

# HANDBOOK OF LANGUAGE AND SOCIAL INTERACTION

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# The Two Solitudes: Reconciling Social Psychology and Language and Social Interaction

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In his classic novel, Hugh MacLennan (1945) described English and French Canada as two solitudes living in the same country but in effectively separate worlds. This term has become a metaphor for many kinds of estrangement, and it often seems an apt one for the relationship between the two disciplines of Language and Social Interaction (LSI)<sup>1</sup> and social psychology. In spite of potential common ground, they are in many ways quite isolated from each other. Indeed, the analogy with French and English Canada may extend into the present, in which they are sometimes mutually hostile. Of course, these attitudes are heterogenous within both fields, and there are also positive relationships. The other two chapters in this section (Bradac & Giles; Gallois, McKay, & Pittam) review the thriving sub-area of language and social psychology, which attempts to bridge some of the divisions.

This chapter addresses the often unspoken but clearly understood barriers to mutual respect and collaboration between social psychology and LSI, with the hope of at least clarifying and possibly even overcoming some of them. In order to do so, it is first necessary to sort out a sometimes confusing terminology for several overlapping or nested areas, whose names may also overlap. My focus here is on two broad disciplines: Mainstream, traditional *social psychology* is a major division of the field of psychology; its assumptions and methods derive from general psychology. Language and Social Interaction is well defined in this handbook as a diverse, multidisciplinary convergence of interests that include conversation analysis, pragmatics, discourse analysis, ethnography, and the subarea of social psychology called language and social psychology. (Although I will not directly discuss the latter, some of the analysis may clarify its position and possibilities within LSI.)

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<sup>1</sup>The capitalized forms (Language and Social Interaction and LSI) refer to the discipline. Other forms refer to the topics.

On the surface, the fit between the fields of Language and Social Interaction and social psychology is an obvious one: Both are strong research traditions that share *social* as a key identifying term. For social psychology, the relevance of both social interaction and language is clear and, conversely, one can easily imagine social psychological aspects of Language and Social Interaction. It is therefore surprising to find that the two fields are so isolated from each other; indeed, the mention of one by the other may even be critical and dismissive. Even though these research traditions have evolved independently, the division is not simply because they are unaware of each other. Rather, both traditions implicitly base their identities, to some extent, on rejecting central and identifying characteristics of the other. The goals of this chapter are to examine the substantive and methodological differences that, in my opinion, divide the two fields and also to propose some new ways of thinking that could make them more compatible and even complementary.

### A BRIEF HISTORY OF THE (POTENTIAL) RELATIONSHIP

Although social psychology, as psychology generally, tends to focus on the individual and his or her mental processes, there is also a long-standing interest in social processes. Thibaut and Kelley (1959) proposed that, from the beginning, there have been two traditions within social psychology. These authors pointed out that the first two books with “Social Psychology” in the title, both published in 1908, prefigured the majority and minority areas of interest in the modern field:

The book by the psychologist William McDougall (1908) was mainly an attempt to identify and classify the various social motives [i.e., the study of the individual]. In the same year the sociologist Edward A. Ross initiated a contrasting tradition for social psychology by defining its distinctive mission as the analysis of social interaction.

In the years since 1908 many writers on social psychology have implicitly accepted the injunction of Ross to concern themselves with social interaction. To mention a few of these, we might point to Dashiell’s (1935) comments on the social situation as a “reciprocal affair,” . . . Cottrell’s (1942) reinforcement analysis of the “interact pattern,” Lewin’s (1947) interpretation of dyadic interaction as a “three-stage process,” Bales’s (1950) extensive discussions of interaction in small decision-making groups, and Sears’s (1951) S–R references to “double contingency” in the “social episode.”<sup>2</sup> (Thibaut & Kelley, 1959, pp. 1–2)

Although the authors whom Thibaut and Kelley (1959) cited were all stressing the importance of social interaction, few of them actually studied it, and none mentioned the unique importance of language and communication to the study of human social interaction. In an isolated article, Wright (1947) proposed “the psychological centrality of communication,” arguing that “the nature of personal communication is not adequately described as [simply] a mode of social interaction” (p. 92), rather it is the central process. It was not until the 1970s that language and communication were again identified as essential to the understanding of social processes. For example, Harre and Secord’s (1972)

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<sup>2</sup>In this article, Sears also introduced the term *dyad* to psychology.

“explanation of social behaviour” focused on the role of language. Potter and Wetherell (1987) proposed a social psychology that focused entirely on discourse. By 1990, there was enough research to fill Robinson and Giles’s first *Handbook of Language and Social Psychology*, with a second edition in 2001.

Calls for a more central role for language and communication in social psychology persist: Both Moscovici (1972) and Tajfel (1972) argued for the importance of language and lamented the lack of social psychological research on it, as have Clark (1985), Forgas (1983), Schneider (1991), Senn (1989), Smith (1983), and Solano (1989), who called the study of communication “an obviously important social mechanism that links cognitive processes to [actual] social behavior” (p. 554).

Still, Wright’s (1947) question remains substantially unanswered: “If communication, understood as the exchanging and sharing of meaningful experiences, is basic to human association and a pre-condition of human cooperation, how does it happen that social psychologists have so largely ignored it?” (p. 94). As Fraser and Scherer (1982, p. 2) observed, “social psychology has managed to construct itself as a discipline while, for the most part, studiously avoiding the systematic study of language.” Kroger and Wood (1992) documented the very low occurrence of language as a topic covered in sources such as social psychology texts and the *Handbook of Social Psychology*. Clark (1985) contributed the first chapter on language in the history of the *Handbook of Social Psychology*. Yet, as a psycholinguist who is not a social psychologist, he echoed Wright’s (1947) question: “Language is a social instrument. . . . it is paradoxical, then, that modern social psychologists have paid so little attention to language use” (p. 179). Given the absence of a social psychological program of research on language as social interaction, his chapter instead introduced LSI (specifically, conversation analysis) to social psychologists. Having gained a foothold, “Language and Social Behavior” appeared in the next edition of the *Handbook* (Krauss & Chiu, 1998), but from a perspective quite different from the LSI approach. This chapter frames language in what Reddy (1979) called the “conduit metaphor”:

