messages and that may be valuable in understanding other interesting communicative phenomena will be outlined. These principles may be useful in themselves, and they can also provide the reader with a framework for evaluating subsequent research on equivocation, such as that found in this special issue. That is, when a study is consistent with these principles, then we must accept its results (positive or negative) as relevant to our theory, and the results can be cumulative. When a study departs from these basic principles, it becomes orthogonal to ours. The new data or theory may supplant us because it is more interesting to others,

but it cannot support or disconfirm our findings. We might criticize such a study (or any study) for technical or logical flaws, but we cannot criticize each other for simply choosing different paths, lest we stifle research itself.

THREE PRINCIPLES

Some deeply held convictions about the nature of communication (Watzlawick, Beavin, & Jackson, 1967) inspired and shaped our program of research into equivocal communication (Bavelas, 1983, 1985; Bavelas, Black, Bryson, & Mullett, 1988; Bavelas, Black, Chovil, & Mullett, 1990a, 1990b; Bavelas & Chovil, 1986; Bavelas & Smith, 1982). Moreover, the research project itself helped us articulate, test, and develop these convictions to the point where they might easily be called a theoretical framework, or at least a coherent set of principles. As will be seen, these principles soon proved their worth by helping us to approach and understand new phenomena such as *motor mimicry* (Bavelas, Black, Chovil, Lemery, & Mullett, 1988; Bavelas, Black, Lemery, MacInnis, & Mullett, 1986; Bavelas, Black, Lemery, & Mullett, 1986, 1987), *interactive gestures* (Bavelas, 1994; Bavelas, Chovil, Coates, & Roe, 1995; Bavelas, Chovil, Lawrie, & Wade, 1992), and *listener responses* (Bavelas, Coates, & Johnson, 1995).

1. AN INTERESTING AND UNEXPLAINED PHENOMENON IS WORTH STUDYING FOR ITSELF, BY INDUCTIVE METHODS

It is possible and desirable to start with a communicative phenomenon, observable in human interaction, rather than with theoretical concepts or dimensions that claim to transcend (mere?) phenomena. Our first principle is to take a phenomenon-initiated inductive stance in which we notice interesting events and wonder what we might learn from them. For example, although we built our equivocation work on the early observations of the Palo Alto Group, we did not keep the accompanying theory, which was that the prototypic equivocal message was verbally and nonverbally incongruent. We did not find incongruence in our data (Bavelas et al., 1990a, chaps. 6-7). We observed instead that the verbal and nonverbal aspects of our messages were highly integrated and congruent in equivocal as well as clear messages. For example, in Experiment 11A, experimental participants had been asked to imagine that they had received a gift so bizarre that they could not tell whether it was a joke or to be taken seriously. When the experimenter asked, "How do you like the gift I sent you," one participant responded as follows:

Umh (with slight laugh). (Starts hesitantly, then brightening:) *It was-ah not BAD* (ends with very slight laugh). (Bavelas et al., 1990a, p. 135)

The verbal description "not BAD" is a classic equivocation, because "not bad" does not by any means equal "good." The nonverbal (voice quality) features were also equivocal, lurching from a slight laugh to hesitation, to a bright tone and back to the slight laugh. She equivocated both verbally and nonverbally. Another participant replied,

(Sounding puzzled:) *Ah? . . .* (Slight laugh, which slurs into the first word:) *It's quite intriguing.* (Bavelas et al., 1990a, p. 135)

"Intriguing" is, of course, a more sophisticated form of the classic equivocal descriptor, "interesting." The term raises possibilities but conveys nothing definite—just like the puzzled tone, slight laugh, and slurring. None of our adult participants said "it's great" in a less-than-convincing tone of voice; only some of the children showed verbal-nonverbal incongruence, which we concluded was a developmental stage (chap. 8). The discovery that congruence is the adult norm has greatly assisted our subsequent research and theorizing about nonverbal acts (Bavelas, 1994; Bavelas & Chovil, 1997a, 1997b).

