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WHAT IS COMMUNICATION SCIENCE?

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We provide both simple and complex answers to the question posed in this chapter's title. Before attempting to answer the question, however, it is necessary to consider briefly the historical context within which communication emerged as a social scientific discipline. Detailed treatments of the development of communication research are provided in the present volume (Bryant & Pribanic-Smith, Chapter 2, this volume) and elsewhere (Delia, 1987). Our extremely abbreviated excursion into this history is meant to provide a framework that offers a partial explanation for the shape of contemporary communication science as well as some tentative answers to the "What is communication science?" question.

◆ *Historical Perspective*

The social scientific study of human communication is a relatively recent development, one that emerged in the years following World War II (Bryant & Pribanic-Smith, Chapter 2, this volume; Delia, 1987). At that time, there was a great deal of optimism about the future of the social sciences in general, especially with regard to their relevance for

improving the conditions of society (Lewin, 1945). Many social sciences already had been established early in the 20th century; however, although studies of communication were conducted during this pre-World War II period, they were carried out by researchers who called psychology, sociology, and political science their disciplinary homes (Schramm, 1964). It was not until the late 1950s and early 1960s that cadres of communication researchers, educated in such communication-related departments as speech and journalism, began to appear on the scene. Thus, in relation to other social science disciplines, communication as a social science was a relatively late arrival to the social science family (McLeod, Chapter 11, Kosicki, & McLeod, this volume).

Because the social scientific study of communication took hold in speech and journalism departments that were then, and still are, dedicated to the inculcation of various communication skills—for example, oratorical and argumentation skills in the case of speech departments and writing and editing skills in the case of journalism departments—from the beginning there was a strong impetus to undertake research addressing practical aims. By using the rapidly developing arsenal of quantitative methods to attack important practical research questions, not only could communication skills be better taught to students, but pressing social problems also could be solved by improving communication effectiveness.

A case in point is the work done on communication and national development. During the 1950s and 1960s, considerable faith was vested in the proposition that the development of mass media systems in Third World countries would create a climate in which individuals would become better educated, less fatalistic, and more civically engaged, thus creating conditions favorable to political and economic development (Lerner, 1958, 1967; Rogers, 1962). Moreover, in these early decades of

the cold war, when there was great fear of a nuclear exchange between the United States and the Soviet Union, communication was seen by some as a key element in the amelioration of conflict (Bowers, 1974; see Ellis, Chapter 17, this volume; Sillars, Chapter 16, this volume) and the prevention of such a conflagration.

At the same time, there were increasing concerns about the potential deleterious effects of the then-new medium of television on members of its mass audiences, especially children (Himmelweit, Oppenheim, & Vince, 1958; Schramm, Lyle, & Parker, 1961). The role of violent television content in promoting aggressive and antisocial behavior, the degree to which soap opera viewing encourages negative views of married life, and the extent to which television consumption promotes escapism were presented as potential toxic effects of media exposure. These concerns about television's effects echoed those voiced almost three decades earlier in the Payne Fund studies of the effects of movies (Charters, 1933). The primary motivation of these inquiries was to demonstrate that such undesirable effects occurred among those exposed to particular media content. By using the tools of quantitative social science to document these negative effects, steps could be taken to modify or eliminate such noxious content.

These applied research foci were further buttressed by Paul Lazarsfeld's extensive research program conducted during the 1940s and 1950s that was dedicated to demonstrating a variety of mass media effects within such domains as politics and marketing (Benoit & Holbert, Chapter 25, this volume; Bryant & Pribanic-Smith, Chapter 2, this volume; Delia, 1987; McLeod et al., Chapter 11, this volume). Rather than being aimed at providing ammunition for public policy arguments concerning harmful media content, a substantial amount of this research was motivated by commercial interests, although such notions as the two-step flow hypothesis and opinion leadership

that emerged from this research corpus (Benoit & Holbert, Chapter 25, this volume; Katz & Lazarsfeld, 1955) assumed center stage in other lines of applied communication research (Rogers, 1962).

In view of past alarmist responses to the introduction of new communication technologies, quite predictably, the potential negative effects of “new media” such as the Internet on their users have recently become a focus of concern. These effects include Internet addiction (Young, 1998) and the possibility that excessive Internet use potentiates social loneliness (Kraut et al., 1998), although evidence suggests that social loneliness may not necessarily be associated with extensive Internet use (Kraut et al., 2002). Still others have noted that the cloak of anonymity surrounding Internet use may embolden individuals to insult others or to “flame” them—behaviors they would not normally display in most face-to-face encounters (Siegel, Dubrovsky, Kiesler, & McGuire, 1983). Thus, the tradition of revealing potential noxious effects of extensive media use continues to preoccupy researchers to the present.