Language pervades social life. It is the principal *vehicle* for the transmission of cultural knowledge, and the primary means by which we *gain access to the contents of others’ minds*. Language is implicated in most of the phenomena that lie at the core of social psychology: attitude change, social perception, personal identity, social interaction, intergroup bias and stereotyping, attribution, and so on. Moreover, for social psychologists, language typically is the *medium* by which subjects’ responses are elicited, and in which they respond: in social psychological research, more often than not, language plays a role in both stimulus and response. (Krauss & Chiu, p. 41; emphasis added)

Thus there has been a strong but definitely minority interest in social interaction, and then language, within the discipline of social psychology. In examining the reasons for this comparative neglect, I shall often echo Danziger’s (1990) approach to the historical development of contemporary psychology. Danziger proposed that a scientific field is not just its theories, its findings, or its individual contributors but also a social context. He emphasized the “socially constructed nature of psychological knowledge” (p. 2) and the “essentially social nature of scientific activity” (p. 3). I too am explicitly assuming that the nature of social psychology and of LSI are in large part defined by the researchers

in those fields, not by intellectual processes independent of their human authors. So far, we have seen that social psychologists have constructed their field with a limited role for language or social interaction, but now we should also ask what interest LSI researchers have had in social psychology. The answer, “virtually none,” must also be considered in a sociohistorical context. LSI arose from conversation analysis within sociology and from ethnography of speaking within sociolinguistics and anthropology; it is in many senses an alternative to both mainstream sociology and social psychology. LSI identity includes what it is not as well as what it is, and what it is definitely not is the use of traditional social psychological research methods to study traditional social psychological topics.

In sum, there has been a persistent but infrequently pursued interest in language as social interaction by mainstream social psychologists and a persistent lack of interest in social psychology by LSI researchers. In this chapter, I examine two of the major reasons for this insularity, namely, differences regarding the appropriate unit of analysis and the preferred research method. In both cases, the purpose is to understand these differences and their rationales in order to seek ways to reduce or at least reconcile them.

### INDIVIDUAL VERSUS SOCIAL UNIT OF ANALYSIS

“The first prerequisite of a successful observation in any science is a definite understanding about what size of unit one is going to observe at a given time” (Kurt Lewin, in Deutsch, 1968, p. 419). In their choices of what to study, mainstream social psychology and LSI are contemporary versions of the difference between McDougall and Ross a century earlier. Social psychology has mostly adopted McDougall’s (1908) focus on the individual as the natural unit of analysis. LSI (whether as conversation analysis or as part of pragmatics, ethnography, or discourse analysis) resembles Ross (1908) with a clear and consistent focus on social interaction as manifested in language. What is striking to someone who works in both fields is how deeply this difference in unit of analysis is embedded and how it affects everything from choice of phenomena to theorizing and explaining. Obviously, in the context of this handbook, a social unit is the appropriate one and needs no further justification. Instead, the focus in this section is on the rationale for and implications of starting with the individual as the basic unit of analysis in social psychology.

Allport’s (1954, p. 3) widely accepted definition of social psychology is that it is “an attempt to understand and explain how the thought, feeling, and behavior of individuals are influenced by the actual, imagined, or implied presence of other human beings.” This definition offers a wide range of choices, but only one combination of these choices, namely, focusing on *behavior* in the *actual presence* of others, would include the study of language in social interaction. As Solano (1989, p. 36) pointed out, “While [Allport’s definition] does not preclude studying interaction, it does not stress its importance.” The many other choices offered in that definition lead to research and theory on the thoughts or feelings of individuals or on the imagined or implied presence of others. Even a cursory examination of social psychology journals and textbooks reveals that these, rather than actual social behavior, have been the preferences of the vast majority of social psychologists. I should hasten to add that theirs are legitimate and interesting preferences; not everyone has to be interested in language or social interaction. However, if the

“presence of others” is a defining characteristic of social psychology, it is striking how relatively seldom others are actually present. Studies in which lone individuals rate hypothetical others on a questionnaire are common. Studies of two or more individuals interacting are much less frequent, and in most of these interactions, the other person is a confederate, so social influence is unilateral and not reciprocal. None of these resemble the presence and influence of others in our everyday social life.

### **Consequences of Focusing on the Individual**

Before examining why traditional social psychology has chosen the individual as its unit of analysis, one might ask, does it matter? I think it does and shall consider here two of the inevitable effects of the traditional social psychological focus on the individual: It eliminates social interaction from both data and theory, and it locates theoretical explanation within the mind.

***Making Language and Social Interaction Disappear.*** The first and obvious effect of focusing on the individual is that this is very likely to preclude learning anything about social interaction or even about the effect of social interaction on the variables of interest. For example, Allport (1968) drew attention to the phenomenon of “motor mimicry” (e.g., wincing at another person’s injury), which had been noted since at least Adam Smith (1759), yet remained “a riddle in social psychology” (Allport, 1968, p. 30). Theories of motor mimicry had focused entirely on the individual engaging in the mimicry (e.g., on empathy or vicarious emotion). Our research group placed motor mimicry in a social context and showed that it communicated understanding and concern to the other person. In our experiments, participants were more likely to display motor mimicry when the other person would see it, and observers interpreted their mimicry as caring or involvement (Bavelas, Black, Chovil, Lemery, & Mullett, 1988; Bavelas, Black, Lemery, & Mullett, 1986). We then went on to discover the true home of motor mimicry in face-to-face dialogue, where it is one means by which listeners help narrators tell good stories (Bavelas, Coates, & Johnson, 2000). The answer to the riddle could not have been found by focusing on individuals.

If the social context is an artifact to be controlled or eliminated, then the experimentally isolated individual becomes the methodological ideal. Yet because everyday life takes place in a social context and isolation is an abnormal state, there are legitimate questions about the external validity and generalizability of pursuing this ideal. Another example illustrates this issue: The social psychological study of social influence began with Sherif’s (1935) studies of groups of several people watching and reporting on an ambiguous stimulus. Their interaction was limited to announcing what they saw each time, but they were all real participants reporting their own perceptions, and their convergence on a group norm was an important finding. Later, Asch (1951) studied social influence with the use of confederates, each of whom announced a scripted decision before the one real participant made his or her decision. No social interaction or reciprocal influence was possible. At most, these findings would generalize to situations in which, for some reason, the individual who reported last could have no influence on the others present, no matter what he or she did. The findings offer no information on how individuals resolve

differing perceptions through talking about them. Johnson (1992) created a variation on Sherif's procedure in which the individuals could talk freely and discovered systematic interaction patterns for agreement and for agreement to disagree.