Another example of choosing a phenomenon and applying the inductive method was our research on what Allport (1968) had called *motor mimicry* (e.g., wincing at someone else's injury). Allport had found observations of motor mimicry dating back to the 18th century, but he concluded that it remained "a riddle in social psychology" (p. 30). All previous theories had considered it a vicarious action, most commonly as vicarious emotion or empathy; the overt behaviour was the unwitting by-product of an intrapsychic event. We had the modern technical advantage of videotape and therefore could accumulate and microanalyze many instances of motor mimicry (Bavelas, Black, Lemery, MacInnis, et al., 1986). Our inductive observations and reasoning soon revealed that the traditional intrapsychic theories were not viable: On close observation and subsequent experimentation, motor mimicry is a precise and articulate message of empathy to the other person; it is not an individual reflex but a communicative act (Bavelas et al., 1988; Bavelas, Black, Lemery, & Mullett, 1986). Since the equivocation project, every discovery our research group has made came from induction, from putting aside theoretical prescription in favour of observing. (These discoveries were subsequently verified by deductive, hypothesis-testing, and usually, experimental research.)

In the particular case of equivocation, theory and preconception obscured the phenomenon for an additional reason—value judgments. Equivocal communication has been dismissed as error or condemned as defective or dishonest, all because of popular views and theoretical models that prescribe clear communication. (Worst sacrilege of all, equivocation apparently violates one or more of Grice's [1975]

maxims.) Praise is rarer but just as fallacious because equivocal messages are not reified entities to be ascribed an absolute value. In many situations, equivocal messages serve values that many of us accept (e.g., avoiding both lying and harm to a relationship). However, in other situations, particularly in a political context, equivocation serves only the self-interest of the politician while frustrating the legitimate needs of the electorate.

Communication and psychology have not yet been able to separate and clearly define phenomena without infusing their definitions with the values of the definer; the lack of a coherent definition of deception or lying is an obvious example (Bavelas et al., 1990a, pp. 172-176). For example, most definitions of deception impose a standard in which the truth must be clear and brutal. Yet, Turner, Edgley, and Olmstead (1975) pointed out that hurtful candor may be a relationship lie, conveying "I don't care if I hurt you." In those cases, a softened statement—less clear but still true and therefore equivocal—would be honest in terms of the relationship as well.

Both our values and our research are best served by separating scientific definition from personal and social values. Only when it is clear what we are talking about can we have an informed opinion about it. So I am not suggesting the possibility or desirability of a value-free science; I have personal values about when to equivocate and when not to. But my personal opinions changed and were greatly refined as I understood more about what equivocal messages were doing, that is, the more I focused on the phenomenon itself. To discuss whether equivocation is good or bad without a clear definition is simply sophomoric and does not serve the values one wishes to address.

Inductive research, which respects the phenomenon under investigation and moves cautiously through experimental tests to firmly based theory, is the hallmark of the natural and life sciences, accounting for their remarkable empirical and theoretical progress. In contrast, inductive work is little understood or appreciated in the social and behavioral sciences (Bavelas, 1987, 1991, 1995), where the ratio of theory to supporting data is alarmingly high. Pavlov, whose Nobel Prize-winning work was more profound than the simple reflex now associated with his name, gave this advice to young scientists early in this century:

Firstly, gradualness. About this most important condition of fruitful scientific work I never can speak without emotion. Gradualness, gradualness, and gradualness. From the very beginning of your work, school yourselves to severe gradualness in the accumulation of knowledge.

Learn the ABC of science before you try to ascend to its summit. Never begin the subsequent without mastering the preceding. Never attempt to screen an insufficiency of knowledge even by the most audacious surmise and hypothesis. Howsoever this soap-bubble will rejoice your eyes by its play, it inevitably will burst and you will have nothing except shame.

School yourselves to demureness and patience. Learn to inure yourselves to drudgery in science. Learn, compare, collect the facts!

