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2

Quantitative Methods for Conflict Communication Research, With Special Reference to Culture

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In their book on processes of social conflict, Pruitt and Carnevale (1993) noted the difficulties in studying conflict. Experimental studies provide control of extraneous variables, but in a simulated setting. On the other hand, manipulating conflict in laboratory or field experiments raises other concerns: If the manipulation is effective, the ethics of the investigation may be problematic; if the manipulation is ineffective, the lack of internal validity means that the investigation cannot inform us about conflict processes. Naturalistic studies are hard to come by, and, when available, have their own problems of internal and external validity (see Cook & Campbell, 1979). Furthermore, in everyday settings individuals and organizations are often hesitant to allow researchers to observe sensitive, confidential, or private interactions associated with conflict.

Despite these challenges, conflict research has made tremendous progress in the past 20 years. In addition to the breadth of conflict venues addressed, a variety of research methods—including quantitative, qualitative, and rhetorical methods—has been developed for studying social conflict. The purpose of this chapter is to examine the types of methods used to study conflict communication at various levels

of analysis, and to suggest the types of analyses that are likely to be effective for future research.

First, we decided to focus this chapter on quantitative issues for several reasons. Qualitative methods, such as participant observation and unstructured interviews, are often used to determine the universe of meaning (i.e., the full range of connotations in addition to the denotations) of a phenomenon, the types of individuals relevant to a domain of investigation, the operative categories employed by cultural participants (i.e., the emic constructs; see Pike, 1967), and the causal attributions provided by individual participants or observers (for discussion of these methods, see Denzin & Lincoln, 2000). For this chapter, we view the goal of conflict research as the creation of general and abstract social science theory, which entails prediction as well as description. To this end, qualitative investigations, however valuable, serve as precursors to studies that explicitly take into account measurement validity, internal validity, and external validity, or, in short, quantitative investigations. Furthermore, although there exist "naïve theories" that can be well explicated by qualitative methods (see Heider, 1958), quantitative investigations are needed to check the naïve causal attributions made by cultural informants (see, e.g., Nisbett & Wilson, 1977). Finally, the literature on quantitative methods germane to conflict communication research is so extensive that even restricting our focus to these methods leaves us with little opportunity to cover them all.

Second, we chose to highlight methods that are especially relevant to cross-cultural and other group comparisons. Like many areas of investigation, the study of conflict communication began with obvious and important applied questions: How do we avoid war? Can we minimize strife within families? Can labor and management cooperate in the modern corporation? These questions lend themselves to group comparisons: warring versus non-warring countries; conflictual versus non-conflictual families; companies with cooperative labor-management relations versus those with records of labor-management clashes.

Group differences play a major role in most studies of conflict, from comparing gender differences in resolving relational conflict to cultural differences in conflict styles. As a result, a good deal of our discussion is spent examining group differences; most of this discussion considers cross-cultural comparisons, but the methods we examine apply equally well to comparing conflict in hierarchical versus nonhierarchical organizations, cross-gender versus same-gender interpersonal relationships, or people from Southern versus non-Southern U.S. states (for the latter, see Nisbett & Cohen, 1996). We hope that our discussion will alert scholars to significant considerations for research on conflict, their methodological challenges, and the needs of future research.

With increased knowledge, research that utilizes categorical independent variables (e.g., group types) is replaced by research that employs continuous independent variables that represent the distinctions embodied in the categories. Thus, during this transition, both types of independent variables (categorical and continuous) are likely to be used, with the goal of showing that the model including the continuous variables is not significantly improved by the addition of the categorical variables (e.g., Oetzel, Ting-Toomey, Yokochi, Masumoto, & Takai, 2000). The study of conflict communication is at the point of this transition, so that methods that allow group comparisons—which we highlight in this chapter—are especially valuable at this time. For example, cross-cultural research in conflict communication is especially amenable to the methods we discuss (see Kim & Leung, 2000, for a review).

Moving from categorical to continuous independent variables is a theory-construction strategy. There are other such strategies, but they are implicit in the methods we choose to discuss. The methods used in a study reflect the conceptual definitions and the generality, abstractness, parsimony, and completeness of the theory used to investigate the phenomenon of interest. We have chosen not to discuss the variety of the conceptual definitions of conflict or their appropriateness for the studies we examined in preparing this chapter (see Putnam, Chapter 1, and Nicotera & Dorsey, Chapter 11, in this volume). Instead, we have kept our focus on the issues involving types of data, sampling and data analysis, and cross-cultural and other group comparisons. We note, however, that the conceptual and

theoretical issues in conflict communication research are the foundation on which investigations are constructed, and methods are useful only insofar as they provide the appropriate tools to answer questions posed by a sound theory. Furthermore, methods and theory are interdependent: Without some specific methods, some theoretical questions cannot be posed, and without some specific theory, the choice of methods is arbitrary, or, worse, irrelevant. In this chapter conceptual and theoretical issues are rarely explicated, not because they are unimportant, but rather to focus the chapter squarely on methods.

We begin by examining several issues that involve measurement and design, and then discuss sampling and analysis, the examination of group differences, and finally draw some conclusions about methods for studying conflict communication.

Types of Data I: Determination of Qualities and Quantities

Measures may be differentiated based on the assumptions implicitly made regarding how the magnitude of a phenomenon is assessed. If communications or acts are to be counted, there must be a clear definition of acts. Measures resulting from these *counts* are in the form of integers (there are 5 or 6 communications, not 5.5), and in principle the frequency scale starts at 0 and has no upper bound. A study can employ counts of the number of times an individual has engaged in jealousy-inducing behaviors (e.g., Brainerd, Hunter, Moore, & Thompson, 1996), the number of multiple goals in a conflict situation (e.g., psychological data in Samp, 2000), the number of hostility and anger expressions in marital interactions (e.g., interactional data in Gordis, Margolin, & John, 2001), or the number of crimes against the person in a society (e.g., sociological data in Daly & Wilson, 1997).