As these developments unfolded within the mass communication domain, during the late 1950s and early 1960s, the first generation of communication researchers concerned with the study of face-to-face interaction emerged from graduate programs. Among this vanguard were Robert N. Bostrom, John Waite Bowers, and Gerald R. Miller from the University of Iowa and David K. Berlo, Erwin Bettinghaus, and Hideya Kumata from the University of Illinois. During the late 1950s and early 1960s, four of these early figures—Berlo, Bettinghaus, Kumata, and Miller—found their way to the newly formed Department of Communication at Michigan State University. Partially because of the rhetorical background from which some of the researchers emerged, they were interested in communication and persuasion and language behavior. Important touchstone works for them were the monographs that

emerged from psychologist Carl Hovland’s research program concerning the role communication plays in promoting attitude and behavior change (Hovland, Janis, & Kelley, 1953), social psychologist Leon Festinger’s cognitive dissonance theory (Festinger, 1957, 1964), and psychologist Charles Osgood’s research in the emerging area of psycholinguistics (Osgood, Suci, & Tannenbaum, 1957). In addition, Kumata had abiding research interests in the then-fledgling area of intercultural communication.

This highly abbreviated overview of communication science’s roots suggests at least three important antecedent conditions that continue to influence the ways in which social scientifically oriented communication research has evolved over the ensuing years. First, within the mass communication domain, the research formula *identify a potential toxic media effect* → *document the effect with research findings* → *ameliorate the noxious effect* through media literacy programs, legal intervention, or suasive discourse aimed at media industry decision makers continues to enjoy considerable cachet and financial support. Within this paradigm, theory may be invoked as a way of rationalizing the inquiry and provides it with the appearance of theoretical motivation, with social learning theory frequently being the theory of choice (Bandura, 1977, 1986), but the primary research purpose is to demonstrate that increasing exposure to the undesirable content potentiates socially undesirable attitudes or behaviors and to use the successful demonstration as evidence in advocacy efforts aimed at eliminating the offending content. Rarely does research with this primary motivation directly test theory in general or, even when it is invoked, social learning theory in particular (see Nabi & Oliver, Chapter 15, this volume). Demonstrating the robustness of the bivariate relationship between exposure to toxic content and its noxious effects, while controlling for possible “third variables,” is key. The formulation

outlined here is not confined solely to mass communication research; however, several researchers have noted the need for increased theoretical elaboration within this domain (Nabi & Oliver, Chapter 15, this volume; McLeod et al., Chapter 11, this volume; Pfau, 2008).

Second, the confluence of interests in communication and social influence processes, increased methodological sophistication, and external funding opportunities have encouraged communication researchers to attack applied communication problems in such contexts as disease prevention and health maintenance. Much of this applied work within the health communication domain has not been motivated by explicit theoretical concerns (see Atkin & Salmon, Chapter 24, this volume; Cegala & Street, Chapter 23, this volume), although again, “theory”—for example, psychological reactance theory (Brehm, 1966)—may sometimes be invoked in a speculative, post hoc fashion to account for communication campaign failures (see Dillard, Chapter 12, this volume). In addition, although establishing that certain “message features” may potentiate desired persuasive outcomes may have important implications for communication praxis, merely demonstrating such effects does not explain why they occur (Dillard, Chapter 12, this volume). Of course, producing successful practical outcomes can motivate theory development efforts; several persuasion theories that appeared during the 1950s and 1960s grew out of the applied persuasion research undertaken during World War II.

Third, because the frames of reference of early mass communication and interpersonal communication researchers emerged from such cognate areas as sociology, social psychology, and political science, when their research was informed by theory, the theory was frequently imported from the cognate discipline. For example, communication research concerning counterattitudinal advocacy (Miller & Burgoon, 1973) was

driven primarily by dissonance theory (Brehm & Cohen, 1962; Festinger, 1957, 1964) and incentive theory (Janis & Gilmore, 1965), both of which were imported from social psychology. These borrowing proclivities persist today. For example, the elaboration likelihood model (ELM) of persuasion (Petty & Cacioppo, 1986; Petty & Wegener, 1999), a model that is frequently invoked by communication researchers, originated in social psychology (see Dillard, Chapter 12, this volume). In general, then, these three antecedent conditions and others created a climate that attenuated theory development within the communication science domain and precipitated a theoretical trade deficit in which communication became the debtor discipline (Berger, 1991), although broader purviews of the communication discipline that include humanistic approaches may provide more evidence of original theory development efforts (Craig, 1993). In addition, we recognize that in a few instances, theories imported from other disciplines have been extensively elaborated and extended by communication scientists (Lang, 2000).

The first two of these three antecedent conditions propelled communication researchers in the direction of increased methodological sophistication. From the great expectations surrounding the introduction in the 1960s of high-speed, main-frame computing that enabled researchers to analyze ever-increasing numbers of variables, to contemporary fascinations with structural equation modeling and hierarchical linear modeling that are now the stuff of laptop computers, there has been an abiding faith in the notion that sophisticated methodological tools will yield large increments in our understanding of communication processes. Although we certainly agree that such tools are critical in plying the communication research trade, absent theory, they are, after all, merely tools. It is only when theory animates their use that these tools become truly powerful. In short,

“Data without theory are like babies without parents: Their life expectancy is low” (Gigerenzer, 2000, p. 296). Communication science, then, is all about explanation, and explanation is all about theory development (see Pavitt, Chapter 3, this volume).