But learning, experimenting, observing, try not to stay on the surface of the facts. Do not become the archivists of facts. Try to penetrate to the secret of their occurrence, persistently search for the laws that govern them. (Pavlov cited in Morison, 1960, pp. 187-188)

2. COMMUNICATIVE ACTS ARE PART OF A COMMUNICATIVE SEQUENCE

The essence of our approach is to keep the phenomenon in its living context, which is the communicative sequence of which it is a part. The sequence is framed by its interpersonal setting and includes, at the very least, its immediate precursors and consequences. Thus, we learned about equivocal communication by watching it inside naturally occurring conversations. It is true that our initial experimental tests (Experiments 1 to 5 in Bavelas et al., 1990a) were highly restricted and artificial; I would be the first to admit that the main reason was my cowardice, especially about the cost of scaling real messages if there was no hope for our hypothesis in the first place. But, encouraged, we moved quickly toward eliciting real messages in fuller settings, and we always included (a) a familiar interpersonal setting, (b) a preceding message, and (c) implied consequences. We did not examine abstracted, reified equivocation. Subsequently, we (Roberts & Bavelas, 1996) have proposed that meaning, in general, requires the analysis of a minimum three-step sequence; meaning is not in the message, or in the sender's intention, or in the receiver's interpretation; it is in the sequence.

It may be self-evident that an equivocal message occurs as part of a communication sequence, but adopting this principle whole-heartedly leads to a different kind of theory. The first change is a move away from taxonomy toward function. A great deal of communication theory is taxonomic, that is, with the goal of classification or typology. Many theories generate categories of messages, relationships, people, goals, or whatever. Other theories propose or organize sets of dimensions, factors, or variables, which are simply more abstract taxonomies. The problem with a taxonomic approach is that it is usually static, and it renders the phenomenon static; the goal is to put the phenomenon in a category and leave it there. Categories may affect categories as the theorist dictates, but nothing happens in the real world of communication. One might argue that taxonomists predict behaviour from their categories, but even when successful, the link is an obscure one: How can being in a category cause action in the world? Isn't it better to study action in the world?

Our earliest work on equivocation (Sluzki, Beavin, Tarnopolsky, & Veron, 1966/1967) was taxonomic; it produced an interesting typology,

which just led to more subtypes and away from the fascinating conversations in which they occurred. Only later, when we began to insist on including the sequence in which the equivocal message was embedded could we begin to learn, not what kind it was, but what its function was (namely, to solve a communicative dilemma). Unfortunately, by convention, we must give names to our research topics, thereby converting them from verbs to lifeless nouns.

There is another, more subtle, benefit of focusing on function rather than category. The ideal category system is mutually exclusive and exhaustive so that an event "belongs" in only one category. Among other problems, this leads easily to arguments over whose territory the phenomenon belongs in. But a communicative phenomenon can have many different functions. So, instead of narrowly defending our categorical territories, we can broaden our view to embrace the multifunctioned wonder of human communicative behaviour. For example, in our studies of hand gestures (Bavelas, 1994; Bavelas, Chovil, et al., 1995), we have discovered how liberating it is to focus on the interactive functions of certain gestures rather than classifying them as *interactive gestures*. When, by convention, we have to call them interactive gestures, we implicitly and unwillingly seem to compete with other researchers who have their own classifications (i.e., saw other functions). If we could always talk about the interactive-functions of some gestures, then it would be clear that a particular gesture can and does have many different functions at its particular moment in conversation; we have simply added a new one.

Looking at sequence also creates the possibility of looking for a causal explanation in the sequence itself. The vast majority of researchers explain communicative acts intrapsychically, that is, by hypothetical mental causes located within the individual. Indeed, when someone asks why something happened, he or she is usually seeking a nonobservable answer (that is, an invented explanation in the mind or in the past, or both) as dictated by accepted notions of causality. The alternative is to look at the function of the act in the interaction, to ask "what for?" rather than "why?" (Watzlawick et al., 1967, p. 45). When our interest is in communicative phenomena rather than mental life, we can look for patterns and functions in the surrounding events and leave hypothetical mental states (which are interesting in their own right) to the cognitive and personality psychologists. The purpose of placing equivocal messages in a communicative sequence is to emphasize the press of the situation and the vividness of the consequences, without which equivocation is merely funny talk that has to be explained mentally. (It should not be necessary, this late in the 20th century, to add that being interested in phenomena outside the mind is not the same as a radical behaviourism that denies the existence or importance of the mind.)