Measures created by comparison to a standard (such as a yardstick) yield magnitudes of a different sort. Time and distance are prototypical *amounts,* but many measures may be constructed by defining a non-material yardstick and having respondents or coders make comparisons to it. The following examples illustrate this point:

> If 0 is not feeling hostile at all, and 100 is the level of hostility you feel when you are cut off in traffic, how hostile do you feel in this negotiation? There is no highest number.
>
> If 0 indicates no aggression, and 100 is the level of aggression in the interrogation scene shown from *Law & Order* [a particular scene is shown], how much aggression is there in this video [a hostage negotiation video is shown]? There is no highest number.

An example of amount (or magnitude) scaling in studying war and conflict is found in Sulfaro and Crislip's (1997) study on Americans' perceptions of foreign policy threats. Participants rated their perceptions of 19 countries' hostility toward the United States with two magnitude scales and one Likert-type scale (i.e., a 7-point scale varying from "least hostile" to "most hostile"; p. 110). Results indicated that the two magnitude scales were almost identical in measuring hostility across the 19 countries with an "R^2 for [the logarithmically transformed variables] . . . near . . . .987" (p. 116), whereas the Likert-type scale correlated poorly with the magnitude scales because it dealt poorly with extreme values.

By making ratios and differences, counts and amounts may be used to create derivative measures: Examples include the ratio of the number of hostile words to the total number of words expressed (ratio of a count to a count); and the acceleration of aggressiveness in interaction, as assessed by the change in the magnitude of expressed aggressiveness over time (a change in an amount divided by an amount). One study in which a ratio is derived and employed (counts over amounts) is Fuller, Murphy, Ridgley, and Ulack's (2000) research on potential conflict in Southeast Asia. Another example of such derivative measures is the use of physiological data, which tend to be amount-over-time ratios. Buss, Larsen, Westen, and Semmelroth (1992) measured the acceleration of a negative emotion, jealousy, with such physiological measurements as electrodermal activity, pulse rate, and electromyographic activity.

We argue that the methods described here (counts, amounts, and their derivatives) allow greater precision, typically evidence higher

levels of reliability, and assist in the determination of the functional forms that relate our variables of interest when assessing hypotheses (Woelfel & Fink, 1980). We also realize that the typical investigator employs scales such as a 1–7 scale, with response alternatives bounded at both ends (e.g., one cannot go below 1 or above 7). Such scales are generally not examined for their many implicit assumptions: that the vectors emanating from the scale's neutral point to the end points are separated by 180°; that the distance between adjacent pairs of scale units is equal for all pairs; that the number of response alternatives is adequate for the phenomenon being scaled; and that the boundedness of the scale does not cause scale distortions due to floor or ceiling effects (see Torgerson, 1958, for a discussion of some of these issues). Conflict research, like all social science research, would benefit from greater consideration of measurement options in terms of response scales and their assumptions.

Types of Data II: Levels of Analysis

Conflict communication research employs data of a psychological, interactional, or sociological sort. Psychological data are descriptive of individuals, and include emotional states and traits, personality states and traits, level and type of motivation, and types and degree of knowledge. Examples of such data are measures of hostility (Buss & Durkee, 1957), propensity for abusiveness (Dutton, Landolt, Starzomski, & Bodnarchuk, 2001), and ethnocentrism (Neuliep & McCroskey, 1997a). Interactional data include attributes of verbal and nonverbal communicative behavior of people in simulated or actual interaction. Examples of such data are integrative and distributive behaviors in negotiation (Cai, Wilson, & Drake, 2000; Donohue & Roberto, 1996), emotion change between interactants (Rogan & Hammer, 1995), and coded linguistic measures (Cook-Gumperz & Szymanski, 2001; Scarry, 1985). Finally, sociological data involve attributes of groups, organizations, states, and cultures. Examples are Cashman's (1993) review of national attributes and international conflict (see also Diehl, 2004; Speer, 1986) and Doreian's (1981) analysis of network data to predict the mobilization of individuals taking sides in a conflict.

The typical methods used to gather these three types of data differ. However, all three types of data require evidence of validity. For theoretical concepts, validity is typically assessed by construct validation methods (or by related techniques, such as multi-trait multimethod matrix approaches; see Campbell & Fiske, 1959). It is quite common for measurement validity to be ignored prior to data collection, because a measure (say X) of a theoretical variable (say X^*) may be validated by finding support for a set of hypotheses in which X^* is measured by X. This strategy is risky: Without independent evidence of measurement validity, we cannot determine if failure to find support for a set of hypotheses is due to poor measurement, inadequate theory, or both.

Psychological data

For psychological variables, the typical data-gathering tool is a multi-item scale completed by the respondent. For such data we generally require evidence of reliability, especially internal consistency reliability as assessed, for example, by Cronbach's α. In addition, the dimensional structure of such scales is investigated by exploratory factor analysis (e.g., Lee & Rogan's, 1991, assessment of Putnam & Wilson's, 1982, Organizational Communication Conflict Instrument [OCCI]), confirmatory factor analysis (e.g., Oetzel et al.'s, 2000, examination of a typology of facework behaviors), or a full-blown structural equation model (e.g., Reese-Weber & Bartle-Haring's, 1998, confirmation of Rubenstein & Feldman's, 1993 three-factor conflict resolution structure).

If using exploratory factor analysis, the investigator may create a set of measures, each representing a single scale dimension, or create a single factor that represents the principal construct of interest. Either of these choices may be made by (a) eliminating items that do not load on the main (first) factor of interest by some criteria, and then adding or averaging the resulting items; or (b) computing factor scores for the one or more dimensions that the researcher deems to be interpretable (see Vangelisti & Crumley's, 1998, study of underlying factors of hurtful messages).