In providing this short answer to the question of what communication science is, we do not mean to imply that applied communication research should be relegated to second-class status. To the contrary, for those who persist in bifurcating theory and application, we would remind them that the understanding theory provides is more likely to yield successful interventions in practical affairs than are intuitively driven interventions and campaigns based on trial and error (see Atkin & Salmon, Chapter 24, this volume; Dillard, Chapter 12, this volume; Lewin, 1945), even when the trial-and-error-based programs are evaluated with highly sophisticated research methods. Engineering advances based on applications of basic physical science principles provide ample evidence of the synergistic relationship between theory and application. Of course, theory-based interventions presuppose theory on which to predicate desired interventions. We remain somewhat skeptical that contemporary communication theories yet provide stable enough launching pads for such applied efforts (see Cegala & Street, Chapter 23, this volume; Dillard, Chapter 12, this volume). However, we are optimistic about prospects for remedying this lack through increased attention to theory construction within the communication science community (Pfau, 2008). Ultimately, we are reminded of Kurt Lewin, a “founding father” of both social psychology and communication research (Schramm, 1964), who once observed, “Nothing is as practical as a good theory” (Lewin, 1945, p. 129). Now that we have provided some tentative answers to the query, “What is communication science?” we now consider more complex issues.

◆ *Regularities in Communicative Conduct*

As we have already observed, communication science involves something more than using sophisticated and powerful research methods and statistical techniques to assay hypotheses and something more than trying to solve various practical communication problems, for example, devising persuasive messages to encourage people older than 40 years of age to have yearly physical examinations. Communication science practitioners not only wish to identify regularities with respect to communication and its effects but also fundamentally seek to explain why such regularities occur (see Pavitt, Chapter 3, this volume). For communication scientists, the problem to be solved is one of identifying and then explaining regularities by constructing and testing theories.

In the physical world, observing that the sun rises in the east and sets in the west and that its path through the sky changes in a highly predictable way with the changes of the seasons permits generalizations about the sun’s behavior; however, by themselves, these generalizations do not explain why the sun behaves in this fashion. Indeed, alternative explanations for these observed regularities precipitated heated conflict between Galileo, an advocate of Copernican explanation, and the Roman Catholic Church concerning the center of the universe. Does the earth rotate around the sun or is the earth at the center of the universe, with all other celestial bodies rotating around it? These theoretical debates almost cost Galileo his life. In this case, the observed regularities were not in doubt—indeed, the sun rises in the east and sets in the west—but their explanation most certainly was.

Although examples of regularities in the physical world readily come to mind, some might argue that in the domain of social action and communication, such

regularities are rare, thus raising the issue of whether explanation is a useful pursuit in which to engage. After all, if there are but few regularities to be found in social and communicative commerce, why be concerned with their explanation? There are at least three critical responses to this question. First, the view that physical phenomena are necessarily highly predictable is somewhat naive. There are many physical phenomena that demonstrate stochastic behavior that makes their prediction and explanation difficult. That is, as is the case with the social world, probabilistic relationships abound in the physical world; nonetheless, physical scientists seek to provide explanations for them. Moreover, nonlinear, dynamic systems that may appear to be highly unpredictable and chaotic may, in fact, be predictable, thus calling into question the usefulness of the “predictable-unpredictable” bipolarity in the first place (Kelso & Engstrom, 2006). Rather than being a matter of “either predictable or unpredictable,” it may be more a matter of both “either-or” and “both-and.”

Second, some have suggested that conversational behavior and language use are considerably more routinized than might be supposed. Because goals recur in everyday social life, conversational routines have evolved so that social actors can satisfy their goals efficiently (Coulmas, 1981). Linguists have estimated that up to 70% of the utterances made in daily social intercourse are formulaic (Sinclair, 1991; Wray & Perkins, 2000). This formulaticity is postulated to be necessary because of both recurrence of goals and the considerable cognitive load requirements associated with message production (see Berger, Chapter 7, this volume). Because of the cognitive demands involved in performing social interaction, individuals typically do not have the time to formulate highly unique utterances. Furthermore, the instrumental use of communication to achieve mundane, recurring goals efficiently simply does not require such

uniqueness (Clark, 1994; Wittgenstein, 1953). Although these observations concern language use in social interaction contexts, given the recurring nature of audience members’ goals, we assume that much media content also exhibits considerable formulaticity.

Finally, if regularities in communicative commerce are indeed rare, efforts to devise effective communication “strategies” for achieving practical outcomes such as persuasion would seem to be doomed to failure from their beginnings. Attempting to be “strategic” in the face of abjectly capricious systems, whether the systems are individual or social, would seem like embarking on a fool’s errand. Thus, those who design messages to induce some desired change in audience behavior, attitudes, beliefs, or some combination of the three must at least behave “as if” there are “message features” that will be instrumental in bringing about the desired changes (see Dillard, Chapter 12, this volume). The supposed linkages between “message features” and desired changes may be intuitively derived, but nevertheless, even these intuitions represent naive generalizations about what “message features” will prove to be “effective” with specific types of audience members or audiences (see Benoit & Holbert, Chapter 25, this volume). If social actors behave in highly capricious ways, practical attempts to influence audiences would seem to be not worth undertaking.