***Privileging Mental Explanations.*** The second effect of studying individuals alone is that intrapsychic explanations become logically inevitable:

Failure to realize the intricacies of the relationships between an event and the matrix in which it takes place, between an organism and its environment, either confronts the observer with something "mysterious" or induces him to attribute to his object of study certain properties the object may not possess. Compared with the wide acceptance of this fact in biology, the behavioral sciences seem still to base themselves to a large extent on a monadic view of the individual and on the time-honored method of isolating variables. . . . If [an individual] is studied in isolation, then the inquiry must be concerned with the . . . *nature* of the human mind. (Watzlawick, Beavin Bavelas, & Jackson, 1967, p. 21)

If the social context is eliminated methodologically, then the only available theoretical explanations are mental ones, and this indeed is the case in traditional social psychology. Starting with McDougall's (1908) theory of instincts, the explanatory processes have continued to be almost exclusively mental, albeit varying considerably over the decades. Currently, cognition has eclipsed or subsumed earlier interests in perception, learning, emotion, motivation, personality, and other hypothesized mental processes, but the effect is the same. In their review of the cognitive perspective in social psychology, Markus and Zajonc (1985) made two telling points: "[The] adoption of the cognitive view among social psychologists has been so complete that it is extremely difficult for most of the workers in the field to conceive of a viable alternative. . . . The result is that one can no longer view today's social psychology as the study of social behavior. It is more accurate to define it as the study of the social mind" (p. 137). Language in social interaction cannot be conceived as a viable alternative precisely because they have been eliminated before the study began. Thus, the initial choice to study isolated individuals leads to a circularity in which only mental explanations are possible, which justifies the study of isolated individuals.

### **Questioning the Rationale for Focusing on the Individual**

Given that the effect of choosing the individual as the unit of analysis is to lead social psychology away from language in social interaction, one must ask why this choice has so dominated the field. If the reasons behind the choice could be questioned, social psychology might move more toward LSI when that was appropriate. I propose there are two primary reasons, a passive one that assumes there is no alternative and an active one based on the logic of reductionism. Both, I will argue, are weak.

***Passive Acceptance.*** Although it is surprising to an outsider that traditional social psychology studies individuals, social psychologists themselves seldom comment on the choice, much less defend it, perhaps because it is not seen as a choice. One highly influential treatise on the topic is Allport's chapter on "The Historical Background

of Social Psychology,” which introduced the first three editions of the *Handbook of Social Psychology* (1954, 1968, 1985). Allport’s interpretation of history understandably reflected his own view of the field and has occasionally been questioned by others (e.g., Kroger & Wood, 1992; Lubek & Appelbaum, 2000). A close reading of his chapter reveals at least three ways in which Allport constructed social psychology as the study of individuals. The first is by definition. According to Allport, psychology is the study of the individual and “social psychology is above all else a branch of general psychology. Its center of emphasis is the same: human nature *as localized in the person*” (1985, p. 3, emphasis added). In other words, to be a psychologist means to study the individual, and social psychology is no exception. (Social psychologists who do take interaction as their unit of analysis will at some point hear the question from colleagues, “Is that really psychology?”; cf. Robinson, 1998).

Second, the only alternative Allport offered to the study of “human nature as localized in the person” was the study of society as a whole. Immediately after defining psychology as the study of individuals, Allport went on to comment that “sociology, anthropology, and political science are ‘higher-level’ disciplines. . . . They wish to know the course of society with the individual extracted” (p. 3). In Allport’s view, one must choose between studying an individual and studying a society; there is no intermediate unit between these two.<sup>3</sup> An obvious alternative is to interpose a new unit, the study of the relationship or interaction between individuals, as manifested in their communication (Watzlawick et al., 1967, p. 21). Physics includes units of analysis ranging from subatomic particles to galaxies. As a mature discipline, psychology could also be seen as encompassing units of analysis from neurophysiology to social interaction.

Finally, Allport was consistently negative in his characterization of social interaction. For example, when he surveyed several historical theories that went beyond the individual, he selected and emphasized negatively connoted titles and topics. *La Psychologie des Foules* (Le Bon, 1895), translated as *The Crowd* was equated with irrational behavior in a mob and implicitly associated with fascism and crime (pp. 24–25). Terms such as “the group mind,” “suggestion,” and “conformity” also presented social processes in the worst light, that is, as the enemy of the rational individual. Then, after identifying the four historically important “units of analysis” as instinct, attitude, habit, and sentiment (pp. 33–38), he raised a potential criticism of his position: These four units of analysis “all have the weakness of assuming a fixity of disposition [within the individual] and overlook the flexibility of behavior that is exhibited when environmental situations alter” (p. 38). If one remembers that Allport was also a personality theorist, it is not surprising that he quickly dismissed this potential weakness by presenting a false choice between individual consistency and complete situational specificity. He was particularly harsh toward those who would favor “momentary situationism” (p. 38), a term that ironically captures what many of us aspire to document. It is precisely the rich and impressive moment-by-moment improvisation by each person to what the other person offers that makes language in social interaction so worthy of study. Nothing in what we discover demeans the dignity of the individual, as Allport implies; quite the contrary.

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<sup>3</sup>In an aside later in the chapter, Allport did mention the “dyadic relationship.” He erroneously cited the social psychologist Heider (1958) as having studied this topic extensively.



**The Logic of Reductionism.** The choice of the individual as the unit of study does not depend solely on passive acceptance of the rationale just outlined; it is also the active product of a certain version of reductionism. Many researchers who have never read or do not accept the highly individualistic view of social psychology presented by Allport and others would probably still assume that the logic of science, and reductionism in particular, mandates that they start with the individual as the unit of analysis: “For a very long time reductionism remained the generally accepted philosophical aim of the natural sciences as well as of psychology. It was supposed that the basic goal of science is to reduce complex phenomena to separate simple parts, and that such reduction provides significant explanations of phenomena” (Luria, 1987, p. 675). Applied to language in social interaction, this reductionism would dictate that the best approach is to reduce this complex phenomenon to separate simple parts, namely, individuals. Once we understand the nature of individuals, this reasoning goes, then their conversations are simply the additive results of the separate individual efforts.