Free-standing measurement models (i.e., measurement models that are not part of "full"

structural equation models, which include relations between theoretical or latent variables) may be investigated with confirmatory factor analysis. Such an analysis can create scale composites that have advantages over the composites created via exploratory factor analysis: If the models thus created are over-identified (see Fink, 1980), full-information estimation methods to create scale composites can be used; this approach exploits the hypothesized structure that is presumed to have generated the covariances among the items. Furthermore, the measurement model that is imposed is tested as a single hypothesis (i.e., that the estimated population covariance matrix among the scale items does not differ from the estimated population covariance matrix as constrained by the measurement structure imposed by the investigator). Rejection of this hypothesis requires rethinking (and, presumably, reanalyzing) the measurement structure that was imposed. However, this procedure may have disadvantages: If our scale items or the model relating them is relatively arbitrary, it is unlikely that the data will fit a model with many constraints.

If the investigator were to test the dimensional structure of the scale items within a full structural equation model (i.e., one that incorporates both a measurement model and a theoretical model), the advantages and disadvantages would be basically the same as above. When employing a full structural equation model, investigators typically choose between what are called a one-step and a two-step approach. The two-step approach (Anderson & Gerbing, 1988) separately estimates the measurement model, as discussed above, respecifying it until the data fit. Then the respecified measurement model is incorporated within a full structural equation model. This approach differs from straightforwardly and simultaneously testing the full model, which includes the measurement component (see, e.g., Corcoran & Mallinckrodt, 2000).

Interactional data

Interactional data are typically the results of systematic observation. Coders (judges, observers, raters) are given coding rules and then code or rate the behaviors that are observed. If the behaviors are to be counted, then rules are needed to unitize the behaviors (i.e., determine where one behavior ends and another begins). For example, in a study regarding the interaction between hostage takers and negotiators, Taylor (2002a) analyzed transcripts of nine real hostage incidents. A rhetorical structure analysis was conducted to divide each transcript into separate episodes, or dialogue movements, based on changing themes. In addition, thought units were unitized from each episode before they were coded and subjected to data analysis. Other examples of creating rules to unitize data may be found in Gordis, Margolin, and Garcia (1996) and Gordis et al. (2001); these studies examine conflict within the family.

There is a great deal of literature on the factors that affect the unitization of behavior as assessed by actors and observers, so the process of unitization should not be thought of as without difficulties (see, e.g., Girbau, 2002; Lemus, Seibold, Flanagin, & Metzger, 2004; an elaborate discussion of this issue is found in Krippendorff, 2004). In addition to creating rules for unitization, the results of the unitization must be assessed for reliability; as Krippendorff (2004, p. 251) indicated, there must be agreement not only on the total number of units, but on the actual location of the units in the behavior stream.

If observational data are quantitative ratings rather than frequency counts, reliability is typically assessed by inter-coder reliability, most commonly in the form of one or more bivariate correlations. For scores derived from multiple coders, Cronbach's alpha may also be reported: Data from multiple coders may be analyzed as if each coder variable is an item within a multi-item scale, and the consistency of these multi-coder items may be assessed like multi-item psychological scales. So, if we have three coders rating the level of conflict in a set of groups, Cronbach's alpha, representing the consistency of the coders, may be computed and reported. Further, the coder variables may be treated as congeneric measures (multi-item single-factor scales; see Loehlin, 2004, p. 95). In other words, we can assume that each coder's rating of conflict is caused by a single true (i.e., reliable) level of observed conflict, and that the ratings have random errors that are independent. Given this model, we may subject the coded ratings to assessment within a measurement model or in a full structural equation model (see Fink, 1980).

Additional examples of unitization and reliability assessment in interactional conflict data may be found in research on hostage negotiation (Donohue, Ramesh, & Borchgrevink, 1991; Donohue & Roberto, 1993, 1996; Rogan & Hammer, 1995; Taylor, 2002b), family conflict (Gottman, Levenson, & Woodin, 2001; Smetana, Yau, & Hanson, 1991), interethnic conflict (Collier, 1996), and third-party mediated conflict (Jones, 1988).

Sociological data

This type of data may be differentiated into several subtypes. To assist us in developing a vocabulary, we paraphrase Lazarsfeld and Menzel's (1961, pp. 427–433) differentiation between individual and collective properties:

> Properties of Collectives: (a) *Analytical:* properties of collectives obtained by performing some mathematical operation on some property of individuals; (b) *Structural:* properties of collectives obtained by performing some mathematical operation on data about relations of individuals to some or all of the others; and (c) *Global:* properties of collectives not based on information about individuals.
>
> Properties of Individuals: (a) *Absolute:* characteristics of members obtained without making use of information about the collective or of information about relationships of an individual to other individuals; (b) *Relational:* properties of individuals computed from information about relationships between the individual and other individuals; (c) *Comparative:* properties that characterize an individual by comparison between this individual's value on some absolute or relational property and the distribution of that property over the entire collective; and (d) *Contextual:* properties that describe an individual in terms of a property of the collective.

The variables identified as properties of collectives are closest to what is here referred to as sociological variables. For example, characterizing a culture as individualistic (vs. collectivistic) may be based on a content analysis of archival data or of contemporary texts (global; e.g., Castilla, 2004), on the aggregation of individual responses to survey instruments (analytical; e.g., Hofstede, 1980), or on analysis of the density of friendship networks (structural; e.g., Brass & Labianca, 1999). On the other hand, there is cross-cultural conflict research in which individuals are characterized by a property of the culture of which they are members (relational or contextual) (see Kim & Leung, 2000, for a review).

One problem in research (including conflict research) that employs sociological variables is that the reliability and dimensional structure of such variables is seldom investigated. Global variables can be assessed for inter-coder reliability, and the individual scores that enter into analytical variables may be assessed for internal-consistency reliability. Furthermore, if individuals are sampled to represent the population of interest, aggregating properties of individuals to create a societal-level variable (analytical) is like treating each individual as a random "item" for the composite variable: With some assumptions, the Spearman-Brown prophecy formula (see, e.g., Lord & Novick, 1968, chap. 4; Nunnally, 1967, chap. 6) may be used to estimate how the averaged or summed score increases in reliability as the sample size on which it is based increases.

When using variables from different levels (e.g., sociological vs. individual), researchers need to guard against the ecological fallacy (Hofstede, 1980), which occurs when relationships between variables at one level are assumed to hold at a different level. Smith (2002) provided an example of the problem by examining the predictors of happiness at the cultural and individual levels.