Our theory, quite simply, is that the situation and the preceding message can create an avoidance-avoidance conflict in which any unequivocal response would lead to unwelcome consequences. The explanation lies in the obvious negative valences of alternatives in the sequence, and there is no need to invent explanatory mental processes external to the communication. Similarly, motor mimicry is elicited (caused) by the communicative situation and is not the by-product of feelings or emotions (Bavelas et al., 1988; Bavelas, Black, Lemery, & Mullett, 1986); interactive gestures are caused by face-to-face dialogue (Bavelas, Chovil, et al., 1995).

3. OUR METHODS MUST KEEP THE PHENOMENON IN ITS COMMUNICATIVE SEQUENCE

A researcher who accepts the above principle, that phenomena such as equivocal messages occur and function only in a communicative sequence, faces some hard but not impossible methodological problems. Certainly, one cannot simply plug an equivocal message into a currently fashionable formula (usually, a new statistical method) and expect to learn about equivocation as we have defined it. On the other hand, one of the exciting benefits of an inductive, phenomenon-centered approach is that each instance requires new methods. Finding new and suitable methods means exercising the logic underlying all research methods rather than staying on the surface and accepting or rejecting methods by form, familiarity, or prestige.

Our first step was to identify when equivocal communication is occurring, a process I will call measurement, with the caveat that numbers may or may not be involved. One chooses not quantitative or qualitative analysis but the kind of analysis that best suits the phenomenon (Bavelas, 1995). In our case, we did both, and they complemented each other. Our measurement decisions (Bavelas et al., 1990a, chap. 2; Bavelas & Smith, 1982) were dictated by the principles set out above. First, we were interested in the actual phenomena of equivocal messages, so we did not ask people whether they would equivocate, lie, or tell the truth. Those are categories, not living messages. (Moreover, when we talked with our participants after an experiment, as we always did, it was clear that there is little consensus on what those terms mean. Even in scholarly articles, it is common to read descriptions of "lies" that are really equivocations; see below.)

Like any message, an equivocal message does not suddenly appear in a vacuum; it occurs because there is a person to receive it as part of a communicative sequence. Therefore, we chose to identify equivocation by the informational impact on (potential) receivers, our lay judges. Equivocation is about clarity or lack thereof, so we wanted to know what information a recipient would get or not get from the message. The dimensions we chose to use (Haley's four elements) were the

simplest possible way to ask that question. We were not interested in how anyone felt about the message, what they thought was the motive behind the message, or any other properties external to the information in the message for a receiver.

Most especially, we were not interested in expert analyses of the messages. The latter was very difficult to resist, early on, because we kept seeing fascinating properties and potential variables. It was constantly tempting to freeze the message midstream and talk about its properties. Fortunately, we kept the faith and were both chagrined to see how wrong we would have been and rewarded by the richer insights our judges gave us. The qualitative analyses we reported for every experiment with real messages (Experiments 6-19 in Bavelas et al., 1990a) are their insights, not ours. They told us what they saw in a message and thereby explicated for us what the four simple dimensions meant. Others undoubtedly disagree, both with us and with our judges, and that is fine as long as they realize that, having used a different method or definition or having imposed their expert analysis, they are no longer talking about the same phenomenon. (As we feared, this is unfortunately common, probably in large part because our scaling method is so time-consuming and costly.)

Having figured out how to measure equivocal communication, the second choice was how to test our theory of when and why it occurs. Strange as it may seem, the two methods most suited to a test of our situational theory are conversation analysis and true experiments. Conversation analysis emphasizes the whole text and the relationships in sequences of communication; it would not separate a message from its pair partner. True experiments, too, are intrinsically focused on the relationship between events in that they set up conditions in order to observe the immediate consequences. Another similarity is the discipline of precision and focus that both planning an experiment and doing conversation analysis require. Neither permits sweeping abstractions or multivariate confusion because the precise details are essential and of greatest interest.