However, even the most traditional explications of the principle of reductionism raise questions about its application: “The debate is typically not over whether the more molecular components *exist*, it is over whether or not greater insight into the underlying nature of the phenomena under consideration can be achieved by reaching down to them. . . . In psychology too much has been made of ultimate reduction” (Reber, 1985, p. 623). In psychology, reduction has often been to the neurological level. Yet the neuropsychologist Luria asserted that, in spite of the general acceptance of reductionism in the past:

There are grounds to suppose that it may be false. To study a phenomenon, or an event, and to explain it, *one has to preserve all its basic features*. [emphasis added]. . . . It can easily be seen that reductionism may very soon conflict with this goal. One can reduce water (H<sub>2</sub>O) into H and O, but—as is well known—H (hydrogen) burns and O (oxygen) is necessary for burning; whereas water (H<sub>2</sub>O) has neither the first or second quality. . . . In order not to lose the basic features of water, one must split it into *units* (H<sub>2</sub>O) and not into *elements* (H and O). [emphasis original] (Luria, 1987, p. 675)

Thus, even though the *elements* of a conversation may be the individuals involved, they are not the appropriate *units* of analysis, because they do not preserve the basic features of a conversation, which are interactional. The unit chosen must be the conversation itself. To break it down further is to lose the basic features of the event of interest, which reductionism cannot and does not require.

One of the founders of North American social psychology, Kurt Lewin, proposed that one of the most important decisions a researcher makes is his or her initial choice of the unit of analysis appropriate to the problem. Lewin’s phenomenological life space (“which consists of the person and the environment viewed as one constellation of interdependent factors”; Deutsch, 1968, p. 417) was not the same as a unit for Language and Social Interaction, but it does illustrate that there is a strong historical precedent for a unit larger than the isolated individual. Indeed, there are many resonances for LSI researchers in Deutsch’s chapter on the “field theory” of Lewin and his colleagues. For example, “It needed great scientific imagination to realize that it was not the charges nor the particles but the field in the space between the charges and particles which is essential

for the description of physical phenomena” (Einstein & Infeld, 1938, p. 259; quoted in Deutsch, p. 413).

### Prospects

It would be easy to cast the two solitudes of social psychology and LSI as irreconcilably divided on their units of analysis: Contemporary social psychology, especially with the ascendancy of social cognition, seems committed to the study of individuals and their mental processes, at best neglecting the very stuff of Language and Social Interaction and at worst regarding it as an epiphenomenon—a secondary and therefore not very interesting by-product of or conduit for the psychology of individuals. But that conclusion would be wrong and unduly pessimistic, for several reasons.

First, it is noteworthy how often references to social units and processes have been cited, almost wistfully, in social psychology. For example, Sears’s (1951) advocacy of the dyad as the appropriate unit of analysis has been echoed in the literature through the years, as has Bandura’s (1969) characterization of learning as a reciprocal influence process. Major reviews have asked “Where oh where is the social in social cognition?” (Forgas, 1983, p. 129) and “What is social in social cognition?” (Schneider, 1991, p. 553); the latter author proposed that one answer was more attention to communication. Second, the most recent editions of the *Handbook of Social Psychology* have finally included chapters on language (Clark, 1985; Krauss & Chiu, 1998). As shown in the other two chapters in this section of the present handbook (Bradac & Giles; Gallois, McKay & Pittam), language and social psychology is a rapidly merging specialization. Although not tightly connected to either mainstream social psychology or LSI, it represents the concrete efforts of many researchers to work on topics that include both disciplines and thereby has at least the potential to move beyond the individual and the intrapsychic. Finally, although I have suggested that social psychology should leave individual and mental constructs behind in shifting to a social unit of analysis, there are those who would tackle mental phenomena directly, recasting them in social and discursive rather than individual units (Potter & Wetherall, 1987; also see Edwards’s chapter in this handbook in the section on “Discourse Analysis”). And there are occasional voices within LSI who have suggested that mental or cognitive concepts need not be excluded entirely (Arundale & Good, 2002; Pomerantz, 1990–1991, Sanders, forthcoming). There are many implicit invocations of individual mental processes as explanations in LSI research; it would be good to recognize these and thereby enliven the debate about unit of analysis from a broader perspective.

Clearly, if the study of language in social interaction is going to become a definite area within social psychology, both traditional social psychologists and those who are advocating this rapprochement must become more sophisticated and articulate about the legitimacy of a social unit of analysis. Much of the resistance is embedded in traditions and assumptions that could be debated once they are recognized.

### WHAT METHODS ARE APPROPRIATE AND ACCEPTABLE?

The previous section focused on the need for social psychology to expand to embrace a truly social unit of analysis, as LSI already does. As will be seen in this section, both LSI and social psychology contribute equally to their second major difference, which is

the choice of research method. More precisely, the difference lies not in their choice of method but in their mutual rejection of the other's preferred research method. The ideal for most social psychologists is an experiment, conducted in the lab, with control and manipulation of variables, in which behavior is objectively measured, then analyzed by quantitative and statistical methods. Many LSI researchers reject these choices, labeling them with terms such as "positivist" and "artificial." Instead, they place highest value on data gathered anywhere but a lab, with no experimental manipulation or control, analyzed at the level of meaning, using qualitative methods and no statistics. Most social psychologists reject these choices, using labels such as "unscientific" and "subjective."<sup>4</sup> As will be seen, the pejorative labels on both sides are misleading and inaccurate, and they often reflect a double standard. More fundamentally, in my view, they reflect polarized and stereotypic thinking that presents only two alternatives, when in fact there are several independent choices, which can be separated and recombined in many different ways (Bavelas, 1995; Robinson, 1998; Robinson & Giles, 1990). Here I will attempt to be more specific about three related methodological choices, their justifications, and some possible reconciliations:

1. Where the data are obtained (inside or outside a lab).
2. Whether and how the researcher intervenes (conducts an experiment or not).
3. How the data are analyzed (interpretation, objectivity, and statistics).