Such methodological concerns as reliability and validity of measures and the internal and external validity of experiments, as well as the degree to which stimulus sampling permits generalization across message types (see Boster & Sherry, Chapter 4, this volume), are issues that must be addressed in the process of identifying to-be-explained regularities. However, even when the canons of “best methodological practice” have been observed and the regularity duly identified, its explanation remains problematic. One can demonstrate repeatedly that varying a set of

antecedent conditions reliably produces a certain set of outcomes, even when potential mediators and moderators are taken into account, but still have to clear the hurdle of explaining why the reliable covariation between the antecedents and consequences occurs, as well as their mediation and moderation. While a useful step in the right direction, becoming swept up in mediation-moderation mania by itself does not necessarily yield theoretical explanations. As observed earlier, theoretical disputes arise even when all parties to them agree on the nature of the observed covariation, including its mediation and moderation.

Although certifying the reliability of covariation, causal or otherwise, between variables through replication studies is critical to the scientific enterprise, the value of replication studies typically has been diminished in current communication science praxis. While this lack of attention to replication is sometimes bemoaned (see Benoit & Holbert, Chapter 25, this volume; Nabi & Oliver, Chapter 15, this volume) and most agree that more replication studies would be better, replication is less valued than the presentation of new findings. Sometimes individual studies purported to demonstrate a relationship between variables are followed by later-published studies that fail to replicate the original findings. However, given the tendency to devalue replication research, frequently on the grounds that it is not “innovative” or “cutting edge,” we wonder how many unchallenged published findings would survive the test of studies specifically aimed at their replication. We further wonder how many failures to replicate findings remain unpublished and forever sealed in the files of researchers whose work was rejected by publication outlets. One way to obviate this problem would entail requiring researchers to report a series of studies that adduce replication evidence, rather than just a single demonstration of the key relationship or relationships. Meta-analysis is beginning to play a key

role in determining the robustness of specific relationships across studies and potential moderators of these relationships (Allen & Preiss, 1998; Allen, Burrell, Gayle, & Preiss, 2002; Preiss, Gayle, Burrell, Allen, & Bryant, 2007). Having established that a phenomenon displays regularity, we turn to the issue of theory and its role in communication science.

◆ *Where Do Communication Theories Come From?*

In dealing with issues related to theory and its role in explanation, we do not wish to cover the territory that is aptly presented in Chapter 3 (see Pavitt, Chapter 3, this volume). Instead, we will identify some strategies for generating theoretical explanations for communication phenomena. Before doing so, however, we will enumerate some misuses of the term *theory* that can be found in the current communication science literature. We hope that by identifying at least some of these faux theories, researchers will avoid the pitfalls associated with promoting the illusion of theory without its substance.

PHENOMENON + “THEORY” ≠ THEORY

Unfortunately, the term *theory* is sometimes misused to characterize a body of research that merely demonstrates that a particular phenomenon occurs or can be produced under a set of conditions. Merely appending the term *theory* to the phenomenon does not constitute a theory. Examples of this kind of error are currently evident in the mass communication effects literature (see Nabi & Oliver, Chapter 15, this volume). Thus, a series of studies designed to demonstrate that priming or framing effects can be induced under a certain set of conditions or within a specific

context may be portrayed as evidence for “priming theory” or “framing theory” when in fact, the studies themselves have only demonstrated that the priming or framing effects have occurred. Although observing that news stories “framed” in a particular way reliably give rise to particular types of attributions and interpretations may have important implications for understanding the formation of individual and public opinion, demonstrating these relationships by itself does not explain how the content of the news stories so framed is transmuted into the interpretations, attributions, and opinions. It is one thing to identify the effects of various news story frames, but it is quite another to explain why the observed framing effects occur.

We assume that priming and framing effects arise out of interactions among a variety of cognitive structures and processes. Until the structures and processes are identified and their interactions understood, there can be no “priming theory” or “framing theory” that provides an explanation for why such effects occur. As noted previously, identified regularities need to be explained by recourse to mechanisms that account for the regularity in question. Moreover, while demonstrating that the regularity can be replicated across contexts speaks to the generality of the regularity, generalization studies by themselves do not explain. It is only when research is predicated on the desire to answer the “why” question that one can lay claim to engaging in “theoretically driven” research.

THEORY ≠ MODEL

Another unfortunate error, one that has become increasingly more frequent with the advent of more sophisticated causal modeling techniques that can be executed with point-and-click ease, is that of equating models and theories. It is not uncommon to find the terms *theory* and *model* used interchangeably. There are obvious examples of

models that offer little in the way of theoretical explanation. A working, radio-controlled model of an airplane that a child successfully flies does not by itself provide a theoretical explanation of the aerodynamics of flight. Similarly, regression models that identify independent variables that account for variability in a dependent variable, by themselves, provide no theoretical account for the shared variance. In this regard, the term *explained variance* in this regression context is misleading. No matter how high the coefficient of determination (r^2) values may be, they do not provide theoretical explanations for the relationships they characterize.