It is reasonable to employ measures taken from different levels of analysis. However, because the data-gathering methods and the resultant assessment of reliability and scale dimensionality are likely to differ across levels, investigators must be aware of these differences and take them into account in the creation and assessment of the measures used.

Types of Data III: Time Dependence in Research Design

Studies can be differentiated by whether they employ cross-sectional data, such as surveys at one point in time; panel data (experimental and non-experimental) that employ at least two

points in time; and time-series data, based on many points in time. (Pooled time-series cross-section studies of conflict communication are rare and need not be discussed here.) We discuss how the conception of conflict interacts with the kind of design employed. This discussion is followed by the identification of a variety of measures that have been used to study conflict.

Although some reviews of conflict communication research differentiate conflict studies based on whether the data are self-report versus observational (Canary, Cupach, & Messman, 1995) or on other differentia (e.g., Nicotera, Rodriguez, Hall, & Jackson, 1995), the distinction we wish to emphasize is the focus on conflict outcomes versus the process that leads to the outcome. Most studies of conflict examine either a sample of conflicts (e.g., Holmes & Sykes, 1993; Poole & Roth, 1989) or a sample of individuals experiencing, anticipating, imagining, or recalling conflict (e.g., Cai & Fink, 2002; Maoz & Ellis, 2001; Ting-Toomey, Oetzel, & Yee-Jung, 2001). The use of samples of individuals rather than of conflicts has been associated with outcome-oriented research rather than research oriented to the relational process between actors (e.g., individuals, groups, organizations, states, cultures) that may sometimes result in conflict. The data examined for such outcome-oriented investigations are likely to be cross-sectional, and, even when attributes of conflict are included or predicted, there is not likely to be a no-conflict control group that would enable the conflict-generating or conflict-resolving process to be understood. This kind of research (i.e., research without a no-conflict control group) helps the investigator explain the management of conflict once an interaction event has reached a threshold that justifies the label conflict.

We can contrast this approach with understanding conflict as a process, entailing a trajectory of variables indicative of conflict, disinterest, and accord between parties. Although most process-oriented conflict studies tend to focus on contexts or relationships that are conflictual, trajectories of conflict variables represent time courses of cooperation just as well as time courses of conflict (for studies over time see, e.g., Holmes, 1997; Holmes & Sykes, 1993; Poole & Roth, 1989; Rogan & Hammer, 1994, 1995). Such over-time data allow for the explanation of conflict trajectories, whether they refer to dyads or to states. Considering conflict in this way makes conflict "normal," in the sense that we are not viewing conflict as an aberrant segment of a relationship or as a pathological event but rather as a dynamic generated by ordinary sequences and magnitudes of activity. Both Freud and Festinger exemplify scholars for whom conflict was part of the normal process of emoting, thinking, interacting, and behaving; in a word, living. A *normal conflict approach* considers conflict to be ordinary and normative (in an actuarial sense) within the vicissitudes of action of people, groups, communities, states, and cultures.

There are methodological implications of such a normal conflict approach. In this approach, conflict is likely to be examined as a continuous variable, and over-time investigations of the causes and consequences of conflict (e.g., panel and time-series investigations) are more likely. When conflict is examined experimentally, a control group is more likely to represent a state of no conflict or of cooperation rather than of a different type of conflict or of a low level of conflict.

A classic example of the normal conflict approach is the small-group interaction analysis developed by Bales (1950). Although Bales's work is not generally considered within the conflict literature, we can see how it does fit and how it reflects the normal conflict approach. Bales assigned the communicative possibilities exhibited by interactants to 12 categories. A communication by an interactant may (a) show solidarity, (b) show tension release, (c) agree, (d) give a suggestion, (e) give an opinion, (f) give orienting information, (g) ask for orienting information, (h) ask for an opinion, (i) ask for a suggestion, (j) disagree, (k) show tension, or (l) show antagonism. In any observed interaction there may be conflict, as indicated by messages of type (j), (k), or (l). However, the coding system is not restricted to interactions that necessarily involve conflict. In a similar way, negotiation research often looks at the unfolding of interaction, but the context assumes some level of conflict will arise during the interaction because of the competing goals of the interactants.

Additional interaction coding schemes were developed from the mid-1980s to the

Table 2.1 Studying Interethnic and Intercultural Conflict

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
Interethnic	Questionnaire	Personal report of interethnic communication apprehension	Neuliep & McCroskey (1997b)	Toale & McCroskey (2001)
	Questionnaire	General ethnocentrism scale	Neuliep & McCroskey (1997a)	Toale & McCroskey (2001)
	Questionnaire	Black metastereotypes	Sigelman & Tuch (1997)	
	Questionnaire	Ethnic/cultural identity	Ting-Toomey et al. (2000)	
	Questionnaire	Goal concerns & conflict tactics	Ohbuchi & Tedeschi (1997)	Ohbuchi, Fukushima, & Tedeschi (1999)
	Discourse analysis	Interethnic conflict	Speicher (1995)	
Intergroup	Discourse analysis	Intergroup conflict	Maoz & Ellis (2001)	
International	Coding	War: Text scoring system for historical analysis	Winter (1991)	Winter (1993)

mid-1990s, reflecting a continued interest in interaction during conflicts. For example, hostage negotiation, business negotiation, and marital mediation were studied using the methods of conversation analysis. Lag-sequential analysis was used to study interaction patterns during simulated negotiation (e.g., Cai & Donohue, 1997), and phase mapping was used to study conflict phases during interaction (e.g., Holmes, 1997). These approaches should have created opportunities for process research; however, we note that such investigations have declined over the past several years. Indeed, interaction analysis of conflict has been largely abandoned. A notable exception to this trend is a recent issue of the *International Journal of Conflict Management* (2003; also see earlier work, such as Gottman, Markman, & Notarius, 1977).