### The Location of Data Gathering

There is a widespread belief within LSI that data gathered in the lab are inherently worthless, because the situation is artificial or unrepresentative and the behavior is completely influenced by the experimenter. This is not an opinion shared by one of the founders of conversation analysis. Schegloff (1992) pointed out the need for a more nuanced approach. After comparing two lab experiments, one that studied only an individual and one that included a dyad, he concluded as follows: "Even though both of those settings can be characterized by a single context description [namely]: 'laboratory,' the vernacular terms do not do the work. In one case 'laboratory' is, and in the other case it is not, procedurally consequential *for the particular phenomenon being studied*" (Schegloff, p. 116; emphasis in original). The procedurally consequential aspect for Schegloff, as for all LSI researchers, was whether there were two people. As discussed in the first section, social psychology is full of lab studies that include only one person (or one participant with one or more confederates), but this is a unit-of-analysis problem; it has nothing to do with the lab as a location. The rejection of all lab studies because of those that use individuals or confederates is an error of metonymy, in which one feature stands for others that, in this instance, may not be present.

Surprising as it may seem, some LSI researchers reject lab data simply because it was gathered in the lab, rather than in what they call "the real world," as if the lab were the

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<sup>4</sup>Those who doubt the extent of this division might, on the one hand, read Aronson and Carlsmith's (1968) advocacy of experiments over other methods and, on the other hand, notice the summary dismissal or virtual absence of experimental data within many areas of LSI.

entry point to an unreal world with entirely different (artificial) laws and phenomena. Rejection at this level imposes a curious dichotomy in which the lab setting is seen as different from all other settings, in that it affects social interactions in ways that other settings do not, rendering them not real, and hence not typical or informative. The irony is that LSI as a field (especially ethnography and microethnography, as shown in chapters in this handbook) has focused precisely on studying behavior in context. It is therefore hard to justify rejecting some behavior solely because of the context in which it occurs, rather than wanting to study or compare it to other contexts. Similarly, behavior in the lab may also be dismissed as trivial. There is no question that an ethical experimenter will not introduce variables that are as important to the participants as some events in the rest of their lives, but it would be hard for a field grounded in everyday events to reject behavior as not warranting our attention because of its ordinariness.

A more sophisticated but equally fallacious criticism uses a version of the Heisenberg uncertainty principle from experimental physics to assert that observation always distorts the data being observed, rendering it useless. If this is so, why have experimental physicists continued to do research? And why is observation outside the lab considered exempt from this problem? The answer is that the metaphor is based on a wrong understanding of the principle (Bavelas, 1984), which simply cautioned that one must always be aware of the inevitable effects of observational choices. It applies to all observation, inside or outside the lab. Social interaction is not intrinsically more natural, in the sense of being unaffected, when observed (and recorded) outside the lab. Whether the researcher films in the lab or sets up cameras at family gatherings, the participants' behaviors will be affected by their understanding of the meaning of this observation. Our job is to come to an appreciation of these effects in each instance, rather than exaggerating them in the case of lab research and ignoring them in other settings.

Just as most LSI researchers reject the lab as a location, social psychologists most often insist on being in the lab and are at best ambivalent about other locations, for several reasons. The first is the mirror image of the LSI objection, namely, that the findings of studies conducted in particular locations, such as suicide centers or work sites, are particular to that location, whereas the results of studies in a particular lab are somehow context free. Thus, critics in both disciplines apply the same double standard: Context matters only when it's your context, not mine!

In addition, psychologists often equate field studies with nonexperimental studies, assuming that experimental control and manipulation of the independent variable is best or only achieved in the lab. However, this assumption mixes two different issues. There are many studies conducted in the lab that do not have experimental control because of poor design or procedure; nothing about the physical setting can improve them. There are even more lab studies that do not manipulate the independent variable because it is not possible to do so: Gender and age are often treated as independent variables even though the researcher has no control over them and cannot manipulate them, even in the lab. Finally, it is possible to conduct some formal experiments outside lab settings, and there are also a variety of quasi-experimental designs that may be just as satisfactory, especially with the generalizability advantage that may come with a field setting. Of course, this assumes that one wants to do an experiment in the first place, a topic I will turn to next.

## Experimental Intervention

The advocacy or rejection of the experimental method seems to arise from at least two of its features. These are the control or manipulation of variables and the goal of hypothesis testing.

**Experimental Control.** Social psychologists value experimental control and manipulation of variables extremely highly and usually dismiss nonexperimental studies as inferior (e.g., Aronson & Carlsmith, 1968). Thibaut and Kelly (1959) identified this commitment to the experimental method as perhaps the major reason for the neglect of social interaction in psychology:

In the typical experiment in psychology the subject is in some manner under the management of the experimenter. . . . the experimenter exerts control over the behavior of the subject, and the procedures by which he does so constitute the independent variables of the experiment. The behaviors that the subject actually emits constitute, of course, the dependent variables. . . .

The situation is sharply different when social interaction is considered. . . . The possibility is now introduced that each subject will exercise control over the other. . . . the complexity that is added by reciprocal control may be denoted by the loss of a clear separation between independent and dependent variables . . . each [individual's] behavior is in part dependent and in part independent variable; in no clear sense is it properly either of them. . . .

If it is true that free social interaction leads to an ambiguity about what is dependent and what is independent, and since social psychology is traditionally committed to the use of experimental methods . . . how has investigation proceeded? The answer seems to be that the problem has been largely bypassed [using only methods in which] some degree of experimental control has been maintained. (Thibaut & Kelly, pp. 2–3)

In other words, given a hard choice between studying actual social interaction and using experimental methods, social psychologists have given up the social interaction in order to retain the experimental method. In the early 1980s, I had the opportunity to ask the distinguished social and developmental psychologist Robert Sears why so little research had been done along the lines of his well-received 1951 Presidential Address to the American Psychological Association, in which he advocated the dyad as the unit of analysis for social phenomena: Was it a conceptual barrier such as Kuhn (1970) had described, requiring an entirely new paradigm? Or was it the lack of experimental methods for studying social interaction? His reply was that it was definitely mostly the latter, that psychologists will tend to work in areas that fit the methods they have learned and to avoid those topics where their methods don't work. (It is important to add that being led by their method is not unique to psychologists.) The solution, of course, is to develop new methods and new ways of thinking about variables. For example, rather than examining each individual's behavior as the dependent variable, their joint actions can be the focus (e.g., Clark & Wilkes-Gibbs, 1986).