Although I have used and still use a variety of methods, I often choose experiments. There is an irresistible challenge in applying experimental methods in areas where most modern experimentalists fear to tread. Historically, experimental psychology has been full of elegance and innovation, not statistics, and I enjoy that classical challenge. Equally important, true experiments are informative because they take a risk by testing an hypothesis. That is, I enjoy the transition from the openness of the inductive phase to the strict deductive logic of hypothesis testing. A clearly stated hypothesis in a situation entirely of your own design and choosing leaves you nowhere to hide.

(For a multivariate study to be a true experiment, it would not only have to apply random assignment to manipulated conditions, it would also have to be testing explicit and complete hypotheses for all

variables and their combinations. Ideally, it would present a table at the outset showing clear correspondences between conceptual and statistical hypotheses for every independent and dependent variable and their potential interactions. After the results, there would be a similar accounting of each and every test: which hypotheses were supported, which were disconfirmed by opposite findings, and which were tested but found not significant.)

NEW RESEARCH ON EQUIVOCATION

In this two-part special issue of the *Journal of Language and Social Psychology*, there are two and a half articles that are consistent with the above framework and therefore address our theory. The others are on different topics, with different goals and principles, so they do not offer support, disconfirmation, extension, or revision.

Both Galasiński (1998) and Bull (1998) have extended our knowledge of equivocation well past lab experiments. The public settings they chose (a presidential debate and media interviews) were rife with the avoidance-avoidance conditions that should evoke equivocation. Moreover, the valences in these situations were more potent than any we could impose in our hypothetical situations. Both studies corrected obvious deficiencies in our studies by replicating them in more consequential settings. In both cases, they found apparent exceptions to our theory as well, and these deserve further analysis.

Bull (1998) explicitly set his study in the context of our earlier work but, equally explicitly, as a revision and extension. One of his goals was to move out of our one-step conflicts into the changing dynamic of an extended interaction. Clearly, the earlier exchanges in a series not only constrain but give new meanings to the later ones. Galasiński's (1998) data also illustrated the dynamic nature of what we called negative valences—which he called, more succinctly, “the stakes.” Initially, his two candidates faced fairly balanced negative consequences (violating the rules of the debate vs. letting an attack go unanswered). As predicted, they equivocated by violating the rules without really violating them, for example, by saying they were forced to respond. However, as the other candidate's attacks became more telling, the balance shifted enough to make a direct response more important than the rules, and their violations were less obviously mitigated. Like Bull's, Galasiński's data and analysis make it clear that avoidance-avoidance conflicts are not fixed or static in a given situation. They may shift as the conversation proceeds, and Galasiński showed that candidates were highly sensitive to these shifts.

Another contribution of both studies was to identify new kinds of equivocation. Although neither Bull (1998) nor Galasiński (1998) formally scaled their messages, they did apply our qualitative definitions

of equivocation skillfully. Bull (1998) noted two different ways of not-answering a question, and I completely agree that they are qualitatively distinct. Going further, our quantitative analyses were necessary and effective for experimental hypothesis testing, but I doubt that extended sequences of real talk can be understood quantitatively.

Another example of qualitative virtue is in Galasiński's (1998) data, where the structure of the most common forms of mitigation ("I'm not interrupting, but . . .") was strikingly reminiscent of van Dijk's (1992) finding in racist discourse, namely, "I'm not a racist, but . . ." followed by racist comments. The structure is a disclaimer followed by a prohibited statement. This structure may be related to the specific nature of the conflict, in which the speaker definitely wants to say something but is implicitly or explicitly prohibited from saying it. This may be in fact an *approach-avoidance* conflict (Lewin, 1938), which we did not heretofore consider: For the speaker, there are attractive aspects to saying a certain thing (the approach part), but there are also negative consequences to saying that very thing (the avoidance part). Does the ensuing equivocation usually take the form of a disclaimer followed by the prohibited statement? It is important to note that both of these articles generated new ideas about equivocation because they (and only they, in this set) gave us examples of real messages in real settings.