Process-oriented approaches typically do not utilize exclusively cross-sectional data, although such an approach is not impossible. For example, a structural equation model, whether recursive or nonrecursive, may use cross-sectional data to represent equilibrium values of a process; an analysis of variance or a regression model may also represent a process even though such analyses may employ exclusively cross-sectional data. However, it is typically the case that different conceptions—conflict as outcome versus conflict as process—result in different kinds of data employing different analytic methods.

Types of Data IV: Measures for the Study of Conflict

Although by no means exhaustive of the possible measures of conflict, Tables 2.1 through 2.3 provide examples of measures used in studying conflict in communication research for studying interethnic and intercultural conflict; interpersonal conflict, including conflict measures related to intrapersonal communication, family, marriage, and relationship conflict, and partner

Table 2.2 Studying Interpersonal Conflict

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
Intrapersonal	Questionnaire	Anger and distress: Interpersonal statement rating	Biglan, Rothlind, Hops, & Sherman (1989)	Kubany & Richard (1992)
	Questionnaire	5 jealousy-inducing behaviors	Brainerd, Hunter, Moore, & Thompson (1996)	
	Questionnaire	Hostility inventory	Buss & Durkee (1957)	Tangney, Wagner, Fletcher, & Gramzow (1992)
	Questionnaire	Jealousy response to relationship infidelity	Buss, Larsen, Westen, & Semmelroth (1992)	Cann, Mangum, & Wells (2001) Cramer, Abraham, Johnson, & Manning-Ryan (2001–2002)
	Questionnaire	Anger and hostility: SCL-90: Symptom checklist 90	Derogatis, Lipman, & Covi (1973)	Tangney, Wagner, Fletcher, & Gramzow (1992) Tangney, Wagner, & Gramzow (1992)
	Questionnaire	Anger expression scale	Guerrero (1992)	Guerrero (1994)
	Questionnaire	Jealousy-related goals	Guerrero & Afifi (1999)	
	Questionnaire	Communication responses to jealousy scale	Guerrero, Anderson, Jorgensen, Spitzberg, & Eloy (1995)	Anderson & Eloy (1995) Guerrero & Afifi (1999)
	Questionnaire	Criticism from ingroup vs. outgroup	Hornsey, Oppes, & Svensson (2002)	
	Questionnaire	Argumentativeness scale	Infante & Rancer (1982)	Infante, Chandler, & Rudd (1989)
	Questionnaire	Verbal aggressiveness scale	Infante & Wigley (1986)	Infante, Sabourin, Rudd, & Shannon (1990) Segrin & Fitzpatrick (1992)

(Continued)

Table 2.2 (Continued)

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
	Questionnaire	Interpersonal jealousy scale	Mathes & Severa (1981)	Dutton, van Ginkel, & Landolt (1996)
	Questionnaire	Cognitive and emotional jealousy	Pfeiffer & Wong (1989) Modified: Guerrero, Eloy, Jorgensen, & Anderson (1993)	Anderson & Eloy (1995) Guerrero & Afifi (1999)
	Questionnaire	Multidimensional anger inventory	Siegel (1986)	Dutton, van Ginkel, & Landolt (1996)
	Questionnaire	TAC: Trait anger scale	Spielberger, Jacobs, Russell, & Crane (1983)	Tangney, Wagner, Fletcher, & Gramzow (1992)
	Questionnaire	Interpersonal control scale for men and women	Stets (1991)	Brainerd, Hunter, Moore, & Thompson (1996)
	Questionnaire	Psychological aggression scale for men and women	Stets (1991)	Brainerd, Hunter, Moore, & Thompson (1996)
	Scenarios	4 jealousy-inducing scenarios	Fisch & Brainerd (1990)	Brainerd, Hunter, Moore, & Thompson (1996)
Family	Coding observations	Family coding system	Margolin & Gordis (1992)	Gordis, Margolin, & John (2001)
	Coding observations	Family coding scheme	Sillars (1980)	
	Physiological measures	Physiological measure of emotions involved in marital conflicts	Gottman, Levenson, & Woodin (2001)	
	Content analysis	Relational maintenance and guilt: Narratives	Baumeister, Stillwell, & Wotman (1990)	Baumeister, Stillwell, & Heartherton (1995)
	Content analysis	Mediation custody and visitation agreements	Mathis & Tanner (1998)	
	Content analysis	Mediation discourse: Language of agency	Scarry (1985)	Cobb (1997)

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
	Questionnaire	Family Communication Pattern instrument – Revised	Fitzpatrick & Ritchie (1994) Ritchie & Fitzpatrick (1990)	Koerner & Fitzpatrick (1997, 2002)
Marriage and Relationship	Coding	Marital coding system	Gordis, Margolin, & Garcia (1996)	Gordis, Margolin, & John (2001)
	Open-ended questions	Women's divorce and mediation	Cheung & Kwok (1999)	
	Questionnaire	Perceived helpfulness of mediation services	Cheung & Kwok (1999)	
	Questionnaire	Marital problems	Gottman (1994)	Gottman, Levenson, & Woodin (2001)
	Questionnaire	Couple's problem inventory	Gottman, Markman, & Notarius (1977)	Gottman, Levenson, & Woodin (2001)
	Questionnaire	Conflict tactics between spouses	Straus (1979)	Jenkins (2000) Swinford, DeMaris, Cernkovich, & Giordano (2000)
	Questionnaire	Marital conflict	Straus & Yodanis (1996)	
Partner Abuse and Violence	Questionnaire	Trauma symptom checklist	Briere & Runtz (1989)	Dutton, van Ginkel, & Landolt (1996)
	Questionnaire	Sources of conflict between men and women	Buss (1987)	Ellis & Malamuth (2000)
	Questionnaire	Intrusiveness	Dutton, van Ginkel, & Landolt (1996)	
	Questionnaire	PAS: Propensity of abusiveness scale	Dutton, Landolt, Starzomski, & Bodnarchuk (2001)	
	Questionnaire	Psychological maltreatment inventory	Kasian & Painter (1992)	Simonelli & Ingram (1998)
	Questionnaire	Severity of violence against women & violence against men	Marshall (1992)	Dutton, Landolt, Starzomski, & Bodnarchuk (2001)

(Continued)