We do not mean to imply that such statistical modeling techniques as hierarchical linear modeling and structural equation modeling are not useful; indeed, when properly used, they are (see Boster & Sherry, Chapter 4, this volume). However, even when models are found to produce much-coveted high R^2 values or highly encouraging goodness-of-fit statistics indicating strong concordance between models and data, the task of theoretical explanation may have only just begun, especially if the evaluated model is itself not anchored in theory. Moreover, if the encouraging goodness-of-fit statistics have been achieved by post hoc “model trimming,” there should be even less cause for celebration. The manifold and complex relationships between theories and models are explicated in detail elsewhere in this handbook (see Pavitt, Chapter 3, this volume). Here, we simply caution that the two should not be conflated.

THEORY-LADENNESS OF OBSERVATIONS

Since the 1950s, there has been a continuing debate concerning the degree to which scientific observations are influenced by theoretical (Hanson, 1958) or paradigmatic (Kuhn, 1970) commitments. In contradistinction to the positivist claim

that observations consist of raw sensory experience, free from the influence of observers' theoretical affinities, these philosophers suggest that theories and broader paradigmatic perspectives that researchers embrace are likely to influence their judgments of what observations count as relevant data and the degree to which data comport with theories or paradigms to which they subscribe, although Hanson (1958) recognized that language can be used to describe sensory experience in a way that is not theory laden, and Kuhn (1970) suggested that observed anomalies may undermine expectations based on theoretical or paradigmatic commitments (Heidelberger, 2003). In any case, philosophical positions like these have been invoked to raise fundamental questions about the ability of researchers to evaluate theories and hypotheses in ways that avoid potential biases.

In contrast to these views of theory-ladenness, Duhem (1906/1974) recognized that experiments might be predicated on two different motivations. First, experiments might be designed simply to determine whether a causal relationship obtains between two sets of events. A naive laboratory assistant with no knowledge of the theories relevant to the events being observed could carry out such experiments. A second and very different motivation for experimentation is to test theory. In this case, the naive laboratory assistant might be able to produce the predicted results but would be unable to interpret them absent a formal theoretical framework; that is, the assistant could report the results but not understand their significance. Thus, in Duhem's view, it is possible to make observations that are minimally influenced by theory. Duhem further argued that experiments not motivated by theory are more likely to be performed in less developed sciences, perhaps a reasonable characterization of present-day communication science.

Although we find Duhem's distinction between the two different motivations for

experimental inquiry plausible, it may be useful to distinguish between intuitive or implicit theories, on one hand, and formally explicated theories, on the other. This distinction is consistent with dual-process approaches to information processing (Stanovich, 2002). Intuitive theories are theories formed through interactions with the physical and social world. They consist of preconsciously processed associations represented in memory that are based on informal observations (Gigerenzer, 2000, 2007; Hogarth, 2001, 2005; Stanovich, 2002). Such intuitively derived theories are the product of System 1 (Kahneman & Frederick, 2002) or experiential processing (Epstein, 1994, 2003)—processing that is affect laden, preconscious, and relatively effortless. By contrast, formal theories are the products of formal reasoning that is governed by logical inference rules. Such theories are the product of System 2 or the rational processing system, which is affect free, conscious, and effortful.

Given the distinction between formal and informal theories and Duhem's differentiation between causality-establishing and theory-testing experiments, it is reasonable to contend that even when experiments are performed merely to establish causal connections between variables in the absence of a formal theory, intuitively based, informal theories guide the selection of plausible causal antecedents for experimental assay. This kind of intuitive, preconscious winnowing process is likely to give rise to the much-vaunted "aha" experience sometimes romantically associated with scientific discovery. Indeed, we suspect that in many communication laboratories, researchers frequently come to work in the morning with "new ideas" that have mysteriously "bubbled up" from their associative-oriented intuitive systems during the night. Because much intuitive knowledge is derived from observations of covariation between variables (Hogarth, 2001; Reber, 1993), it seems plausible that dramatic scientific breakthroughs attributed

to creative processes that involve the sudden and unexpected experience of “seeing connections” between phenomena that have not been heretofore perceived as related are at least partially the product of System 1 processing.

In our view, then, all observations are in a sense “theory laden,” but the nature of the theory informing them may vary. The appearance of “theory-free” inquiry is merely that; however, theoretical predilections, emanating from either formal or informal theories, may exert varying levels of influence on observations. More important, although informal, intuitive System 1 theories may provide researchers with potentially valuable hunches for productive research avenues and creative insights, they are no substitute for formally articulated System 2 theories. The preconscious nature of System 1 makes it difficult if not impossible to encapsulate its theories in natural language. System 2 theories can be assessed for their internal consistency, and because they are expressed in propositional form and are thus less subject to ambiguity, they are more amenable to falsification, if they are indeed false. In addition, because System 2 theories can be expressed in natural language or mathematics, they can be communicated more easily and shared among research community members. Thus, because System 2 theories are explicit, they reinforce the public nature of science and also facilitate replication attempts by geographically distributed researchers.