Bull (1998) explicitly (and Galasiński, 1998, implicitly) added the concept of face-threats to equivocation theory. Certainly, threats to face are a plausible subset of the larger class we called negative valences. The only subset we had identified was lying-versus-hurting-someone; the rest of our conflicts were idiosyncratic but interesting odds and ends.

I do not agree, however, that two classes are isomorphic. We cannot say that threats to face underlie all avoidance-avoidance conflicts, political or otherwise, without the risk of making the concept of face too elastic and circular to be useful (i.e., if there was equivocation, there must have been a face-threat). For example, in our Experiment 11B (Bavelas et al., 1990a), the conflict was between lying to a customer and risking one's job by being honest. It might be possible to call lying a face-threat (to oneself, unless one is caught), but it would be shallow to cast losing one's job that way. Similarly, in the world of political image making, many negative valences are indeed threats to face (appearing incompetent or too competent, evasive or too blunt, too formal or too informal, aggressive or wishy-washy, etc.) However, there are other negative valences that are more parsimoniously identified by their practical consequence, namely, losing a block of votes. The recent phenomenon of single-issue voters is an example: A pro-choice statement is going to lose the votes of all of those who oppose abortion, no matter how much they like or otherwise respect the candidate. It may well be that the concerns I raise can be answered logically or empirically; the reader may be able to think of ways to do so.

My other reservation about the concept of face, generally, is its individual focus. Face preservation is an individual motivation so that individuals use conversation to counter threats to the self-image or self-presentation of one or the other of them. An alternative view is to see relationship first and to propose that self is defined only within relationship (Watzlawick et al., 1967). In a political setting, each candidate is constantly defining himself or herself in relationship to the other: as more informed, more honest, having a better record, and so on. For example, if one insults or criticizes the other, the relationship implied is one of superiority/inferiority. Within that relationship—and only within it, I would argue—the self of each person is defined (positive or negative face). There are subtle issues here that need to be explored further.

In their studies of truly agonizing conflicts (job applications and medical bad news), Robinson, Shepherd, and Heywood (1998) [this issue] have followed our experimental methods and have added new variables that refine our theory. For example, if a fear of being caught is one of the negative valences for lying, then the probability of being caught should affect the choice to lie, equivocate, or tell the truth, as it did in their study. In both of their scenarios, these researchers added a great deal more precision to our global and intuitive estimates of what constituted a negative valence or avoidance-avoidance conflict.

Robinson et al. (1998) have particularly captured the multifaceted anguish of a doctor faced with telling a patient about a serious, even terminal, illness. They demonstrated the need to push such scenarios further to a better understanding of the intricacies of particular communicative conflicts.

My disappointment in this work was the relative absence of messages. I cannot fault the forced-choice format, which we used as well. But the choices did not, as far as I could tell, always include real messages. There were some classic equivocations such as “slightly asthmatic” when health was a job criterion or “nearly 25” when age was a criterion, as well as the medical equivocation, “These [hip] replacements are extremely popular operations.” But, as far as I can tell, there were often choices of categories of messages, such as “The doctor decides to tell the patient the truth in an accurate but compassionate manner.” In my experience, that would usually mean an equivocal message. In our forced-choice experiments, respondents often said, afterward, that they would most prefer to “tell the truth kindly.” When we asked for such a message, it was equivocal, which is not surprising because equivocation is telling the truth kindly (i.e., not bluntly). A formal study to test the relationship between described categories and actual messages would be valuable because the method is widely used.