Table 2.2 (Continued)

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
	Questionnaire	PMWI: Psychological maltreatment of women inventory	Tolman (1989)	Dutton, van Ginkel, & Landolt (1996) Dutton, Landolt, Starzomski, & Bodnarchulk (2001)
Children	Questionnaire	Self-reported delinquency scale	Elliot & Ageton (1980)	Swinford, DeMaris, Cernkovich, & Giordano (2000)
	Questionnaire	Sibling relationship questionnaire	Furman & Buhrmester (1985)	Rinaldi & Howe (2003)
	Questionnaire	Children's hostility inventory	Kazdin, Rodgers, Colbus, & Siegel (1987)	Gordis, Margolin, & John (2001)
	Questionnaire	Children's sibling relationships	Kramer & Baron (1995)	Rinaldi & Howe (2003)
Interpersonal	Coding	Constraining and enabling coding system	Maccoby & Martin (1983)	Smetana, Yau, & Hanson (1991)
	Questionnaire	Conflict issues measure	Fey (1995)	Koerner & Fitzpatrick (1997)
	Questionnaire	Conflict management survey	Hall (1969)	Evaluated: Womack (1988)
	Questionnaire	Avoidance, seeking social support, venting negative feelings	Koerner & Fitzpatrick (1997)	
	Questionnaire	Unilateral avoiding, aggressing, and resisting	Koerner & Fitzpatrick (2002)	
	Questionnaire	Conflict management message style	Ross & DeWine (1988)	Evaluated: Womack (1988)
	Questionnaire	Conflict-resolution behavior	Rubenstein & Feldman (1993)	Reese-Weber & Bartle-Haring (1998)
	Questionnaire	Concern for self or other	Sorenson, Morse, & Savage (1999)	

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
	Questionnaire	5-behavior conflict tactics scale to measure verbal and physical abuse	Straus (1977, 1990) Straus & Gelles (1990)	Ellis & Malamuth (2000) Straus & Yodanis (1996)
	Questionnaire	Thomas-Kilmann conflict MODE instrument	Thomas & Kilmann (1974)	Munduate, Ganaza, Peiró, & Euwema (1999) Evaluated: Womack (1988)
	Scenarios	Hypothetical conflict scenarios	Baxter (1984)	Chen, Ryan, & Chen (1999)
	Scenarios	Conflict scenario and eight resolving procedures	Leung, Bond, Carment, Krishnan, & Liebrand (1990)	Leung, Au, Fernández-Dols, & Iwawaki (1992)
	Scenarios	Conflict scenarios	Miyahara, Kim, Shin, & Yoon (1998)	

abuse and violence; and organizational and community conflict, including the study of conflict in hostage and crisis negotiation and in schools. These measures illustrate the approaches used to study conflict by observation; by qualitative measures such as those derived from interviews and open-ended questions; by coding schemes and content analysis of interactions and texts; and by questionnaires and multi-item scales of variables directly related to conflict, such as anger, depression, aggression, and conflict styles.

In their history of communication and conflict research, Nicotera et al. (1995) noted three levels of organizational conflict theory: strategy and logic (game theory), microlevel approaches (cognitive approaches), and macrolevel approaches (institutional approaches). Tables 2.1 through 2.3 show that, when studying conflict, communication researchers focus more on microlevel self-report measures of cognition and emotion and less on the strategy and macrolevel approaches (see also Canary et al., 1995, on studying relational conflict).

Observer (including peer) behavioral evaluation is used primarily to examine conflict in mediation and negotiation, and in institutional settings such as schools. Further, although researchers may observe actual conflict situations, rarely is the research conducted on the conflict as it unfolds in real time. Instead, researchers generally utilize transcripts or videos of real conflicts, as in hostage negotiations; archival data, as in international conflict negotiations; taped (video or audio) interactions, as in small-group, third-party mediated, or dyadic conflicts; or reported behaviors, either orally, as in interviews, or written, as in questionnaire responses. Nevertheless, these tables also highlight the wide range of instruments and measures that have been developed and used to study conflict.

Sampling and Data Analysis

In this section we will review the issues of which conflict communication researchers should be aware regarding sampling and analysis. Although

Table 2.3 Studying Organizational and Community Conflict

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
Community	Case study	Environmental mediation: Longitudinal cases	Sipe (1998)	
Hostage & Crisis Negotiation	Coding	Integrative-distributive behavior coding system	Donohue & Roberto (1996)	
	Coding	Relational development	Donohue & Roberto (1993)	Donohue (1998)
	Coding	Smallest space analysis	Lingoes (1973)	Taylor (2002a)
	Coding	Thematic analysis	Orbe & Warren (2000)	
	Coding	Speech analysis	Rogan & Hammer (1995)	
	Coding	Emotion change	Rogan & Hammer (1995)	
	Coding	Immediacy coding system	Wiener & Mehrabian (1968)	Donohue, Ramesh, & Borchgrevink (1991)
	Phase mapping	Gamma analysis	Pelz (1985)	Holmes & Sykes (1993)
	Phase mapping	Interaction phase mapping	Poole & Roth (1989)	Holmes (1997) Holmes & Sykes (1993)
Organizations	Questionnaire	Negotiating style profile	Glaser & Glaser (1991)	Gabrielidis, Stephan, Ybarra, Pearson, & Villareal (1997)
	Questionnaire	Disputing process instrument	Morrill & Thomas (1992)	
	Questionnaire	OCCI: Organizational Communication Conflict Instrument	Putnam & Wilson (1982)	Lee & Rogan (1991) Evaluated: Wilson & Waltman (1988) Womack (1988)
	Questionnaire	ROCI-II: Rahim's Organizational Conflict Inventory	Rahim (1983)	Sorenson, Morse, & Savage (1999) Ting-Toomey, Oetzel, & Yee-Jung (2001) Evaluated: Cai & Fink (2002) Womack (1988)

(Continued)

Table 2.3 (Continued)

Focus	*Method*	*Measure or Instrument*	*Original Authors*	*Example of Use*
Schools	Inventory	Achenbach child behavior checklist	Achenbach (1991a)	Salzinger et al. (2002) Gordis, Margolin, & John (2001)
	Inventory	Achenbach teacher's report form	Achenbach (1991b)	Salzinger et al. (2002)
	Questionnaire	Conflict resolution scale	Smith, Daunic, Miller, & Robinson (2002)	
	Questionnaire	Disputant questionnaire	Smith, Daunic, Miller, & Robinson (2002)	
	Questionnaire	Mediator parent questionnaire	Smith, Daunic, Miller, & Robinson (2002)	
	Questionnaire	Peer mediator generalization questionnaire	Smith, Daunic, Miller, & Robinson (2002)	
	Questionnaire	School climate survey	Smith, Daunic, Miller, & Robinson (2002)	

these issues apply to all social-science research, there are particular concerns for research on conflict, and we concentrate on them.