CONFIRMATORY BIASES AND FALSIFICATIONISM

Closely related to the theory-ladenness of observations issue are the confirmatory biases they may promote and attempts to identify and counter these biases. When individuals are given a hypothesis to test concerning another person’s personality—for example, whether the person is an introvert—those testing the hypothesis will tend to ask questions in a way that will confirm their hypothesis (Snyder, 1981). Social actors’ information-gathering proclivities are biased in the direction of confirming their naive hypotheses about others. This bias may arise from paying inordinate attention to cases that comport with the predicted co-occurrences between variables and ignoring co-occurrences that deviate from those hypothesized or developing hypotheses after the results are known, thus promoting illusory theory conformation (Kerr, 1998). It is doubtful that researchers are immune from these biases, although adequate training in research methods should include careful consideration of such issues. Table 1.1 illustrates a common bivariate situation that might encourage confirmatory bias.

Researchers vested in supporting the hypothesis that those who expose themselves to media violence will be more likely to display aggressive behavior are probably more predisposed to focus on cases that fall in the hypothesis-confirming Absent-Absent and Present-Present cells

Table 1.1 Bivariate Confirmatory Bias Scenario

	<i>Aggressive Behavior</i>	
	<i>Absent</i>	<i>Present</i>
Exposure to media violence		
Present	Disconfirming	Confirming
Absent	Confirming	Disconfirming

of the table than the cases that fall in the disconfirming cells. From the point of view of answering the question of why individuals vary in their propensities to manifest aggressive behavior, this confirmatory orientation is problematic for at least two reasons. First, relatively low correlations ($r = .20$ to $.30$) between exposure to media violence and aggressive behavior have been obtained in nonexperimental studies (Huesmann & Taylor, 2006). Although statistically significant and of potential practical significance (Rosenthal, Rosnow, & Rubin, 2000), this relatively low correlation indicates that numerous cases fall into the disconfirming cells of Table 1.1. A second danger is that an intense focus on exposure to violent media content as a cause of aggressive behavior may blind researchers to other, potentially more important causal antecedents of aggressive behavior, such as being raised in a family with a history of family violence. Again, the relatively low correlation suggests that factors other than exposure to media violence act to potentiate aggressive behavior, and these factors may account for more variance in aggression.

Media reform-minded researchers and perhaps those receiving funding from some advocacy groups might be especially prone to fall victim to the confirmatory bias in the above example because their agenda entails demonstrating a positive relationship between exposure to media violence and aggressive behavior. By contrast, researchers operating from a theoretical perspective that is more catholic might be more prone to take seriously the task of trying to explain the “disconfirming” cases. For advocates with vested interests in confirming the relationship between exposure to media violence and aggressive behavior, the disconfirming cells represent a pesky nuisance; by contrast, theoretically motivated researchers would seek to identify the mechanisms that explain both confirming and disconfirming cases. Indeed,

explanations frequently proffered for the relationship between exposure to media violence and violent behavior—for example, social learning and priming—do not necessarily offer coherent explanations for the disconfirming cells. Of course, individuals with theoretical commitments can fall victim to the confirmatory bias (Kerr, 1998); however, researchers whose inquiries are motivated by strong ideological or policy positions that entail specific predictions are probably more likely to do so (McIntyre, 2006).

One potential antidote for this confirmatory bias is for researchers to actively seek disconfirming evidence for their theories. Karl Popper (1963) suggested that researchers should purposely attempt to disconfirm their hypotheses rather than confirm them. He argued that repeated failures to disconfirm hypotheses derived from a theory allow researchers to place greater credence in the theory. In addition, theories themselves may be structured in ways that make their falsification difficult if not impossible. In particular, theories that postulate processes for which there are no independent measures allow those promulgating them the “escape clause” of explaining failures to confirm hypotheses by recourse to the claim that the critical, unmeasured processes were not at work in the disconfirming study. Obviously, in such cases, theories cannot be shown to be false when in fact they may be. Although the plausibility of the notions of theory confirmation and theory disconfirmation can be questioned on logical grounds (Cappella, 1997), confirmatory biases are problematic enough to warrant substantial concern (Kerr, 1998).

THEORY DEVELOPMENT STRATEGIES

To this point, our explication of communication science has focused on a series of cautions, including faux theories,

model-theory confusion, theory-ladenness of observations, and the confirmatory bias and falsification. We now assume a more affirmative stance and outline strategies for devising communication theories. Theory construction is a creative enterprise that is at the same time both intellectually risky and highly entertaining (Berger, 1991). Well-articulated theories that give rise to unambiguous predictions may prove to be false, thus representing a potential threat to the theory developer's self-esteem. Put another way, at least for some, it is much less risky and ego threatening to test someone else's theory or theories than to make public one's own, clearly articulated theory and risk the possibility of its disconfirmation.