Robinson et al. (1998) also equated equivocation with concealment (i.e., not telling the truth brutally), but not answering is only one way of equivocating. It is informative (in their Table 1) that the results for

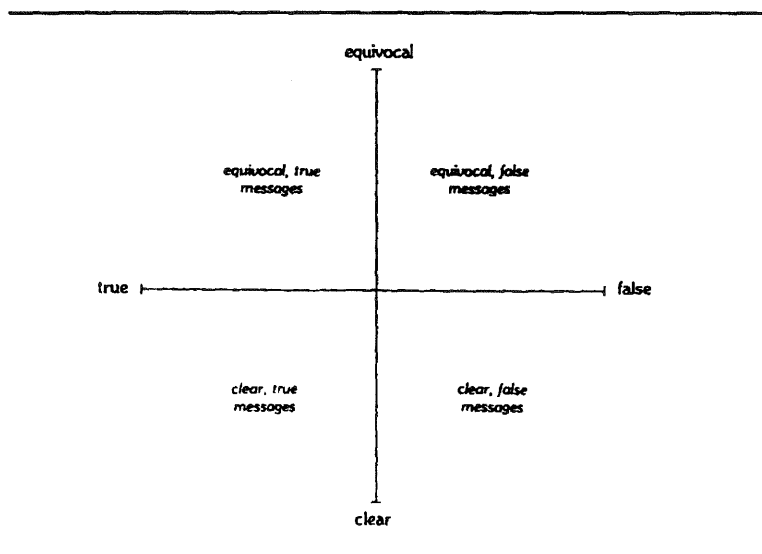


Figure 1. Proposed independent coordinates of truthfulness and equivocation. Source. From Bavelas et al., 1990a, p. 178; reproduced with permission.

the first two scenarios, which offered only a concealment alternative, differed from the third, which offered a different kind of equivocation. Perhaps because they equated equivocation with concealment, the authors consistently and even quantitatively treated equivocation as unidimensionally close to lying, whereas we had proposed and shown that equivocations are truthful on one dimension and unclear on another, orthogonal dimension (Bavelas et al., 1990a, chap. 7) (see Figure 1).

To understand the implications of orthogonality, imagine two simple, dichotomous dimensions: age (over or under 50) and gender (male/female). I would go into the quadrant labeled *female, over 50*, but no one would suggest that the dimensions must be correlated in this way (i.e., all females must be over 50 and all males under 50). Other individuals would go into female, under 50; male, over 50; and male, under 50. Similarly, we showed that the equivocality (clarity/uncertainty) of a message is independent of its truth/falsity. In Experiment 15 (shown here in Figure 2), we created experimental conditions that elicited messages that were, by independent scalings, clear truths, equivocal truths, and relatively clear lies.

We can take a great deal of the responsibility for some of these problems. Our measurement methods (for equivocation and for truth telling) are so costly and time consuming that no one, to my knowledge, has ever used them. Yet, our theory is based on distinctions made by

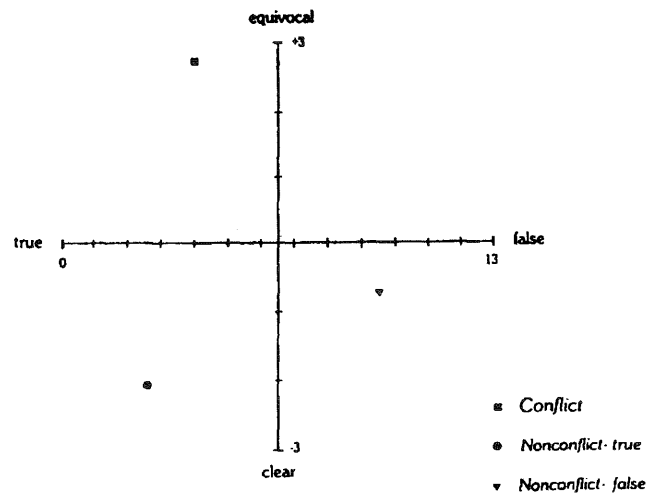


Figure 2. Truthfulness and equivocation means for experimental conditions in Experiment 15.

Source. From Bavelas et al., 1990a, p. 205; reproduced with permission.

those methods. A good alternative, illustrated earlier, is to immerse oneself in the qualitative analyses we reported extensively and to become as sensitive to what is equivocal as our lay judges were.

The remaining articles are essentially unrelated to our work. I must reemphasize that this is not a fatal flaw, just that we must understand that their findings do not support, disconfirm, modify, or extend the original theory.