Sampling

Sampling in conflict research poses particular challenges. At the interpersonal level, including dyadic and small-group interactions, the level of intimacy between interactants may range from low (e.g., strangers) to moderate (e.g., coworkers) to high (e.g., friends) to very high (e.g., romantic partners and family members). The predominant sampling strategy for studies at the psychological and interactional levels is nonprobability sampling, which not uncommonly uses a convenience sample of undergraduates. So what's new? Researchers implicitly rely on the notion that, although the mean levels of the variables under investigation may not be representative of the theoretical population's mean levels, the covariances among the variables—the data that inform us about process—are not biased.

To study conflict we must assume that it is possible to sample not only people, but situations that vary in conflict (magnitude or type) as well, and that the sample of conflicts is reasonable in terms of its range on a variety of variables (e.g., intensity, duration, and theoretical cause of the conflict). This assumption is not met if the conflicts that we may need to know most about are inaccessible or have limited accessibility. For example, because of business secrets, we may not be able to sample representative negotiations between employers and employees; because of security issues, we may not be able to sample representative hostage negotiations. In these cases, we face a selection bias (Berk, 1983); if one or more variables that affect sample membership interact with any variables determinative of the conflict process under investigation, our conclusions are likely to be, at best, incomplete, or at worst, biased and misleading. For example, suppose that the only hostage negotiation transcripts that we have are those made available to us by negotiators for the government. Suppose further that this subset of transcripts is available because the government agents employed threats significantly less in them than in the other transcripts,

which are unavailable to us. Finally, suppose that the presence of threats by government agents changes the effect of one or more conflict predictors significantly: Suppose threats make hostage-takers from ethnic group A more confrontational, whereas threats make hostage-takers from ethnic group B less confrontational. Then, although the sample is biased, it is more important to note that the analysis is likely to be misleading: Presence versus absence of threats interacts with ethnicity, so that the parameters for ethnicity that we estimate from this sample will be useful only if the main effect of ethnicity is relatively large and the interaction effect of ethnicity and presence versus absence of threats is relatively small.

Another example based on hostage negotiation research illustrates a different problem. Suppose we have transcripts from all or from a representative (even if not random) sample of such negotiations; we thereby avoid a selection bias. Thus, it would seem, these transcripts, although difficult to obtain, provide very useful information about high intensity conflicts. But now we face a different kind of problem: To complete the picture and create appropriate theoretical models of conflict processes that include processes that vary in intensity, we need our sample to contain a range of conflict intensities. Thus, we would need transcripts of low intensity conflict that fall within the domain being investigated, and it may be hard to imagine low intensity conflicts when we are considering hostage negotiations. Thus, even a good sample may not allow some aspects of a theory of conflict processes to be investigated adequately.

If a theory of conflict is to be developed, we may require samples that represent the universe of conflict situations, even if the sampling of such situations is a convenience sample. This is a bootstrap operation: We need a theory to sample across the types of conflict situations, and we need a representative sample of situations to generate the theory. In the absence of such theory, we can look to theories in neighboring domains to consider this sampling problem. For example, Foa and Foa (1974) described six types of resource exchanges: exchanges over love, status, information, money, services, and goods. We may identify conflict situations based on the resources primarily involved, thus generating a "resource-exchange universe" from which to sample conflict situations. Similarly, Marwell and Hage (1970) empirically developed three dimensions descriptive of the organization of role relationships that may be useful here. Role relationships differ in terms of their intimacy, visibility, and regulation. If we are examining how individuals in different roles create, resolve, escalate, maintain, or define conflict, we can use the three role-relationship dimensions (intimacy, visibility, and regulation) to generate a $2 \times 2 \times 2$ typology of role-relationships from which we can purposively sample (see also Seeman, 1997). Thus, even in the absence of strong theory we may improve our sampling strategy so that it covers a theoretical universe and thereby enhances our ability to make theoretical discoveries.

Even if we generate an appropriate sampling strategy, we need to consider the sample size that is necessary for the proposed analyses. Sample sizes vary greatly across studies: In some of the studies reviewed for this chapter, sample sizes were inadequate for the methods used; for example, some studies attempted to estimate many parameters via ANOVA or multiple regression on samples with as few as 20 participants. The difficulty in obtaining appropriately sized samples should not be used as an excuse to do analyses that lack statistical power.

Data Analysis: Statistical Dependence

Statistical analyses make assumptions about the distribution and association between residual (error) terms. For example, significance testing of parameter estimates within regression or ANOVA assumes that population errors are homoscedastic, non-autocorrelated, and normal. Similarly, dependence among sample members (e.g., sampling pairs of husband-wife dyads rather than sampling individuals who happen to be husbands and wives, but not of each other) will likely cause correlation among error terms in the statistical model. Violations of statistical assumptions may be corrected by data transformation (Bauer & Fink, 1983) or by appropriate statistical modeling.

Many conflict situations are posed as two-sided situations: employer-employee, buyer-seller, hostage-taker and hostage, aggressor

nation and target nation, violent domestic partner and violated domestic partner. Such dyadic interactions involve interdependence between agents. These samples have dependent (in the statistical sense) units, and that dependence needs to be taken into account in the analysis.