We cannot possibly present here a course of therapy designed to assuage the disconfirmation fears of potential theory builders. We only observe that empirical confrontations between theories are one sign of a vital and dynamic scientific enterprise. These confrontations and the debates they precipitate are crucial for the advancement of disciplines. However, even in an earlier era, some expressed the view that communication science practitioners have shown too little evidence of such theoretical controversy (Phillips, 1981), although others disagreed with this indictment (Miller, 1981). Surely, over the past two decades, the level of theory development activity within some sectors of the communication science community has increased, but levels of theoretical confrontation remain relatively low. This state of affairs may be a by-product of the relative youth of the discipline, its theory-borrowing proclivities, and a situation in which the critical mass of researchers is simply too small to coalesce into opposing theoretical camps. Whatever the reason, we look forward to the day when conferences and journals feature more papers in which competing theoretical explanations are at stake. In the interest of hastening that day's arrival, we turn to strategies for developing theory.

Finding Useful Metaphors and Analogies. Metaphors and analogies have frequently been used as a basis for developing theories—indeed, for rationalizing entire realms of inquiry. In the latter case, since the 1960s, the digital computer with its input, working memory, memory buffers, scripts, long-term store, and output has been employed as a metaphor for various information-processing theories and models. It is probably no accident that advances in computer technology have paralleled the so-called cognitive revolution, although with increased understanding of the neurophysiology and neurochemistry of the human brain, the computer metaphor may cease to be the guiding analogy for information-processing theory. Another well-known example is McGuire's (1964) use of an analogy taken from immunology to formulate his inoculation theory of resistance to persuasion.

We caution would-be theory builders that merely likening communication processes to some other process—for example, neurochemical “communication” that takes place within the human brain's neural networks—does not by itself explain anything about the communication process so analogized. The proposed metaphor, no matter how intuitively compelling and persuasive, does not alleviate the theorists' obligation to propose a theoretical framework that provides an explanation for the communication process of interest. Metaphors and analogies are potentially highly useful starting points for theory development, but they are not the endpoints.

Conflicting Findings. We have already emphasized the need to establish the reliability of regularities before undertaking their explanation; however, a theory development strategy that relies on explaining empirical inconsistencies may motivate theory construction efforts. In this case, contradictory findings gleaned from a body of research are scrutinized for differences among studies that may have been

responsible for producing the seeming inconsistency. If artifactual explanations for inconsistencies between or among studies can be set aside, and if the conflicting findings stem from studies performed in the service of testing theories, it may be possible to propose theoretical explanations that account for the apparent discrepancies. Moreover, even if the conflicting empirical studies were not necessarily theoretically motivated, researchers can search for variables that may account for the observed discrepancies. Having identified plausible candidates, researchers can propose further studies designed to assess the source of the discrepancies that include the presumed critical variables, but they can also advance and evaluate theoretical accounts for their predictions.

Unfortunately, sometimes overly large and active egos get in the way of such reconciliation efforts. That is, when predictions derived from theory are not supported by data, and especially when those adducing the data are not among the theorist's "camp followers," some theorists may respond by vociferously impugning the disconfirming studies rather than by entertaining the possibility that at least some aspect of their theory may be in error. Of course, studies and the data they produce may be flawed; however, if theoretically derived predictions are unsupported by a series of studies featuring replication attempts with variations designed to mitigate potential methodological flaws, there comes a time when confronted theorists must capitulate to such evidence. Indeed, in our experience, it is cause for considerable celebration if over 60% of the hypotheses tested in a given study are supported; consequently, if researchers cannot adopt a reasonably appropriate level of humility in the process of advancing a theory, their data ultimately will very likely humble them.

Anticipating Alternative Explanations. Attempts to devise theoretical explanations for regularities ideally should both

acknowledge and try to take into account alternative explanations. In some cases, the problem is that in proposing a theory, the potential for alternative explanations is not recognized or is recognized but not explicitly acknowledged. However, it is one thing to acknowledge explicitly explanatory competitors, but it is quite another to demonstrate how the proposed theory takes into account the competitors. One alternative to a priori specification of alternative explanations is to let other members of the research community identify and propose them. Of course, this process will play out over time, no matter how many alternatives are considered within the context of a given theory; however, there are efficiencies to be realized if theorists explicitly recognize the existence of alternative explanations and deal with them in their theorizing. Furthermore, by taking such alternatives explicitly into account, theories may become more comprehensive and potentially more powerful. Explicitly entertaining alternative explanations is a manifestation of the falsificationist perspective discussed previously (Popper, 1963).

Surrounding One's Self With Skeptics. Proclivities to fall prey to the confirmatory bias suggest the wisdom of consciously seeking the counsel of those who are likely to be uncongenial or even hostile to one's theoretical arguments. While some scientific theorists and most politicians may derive considerable psychological comfort from the validation they receive from like-minded colleagues, the "cold fusion" and Bay of Pigs fiascos in the realms of physics and public policy, respectively, demonstrate the degraded judgments and decisions that can eventuate from the psychological comfort afforded by congenial supporters (Janis, 1972). It is not hyperbolic to represent groupthink as anathema to scientific theorizing; nevertheless, even though the deleterious effects of groupthink have been clearly demonstrated, many researchers seem to prefer to affiliate with like-minded colleagues and to

minimize their interactions with those who do not share their theoretical orientations. The social organization of professional associations and the research areas reflected in journals speak to the proclivities of researchers to adhere to the “birds of a feather flock together” aphorism.