Hamilton (1998) and Hamilton and Mineo (1998) explicitly label their approach an information-processing theory, that is, a theory about how potential receivers think about equivocal messages. We neglected the receiver in our experiments; the lay judges were not the receivers who had initiated the conflict by asking the key question. The experimenters who asked the question did spontaneously complete the conversation, but of course, their behaviours were not data. Only in our field studies (Bavelas et al., 1990a, chap. 9) did we examine conversational sequences that included the receiver. We specifically analyzed the negative valences faced by interviewers and politicians and showed how both parties dodged around them. How a receiver will respond depends, of course, on the particular valences and the particular equivocation. For example, the receiver may respond to both the kindness and the truth in an equivocal message (Roberts & Bavelas,

1996), but when the equivocation takes the form of answering a question with a question, the receiver has to deal with the new question.

However, Hamilton and Mineo (1998) and Hamilton (1998) did not examine conversational sequences including the receiver's reaction. They were interested in the ways in which individuals might rate the equivocal messages and the relationships among those ratings, in order to track the mental events in their information-processing model. Such data may be well suited to the evaluation of a communicator in noninteractive settings (e.g., mass communication), where ratings such as poll or survey responses are highly relevant. However, individuals' reports or ratings of how they evaluate or comprehend a message are not the same as their reactions to those messages in a communicative sequence nor, I suspect, would they predict how a person would respond to an equivocal message in dialogue. As we argued elsewhere (Bavelas & Coates, 1992), those who propose complex cognitive models of communication have to show how such a model can operate in the split-second timing of actual dialogue. Moreover, the valences that bear on making a rating or report are different than those that press the individual in a dialogue. One can study hypothetical mental processes for their own intrinsic interest, but we cannot assume without evidence that these processes affect conversational behaviour.

The other reason that I believe these studies are not relevant to our equivocation theory is that the authors analyzed data gathered for other purposes and put their expert labels on the variables instead of the original meanings. As one who was intimate with one of those data sets (the equivocation data reanalyzed in Hamilton & Mineo, 1998), I cannot accept their reinterpretation of our dimensions of equivocation without empirical evidence. Our lay judges made highly specific decisions about our messages; one cannot change their meanings just because they seem to resemble other concepts.

Donohue (1998) gave us a privileged glimpse of a major political event, confirming my cherished belief that abstract political events happen in particular. I was entranced by what followed, but the focus was on a model of conflict resolution and not on how particular situations evoke equivocal messages. Indeed, there are (understandably, in view of the secrecy of the proceedings) no messages. I infer from the analysis that equivocation did not refer to a specific message in a specific communicative sequence. Edwards (1998) reported a multivariate investigation of the effect of an individual difference variable, gender, on the interpretation of forced-choice messages as classified by experts. Indeed, the main dependent variable (supporting vs. controlling interpretation) was not about the messages but about presumed motivations behind the messages. There were no conversations, no real messages, and the goal was not to study equivocation.

Similarly, Eisenberg (1998) neither cited nor aimed to discuss equivocation in our sense. His is a theory of the self, identity, and the virtues of ambiguity. However, Eisenberg did mention some ideas worth reading in the original, namely, those he credited to Friedman (1993). Some highly innovative and world-renowned psychotherapists—particularly my colleagues, Steve de Shazer and Michael White—authored these ideas and the theories behind them. White and Epston (1990) introduced the powerful notion of “re-storying” (usually, re-authoring) lives. Their approach is not (as described) ambivalent about history; White and Epston are clear that there are many versions of a patient’s history and that the therapist should help to author the most healthy one. Similarly, the authors of the “miracle question” were de Shazer and his colleagues (e.g., de Shazer, 1985, 1988, 1994), who have written extensively on how the therapist can directly and explicitly refocus the client on specific solutions rather than global problems, engendering an entirely different discourse. Both White and de Shazer are explicit that their therapies use the power of language to construct or reconstruct events. Neither advocates or uses fuzzy ambiguity but, rather, the opposite: clear intervention and dramatic change.

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