Experimental techniques are best at establishing causal relationships, but they impose a certain view of causality. For example, based on experimental results (Bavelas et al., 2000), our research group proposed that when listeners are distracted, they make fewer specific responses (which Goodwin, 1986, called assessors) to the narrator's story, which ultimately leads to a decline in the quality of the story. More traditional social psychologists suggested that we should verify this causal claim statistically, using mediation analysis (Baron & Kenny, 1986; Kenny, Kashy, & Bolger, 1998). However, all of these statistical techniques assume that there is a linear causal sequence (a domino effect) from one individual (or variable) to another. In contrast, we explicitly assumed a continuous *reciprocal* influence between narrator and listener, and indeed, our data empirically violated the technique's assumptions (Bavelas et al., 2000, p. 950). To our knowledge and that of the experts, there were no statistical techniques to test our model, which is a typical one in LSI.

To the same degree as social psychologists value the experimental method, LSI researchers are inclined to dismiss experimental studies as having altered the natural occurrence of language so massively as to be virtually worthless. It is certainly the case that, as previously implied by the social psychologists Thibaut and Kelly (1959), once the participants' continuous reciprocal influence has been removed for experimental control, there is probably no social interaction left that is worth examining. However, if an experiment does include spontaneous social interaction, then this particular reason for rejecting it should disappear. Schegloff's (1992) principle applies here as well: Rather than reject all experimental intervention, one should ask whether a specific intervention was procedurally consequential for the phenomenon of interest. There is no doubt that introducing different experimental conditions is likely to affect the participants' interaction; that is why the experimenter does it. However, as noted above, situational influence is not a foreign or artifactual notion to LSI researchers; there is a growing interest in examining similarities and differences in talk in widely different settings and occasions (e.g., Drew & Heritage, 1992). Perhaps this interest can begin to include the study of similarities and differences in talk both within experiments and compared to other settings.

**Deductive Versus Inductive Approaches.** Heritage and Atkinson (1984) identified a second objection to the experimental method, which is that hypothesis testing is often premature:

Experimental procedures are generally successful to the extent that, through experimental manipulation, behavioral variation is limited to those aspects selected for investigation under controlled conditions. In this context, it is the experimenter who must determine the relevant dependent and independent variables, and the experimenter's formulation of these variables will tend to be restricted by what he or she can anticipate on an intuitive basis. Yet without previous exposure to a range of naturally occurring interactional data, the experimenter is unlikely to anticipate the range, scope, and variety of behavioral variation that might be responsive to experimental manipulation. . . . The most economical procedure, therefore, has been to work on naturally occurring materials from the outset. (Heritage & Atkinson, p. 3)

Thus, an experimental social psychologist might choose variables based on intuition, as Heritage and Atkinson suggested (or, more likely, derive them from theories in the literature), then deduce a hypothesis, and test that hypothesis with an experiment. In contrast, an LSI researcher would go to the data, not just to generate a theory but even to identify what the relevant variables are—an approach that social psychologists may dismiss as atheoretical or lacking rigor, in spite of its established success in many natural sciences (Bavelas, 1987).

The resolution of this difference simply requires noticing the differences both in goals and in the stage of research, which may determine whether and when one chooses to do an experiment. When looking for something new, the best place to look is in the data (Bavelas, 1987) rather in one's preconceptions or the library, where by definition the information cannot be unknown or entirely new. As Heritage and Atkinson (1984) implied, existing social psychological theories are unlikely to be a good guide, especially at the level of detail and particularity at which LSI researchers excel. However, another inference one could make from their position is that, having developed an appreciation of the range and variation in the behavior in nonexperimental data, one could then do a much more sensible experiment than before. (Our research group follows this cycle of induction to deduction in our own research.) As fields such as conversation analysis accumulate a body of knowledge based on close observation of social interaction, some researchers may wish to explore or test hypotheses derived from this inductive knowledge with new methods, including experiments.

Following this line of reasoning, some researchers might start with inductive observations and then change to experiments, whereas some might continue to work with nonexperimental data. The choice would probably depend on the researcher's goal. Those who are curious about a hypothesis that can best be tested experimentally would follow one route; those who are curious about understanding and documenting a particular phenomenon or process would follow a different route. In an ideal and methodologically flexible world, the same researchers might make different choices in their next projects. At the very least, experimental researchers would welcome complementary findings by researchers using other methods, and the reverse (Bavelas, 1999). However, to be honest, given the long-standing and usually unquestioned commitment to experiments per se, it is probably going to be more difficult for social psychologists to accept methods such as conversation analysis or ethnography than the reverse. To achieve this acceptance, we would need to embark on a collaborative discussion and discovery of what constitutes rigor and proof. Imagine that a hypothetical social psychologist agreed to study some data gathered in the field without experimental manipulation, and an equally hypothetical LSI researcher agreed to study some experimental lab data. This intercultural experience would undoubtedly be informative and contribute to the articulation and possible resolution of many of their differences.

### **Data and Analysis**

Seeking mutual understanding about issues such as where the interaction occurred (lab or elsewhere) and with what degree of intervention by the researcher would be an important intellectual effort, in which the discussion would be as valuable as the conclusions,

especially if it could identify areas of agreement as well as disagreement. The same is true for the last set of issues to be examined here, namely, the interpretation and analysis of the data.

**Minimum Criteria for Data.** One kind of data can be dismissed fairly quickly in either discipline: hypothetical instances of language in social interaction. Sacks (1984, p. 25) put it politely when he said that “if we use hypothetical, or hypothetical-typical versions of the world we are constrained by reference to what an audience, an audience of professionals, can accept as reasonable.” A blunter criticism would be that hypothetical examples are fatally constrained by the author’s previous observations and his or her freedom to select from or distort those observations. The examples may be interesting for what they reveal about the way the author views language in social interaction, but it is hard to see how they provide evidence for these views. It is surprising how often (especially in person but also in print) psychologists will reject experimental results they disagree with by offering hypothetical counterexamples that are purely anecdotal. But it is equally surprising, for different reasons, when LSI researchers include hypothetical-typical examples.

Another variation on hypothetical language is the presentation of experimenter-generated language, from which participants must choose, without speaking or writing their own. Our research team did this at the very beginning of a long-term research project on equivocation, out of cowardice about facing the full range of spontaneous equivocation that participants might produce. We did several experiments (Bavelas, 1983; Bavelas, Black, Chovil, & Mullett, 1990, chap. 4) in which we described a situation to the participants and then asked them to choose the reply that they would make in that situation from among the alternatives we provided. The messages we generated were intended to be in four categories: truthful, untruthful, tactful, and equivocal. However, when we later asked naive decoders to rate our messages on clarity/equivocation scales, our “tactful” messages were usually the most equivocal! Moreover, the naive decoders’ ratings predicted the participants’ choices and fit our theory better than our own intended meanings did. Thereafter, we let participants write or say their own responses, which naive decoders interpreted for us. As a result and only then did we begin to learn about the rich and varied world of equivocation. Before that, as Sacks (1984) pointed out, our research was constrained by our supposed expertise.