Although we have emphasized the valuable role critics can play in the theory generation process, we would also suggest that research motivated primarily by public policy and other practical concerns might benefit even more from the responses of skeptics. Individuals whose political and ideological predilections lure them in the direction of confirming hypotheses compatible with their ideological and political leanings surely would benefit from interactions with those who do not share or may intensely oppose their ideological predispositions. Thus, for example, research conducted to assess the relationship between the toys that children play with, for example, toy guns, and their proclivities toward aggressive behavior that is initiated by those who favor gun control might well be designed in such a way that the probability of supporting the hypothesized relationship is increased. What is counted as “aggressive behavior” or “toy guns” might be decided in such a way that the study is biased toward confirming a positive relationship. For example, do such behaviors as verbal or nonverbal insults count as aggressive acts? By involving those opposed to gun control in the study’s design, confirmatory bias pitfalls might be avoided. Again, we recognize that the ideal of including skeptics in theory construction and research design is one that, for the most part, remains a faint outline on a distant horizon.

Letting Intuition Work. Over the past two decades, it has become apparent that a great deal of social perception, judgment, and action result from mental activity that is preconscious and automatic rather than

conscious and calculating (Dijksterhuis, Chartrand, & Aarts, 2007; Moors & De Houwer, 2007). We have already observed that System 1 or the intuitive system is postulated to operate in this way and that even when individuals perform experiments or collect data that are not designed to test an explicit theory, naive, tacit theories may surreptitiously guide their inquiry. However, this distinction between automatic and controlled processing may not be quite as stark as it is sometimes portrayed. People may be conscious of the products of automatic processes but not of the processes themselves. That is, the outputs of these automatic processes are consciously experienced as perceptions, judgments, and actions, as well as “ideas.”

It is hardly far-fetched to suppose that repeated exposure to a wide variety of research literatures tends to automate the knowledge contained in them such that System 1 might establish automatically connections between literatures—relationships that were not consciously perceived by the theorist when exposed to the literatures in question. Some have argued that good science results, in part, from exposure to a wide variety of ideas, some of which may be far afield from one’s own interests (Thagard, 2006). This kind of catholic purview might well encourage the kind of analogic thinking that has proven so valuable in theory generation. At times, such analogic thinking might be practiced at the System 2 or analytic level, as it was by McGuire (1964) in formulating his inoculation theory. However, at other times, automatically drawn analogies may remain preconscious and give rise to the conscious experience of only vague, gut-level feelings or intuitions concerning explanations for phenomena.

We believe it is important to heed these gut-level feelings, for such feelings and intuitions are probably the product of the same system that enables people to have the experience of falling asleep, having unsuccessfully solved a difficult problem,

only to wake up the next morning with the problem's solution. Indeed, temporal separation of an individual's guesses significantly improves the accuracy of their average (Vul & Pashler, 2008). Of course, these intuitions must be subjected to close scrutiny and ultimately be made part of an explicit theory; however, they may prove to be a useful departure point for theory development. Anyone who has done science for a reasonable period of time knows that the full panorama of emotions is operative in scientific endeavors (Thagard, 2006). Thus, dismissing the role that System 1 plays in theory development would be an egregious error.

Be Not Intimidated by Ideologues. Those who theorize about communication and social action should be aware of the possibility that their theoretical explanations may sometimes be at variance with the beliefs of those who subscribe to a variety of political and religious ideologies. In commenting on the chilling effect of political ideology on social scientific inquiry, McIntyre (2006) observed,

At times entire areas of research have been ruled out of bounds simply because their investigation may uncover facts that cause us to question favored ideological assumptions (or violate the basic human prejudice that humans are somehow special). In some instances, this had led to the suggestion that social scientists cannot settle an empirical dispute, simply because it raises political issues that are "controversial." In other instances, well-confirmed facts about human behavior have been rejected altogether simply because they contradict popular political ideology. In this way, the corrosive effect of political ideology reveals its similarity to age-old attempts to resist knowledge, seen throughout the history of scientific inquiry. (p. 62)

We cannot possibly address here the vast panorama of ethical issues associated with the conduct of social scientific inquiry. However, unfettered inquiries, conducted in accordance with the highest

ethical standards, which allow researchers to go where their theories and data take them, are very likely to be most productive. The self-correcting nature of scientific inquiry will winnow those theories that are in error and those data that are flawed. The alternative of dogmatic adherence to "ideologically correct" explanations, regardless of their accuracy, is akin to the situation that existed during the Dark Ages (McIntyre, 2006). We submit that social reform attempts based on sound theoretical principles and reliable data are preferable to attempts predicated on dogma of any stripe.

◆ Conclusion

The discerning reader may have noted that we have not presented arguments concerning the relative desirability of qualitative and quantitative research methods. Debates concerning these methodologies are legion in the communication discipline. In our view, the significance of these debates pales in comparison to the problem of erecting a body of theory that explains communicative action. As we hope we have demonstrated here, sophisticated research methods of any type in the hands of atheoretically inclined researchers are very likely to yield relatively little insight into the nature of communication processes. Asking fundamental "why" questions and constructing and evaluating theories that answer them are activities that are at the very heart of studying communication as a social science.

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