**Selection and Interpretation.** Even given that the language under study is the participant’s own, there is the complicated question of who interprets it. Interpretation begins with selection. The original language in a social interaction are never the data; the data are always selective and focused. One researcher may decide to ignore conversational repairs for his or her current purposes; another may decide to make them the primary data. Neither is obliged to study everything that occurs in a segment (even if that were possible), but rather to acknowledge the selection process. For example, Sacks (1992) freely admitted that he was omitting behaviors such as facial expression only because of the difficulty of studying them at the time and because his goals were more limited, not because they were unimportant or because “it wouldn’t be great to study them. It would be great to study them. It’s an absence” (p. 26).



Sampling is another kind of selection, and here the criticisms on both sides again reflect a double standard. Social psychologists criticize the ad hoc nature of the samples used by LSI researchers, who with equal justice criticize the opportunistic preponderance of undergraduate populations in experimental research. Neither group engages in the kind of random sampling procedures (of people, settings, or tasks) that would ensure generalizability—nor do they need to as long as they qualify their conclusions accordingly. There is in fact an opportunity hiding in these differences, which is the possibility of comparing results across very different samples. Our research group has had this opportunity at least twice: In his analysis of equivocation in the naturally occurring setting of news interviews, Bull (1998) replicated our lab results (Bavelas et al., 1990), and we (Bavelas et al., 2000) replicated Goodwin's (1986) field observations on listener responses in our lab.

Interpretation includes both selection and other more subtle decisions: Did something happen and what did it mean? Curiously, social psychology and LSI ultimately agree on the importance of these issues, but they fault each other at different levels. One of the sharpest divisions between social psychology and LSI is the issue of objectivity. Most social psychologists consider that LSI researchers' interpretations of participants' utterances and exchanges do not meet their standard of objectivity because interpretations are potentially idiosyncratic or even self-serving in the sense of producing data that fit the researchers' conclusions. "Anyone can make up a pretty story," as one social psychologist said about our research group's treatment of gestures and facial displays as acts of meaning, to be interpreted in the immediate conversational context (according to the model outlined in Bavelas & Chovil, 2000, where there are empirical rebuttals of his criticism). In this critic's view, looking for the meaning of language acts within the conversation (rather than imposing them from outside) is subjective.

Notice that the issue here is not whether something occurred; no one suggests that the LSI researcher made up the data or observed it badly—after all, there is a recording. The issue for the social psychologist is whether the act has the meaning that the LSI researcher attributes to it. Ironically, this is the same criticism LSI makes of social psychology practices, although at a slightly different level. Therefore, I will present that side for comparison before proposing a solution. Wood and Kroger (2000) articulated the social psychological blind spot concisely:

The problem with much work in social psychology . . . is that it looks at movements (e.g., the movement of the lever on the "shock" machine in the Milgram obedience experiments, the utterance "Line A is longer than Line B" in [Asch's] conformity studies) as unproblematically equivalent to their meanings, that is, as actions (e.g., obedience or conformity). There is a failure to recognize that to describe something as an action is to make an interpretation. . . . In most instances, the distinction is not acknowledged; the meanings are simply taken for granted. (p. 11)

Wood and Kroger's examples from these two classic experiments in social psychology (Asch, 1951; Milgram, 1963) illustrate several kinds of unacknowledged selection and interpretation. First, as we all know from films of the Milgram experiments (Milgram, 1965), the participants did not simply move a lever on the "shock" machine. Many protested,

even refused, and the experimenter countered with equally important discourse, such as assurances that there was no harm in proceeding. None of this became the formal data. Similarly, in the Asch experiments, none of the details of behavior of the confederates or participant (e.g., tone of voice) is part of the data analyzed, nor is the actual language in which the participant announced the choice (e.g., whether it was mitigated or qualified). In both studies, the details of the language and social interaction were relevant and could have been considered data, rather than these researchers' narrow definition of the participant's response.

Second, in both studies, the researcher imposed his own interpretation on the choice the participant made. Milgram called the choice "obedience" (or "destructive obedience"; 1963, p. 371) when the participant did what the experimenter wanted; others have called it "aggression." One might equally call it "reassurance as an interactional achievement."<sup>5</sup> Asch called his participants' choices "conformity," although others might call it "information averaging," that is, the participant pooled all of the information available, including the perceptions of others, as we often do in ordinary life.

Certainly both Milgram's and Asch's interpretations were due in part to their initial selections of data, which decontextualized the participants' decisions and made it easier to abstract them as obedience or the like. If, after studying the experimenter-participant interaction in the Milgram (1965) film, an LSI researcher described it as an interactional process of reassurance, how is this interpretation less objective than calling the same sequence obedience? The obedience and conformity interpretations strongly supported a unilateral view of social interaction consistent with mainstream social psychology, so there is good reason to question the objectivity of the interpretation. Those who do not look at the details of language in social interaction risk abstract glosses in which they and their theories determine the meaning of events.

My purpose in laying out the reciprocal criticisms is to suggest that these are variations on the same issue, which can be summarized as follows: One reason that social psychologists focus on physical actions such as the movement of a lever is that they consider it objective; everyone can probably agree on whether it occurred or not. However, as I have noted elsewhere, "objectivity is not a given; it is an accomplishment" (Bavelas, 1994, p. 214; see also Bavelas, 1995, pp. 52–53). There is no reified property of objectivity that adheres to some data because it is physical and not to others; there is only the probability that independent observers would agree that something occurred or not. However, especially when studying language and interaction (and perhaps in any kind of study), the meaning of the action is at least as important as its occurrence. Given that the participant moved the lever to the highest level, was the meaning of that action obedience? Simply to call it obedience is not an objective procedure. It is at this level that LSI researchers would justifiably criticize social psychological research in which the researcher decides the meaning of social action by fiat, which is exactly why social psychologists criticize LSI interpretations of data.

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<sup>5</sup>Oddly, the reassurance was in fact true. There was no harm in proceeding, because both the shock and its recipient were bogus. Thus, harmful "obedience" or even "aggression" could have occurred only in the mind of participants and only for those who believed the experimenter when he said that the experiment was real but disbelieved him when he said there would be no harm—which begins to sound like a postmodern novel!

The answer to this dilemma is neither extreme essentialism (there is one and only one real meaning, namely, the one I give it) nor extreme relativism (any meaning, including the one I give it, is equally valid). Rather, it is a matter of being clear and open about one's claims. To use the Milgram example one last time, an LSI researcher should set out explicit criteria for an interactional process of reassurance and show that the instances identified meet those criteria. Ideally, he or she should also describe actions that would not meet the criteria. The social psychologist should do the same, setting out detailed criteria for what does and does not constitute obedience and showing which instances do and do not meet these criteria.

There is an additional standard, which I have consistently advocated, that in each case, independent analysts would apply the criteria and agree; this intersubjective agreement would constitute objectivity at the level of interpretation. My standard is likely to rub both sides the wrong way. Although most psychologists would require interanalyst agreement whenever the hands-on data analysis involves interpretation, I am pushing that principle further into what many consider the theorist's province. And, although most LSI researchers would agree that simply describing a behavior as obedience or conformity is not sufficient, they may fail to examine their own practices for this error. In our research group's experience, explicit interanalyst agreement is valuable primarily because of the demonstrated salutary effect that the process of achieving it has on our observations and thinking: It requires us to be more explicit, clear, and specific than we had ever thought possible, constantly enforcing the discipline necessary for microanalysis. The reward is that it takes us beyond mundane physical movements and actions into the realm of their meanings, which is where social life is lived, while remaining grounded in the public and consensual enterprise of science.

**Statistical Analysis.** It is hard to imagine a difference on which social psychology and LSI are further apart than the use of statistics (Bavelas, 1995, p. 61). For the social psychologists, statistical analysis of data is the pinnacle of a journey that starts with a lab experiment, applies objective, quantitative measurement, and finally climaxes in a  $p$  value. For many LSI researchers, that journey is the road to perdition. By now, the reader will anticipate my pointing out that there is more than one road, and most of the steps along the way are independent of each other. There are statistics for nonlab, nonexperimental, qualitative data, and at least one paragon of experimental psychology, B. F. Skinner, eschewed the "statistical Leviathan" (1959, p. 370). To address the equally passionate commitments either to use or to avoid statistics, one must ask what they might do (and not do) for the researcher.

Descriptive statistics can organize the raw data. Means, medians, modes, percentages, and pie charts serve the same function as finding and articulating a pattern serves in LSI data, and both can also misrepresent: For example, reporting percentages for small  $N$ s distorts the findings by inflation (40% of a sample of 20 is only 8 people), as does the presentation of only positive instances in LSI.

Inferential statistics can help us when our intuitions fail. It is easy to see that a coin that lands heads half of the time is operating by pure chance; it is harder to tell whether a coin that lands heads seven times out of ten is not. Our research group (Bavelas, Coates, & Johnson, 2002) observed that listener responses in face-to-face dialogue tended

to fall in periods of mutual gaze, but this could easily have been attributed to chance coincidence of the two events. Only a statistical analysis could reveal that their coinciding was extremely unlikely to be chance. Virtually all psychologists are conservative in the sense of being concerned about seeing patterns that might be just chance, hence their reservations about LSI's lack of statistical tests for this possibility. However, there are a number of increasingly common statistical practices that completely undermine this primary purpose of statistics, such as interpreting trends in nonsignificant findings, not replicating findings, and especially the use of innumerable multivariate tests without clear hypotheses and adjustment for Type I error.

Finally, there are equally important analytical tasks that statistical analysis cannot do. A search of the literature for a chapter on discourse analysis of interpersonal conflict (Bavelas, Rogers, & Millar, 1985) turned up exactly such an example: The researchers had brought adolescents and their parents together for a discussion of conflictual issues. Moreover, they had recorded these discussions, transcribed them, and analyzed each utterance. However, although they were interested in the relationships between parent and child utterances, they did not analyze these relationships themselves. Instead, they turned to statistics at this crucial point, relying on statistical tests to identify any patterns in the interactions. The results were dreary, unconvincing, and completely forgettable. Statistics can find only what has been preserved in the data; they cannot make sense of interactional sequences that have been reduced to data about individuals.

## **CONCLUSION**

This chapter has examined the two solitudes of social psychology and LSI from the point of view of a researcher who works in both fields (as well as in other disciplines). This vantage point leads to familiarity with both of the solitudes and their views of each other and especially to an awareness of how much each has to offer and how unnecessary the mutual isolation may be. The issues that separate the two fields have been divided here into what they choose to study and how they choose to study it, which are undeniably important issues with many subsidiary ramifications. However, there is some common ground to be found by identifying key differences and re-examining them closely. In every instance, there seem to be ways to move closer, or at least to appreciate and learn from what the other is doing, without necessarily having to change one's own approach. However, a friendly warning is in order: Thinking carefully about these issues can lead to tolerance and even changes in one's own practices.

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Social Interaction: A social exchange between two or more individuals. social group: A collection of humans or animals that share certain characteristics, interact with one another, accept expectations and obligations as members of the group, and share a common identity. In sociology, social interaction is a dynamic sequence of social actions between individuals (or groups) who modify their actions and reactions due to actions by their interaction partner(s). Social interactions can be differentiated into accidental, repeated, regular and regulated. A social interaction is a social exchange between The two solitudes: reconciling social psychology and language and social interaction. In K. L. Fitch & R. E. Sanders (eds.), *Handbook of Language and Social Interaction* (pp. 179–200). Mahwah, NJ: Lawrence Erlbaum. Bavelas, J. B., & Chovil, N. (2000). Visible acts of meaning: an integrated message model of language use in face-to-face dialogue. *Journal of Language and Social Psychology*, 19, 163–194. The two-brain approach: how can mutually interacting brains teach us something about social interaction? *Frontiers in Human Neuroscience*, 6, 215. doi: 10.3389/fnhum.2012.00215. Krauss, R. M. (1987). The role of the listener: addressee influences on message formulation. *Journal of Language and Social Psychology*, 6, 81–97.