Structural equation modeling can represent the dependence in several ways. For example, Duncan's (1969) two-wave two-variable panel model can represent the interdependence between variables with correlated errors and mutual causality. In such analyses some of the variables come in pairs, one element of the pair for each partner in the dyad (see, e.g., Duncan, Haller, & Portes, 1968). So, imagine pairs of variables such as husband's level of anger and wife's level of anger, and husband's level of verbal aggression and wife's level of verbal aggression. A model that has parallel sets of variables for each interactant can be created that takes into account the dependence among the variables due to the dependence among the interactants.

One guiding rule for structural equation models is that, with well-behaved data, the sample size be a minimum of five times the number of free parameters to be estimated (Bentler & Chou, 1987). Another guiding rule is that a model should have a sample size that is 10 to 20 times the number of variables in the model (Mitchell, 1993). By either rule, the sample size for a model with more paths and more variables will need to be greater, perhaps twice as large as a sample without the paired data representing interdependent actors.

Kenny and Kashy (1991; see also Kashy & Kenny, 2000) described two types of interdependence that may exist in dyadic data sets: within-dyad interdependence and between-dyad interdependence. Within-dyad interdependence reflects systematic changes over time within a single dyad, such as the changes over time in uncertainty reduction within a dating couple. Such interdependence may appear as correlated errors over time.

Between-dyad interdependence results from cross-sectional dependence in dyads at a single point in time, such as due to omitted factors that affect both the buyer and the seller at each point in their negotiation. Such interdependence may appear as correlated errors across variables at one time. For example, suppose buyers and sellers tend to negotiate with individuals who are similar in ethnicity, and we have a sample with many buyer-seller dyads varying in ethnicity. Suppose further that the variables used to model the buyer-seller interaction do not include variables correlated with ethnicity. Then ethnicity is an omitted factor that affects both the buyer and the seller at each point in their negotiation, causing correlated errors across variables at each time point.

Because both types of interdependence may exist, models should test for their presence. The two basic strategies, regardless of the specific analytical method employed, are to hypothesize the absence of these effects and examine model fit under this constraint, or to hypothesize the presence of these effects and to test the significance of the statistics that represent the interdependence. If the causes of interdependence are included or controlled for in such models, the effects will be "tamed"; in other words, we will have taken the interdependence into account and will thereby be able to create statistically consistent (see, e.g., Hanushek & Jackson, 1977) parameter estimates.

In studies of groups larger than dyads with multi-level sampling, the same issues appear. So, for example, in studies in which families are sampled, and within each family unit several family members are included, there is interdependence among the sampled units. Analytical methods that deal with multiple levels (with or without multiple time points within the data set) are hierarchical linear models (HLM); repeated-measures, multivariate, and other nested models in ANOVA; and multiple group (or multi-sample) analysis in structural equation models.

Of the three methods mentioned above, HLM is probably used least by scholars studying communication and conflict, but there are several exemplary studies: Julien, Chartrand, Simard, Bouthillier, and Bégin's (2003) study of positive and negative communication during conflict in heterosexual, gay, and lesbian couples (with partners nested within couples); Karney and Bradbury's (1997) analysis of trajectories of marital satisfaction; Rhoades, Arnold, and Jay's (2001) investigation of affective traits and mood on organizational conflict over time; Sanford's (2003) investigation of

"topic difficulty and communication behavior across multiple problem-solving conversations" among married couples (p. 99); and Smith and Zautra's (2001) piece on the effect of spousal conflict, interpersonal sensitivity, and neuroticism on affect in a sample of older women.

Conflict communication research typically involves interdependent participants, and therefore scholars studying in this area need to be aware of the statistical problems—and, once understood, the statistical opportunities—that such data provide. Using a sophisticated analytic method such as structural equation modeling or hierarchical linear modeling encourages the researcher to think about the ways that units interact, and to represent this interdependence in the statistical models employed.

Cross-Cultural Conflict Research, With Applications to Other Group Differences

Conflict studies incorporating culture differ in whether the individual is the unit of analysis and culture is a contextual variable versus those in which the unit of analysis is culture (or nation) and the sample includes a set number of cultures. Almost all conflict communication studies use the former approach. (Some quantitative studies and mathematical models of the causes of war are exceptions; see Cashman, 1993, and Diehl, 2004.) Because of the way culture enters into conflict communication research, it is treated as static by necessity: It is an exogenous variable that varies over people or over space but not over time. However, culture does change over time, sometimes even over relatively short periods. Effects due to cultural change are almost always excluded from conflict communication research.

Many studies examine such static cultural differences in conflict styles (for reviews of these studies, see Oetzel & Ting-Toomey, 2003; Wilson, Cai, Campbell, Donohue, & Drake, 1995). These studies are mixed in their results, some finding members of East Asian cultures to be more avoidant or yielding and people from the United States to be more dominating (e.g., Lee & Rogan, 1991; Trubisky, Ting-Toomey, & Lin, 1991). Other studies find members of both cultural groups to prefer integrating styles (e.g., Cai & Fink, 2002). The typical method used to investigate conflict styles across cultures is to provide a hypothetical conflict scenario, ask participants to consider a conflict with someone (e.g., a friend, colleague, or stranger), and, based on that imagined conflict, to complete instruments such as either the Rahim Organizational Conflict Inventory II (ROCI-II; Rahim, 1983) or OCCI (Putnam & Wilson, 1982) to measure the individual's approach to the conflict. To draw conclusions about how cultures compare in conflict behavior, however, researchers need to answer four questions about the sample and the conflict. First, are the meaning of conflict and the variables representing the conflict process comparable across the cultures being investigated? Second, are the samples comparable? Third, do the samples use the same processes with the same variables (the same equation or equations with the same functional forms and parameter values) for dealing with conflict? And fourth, are the cultures at the same place in the process under investigation? (Note that the discussion that follows can be made for comparing conflict across organizations or any other categorical variable, as well as across cultures.)

Are the Meaning of Conflict and the Variables Representing the Conflict Process Comparable Across the Cultures Being Investigated?

Let us consider the example of conflict styles for this question. The etic approach seeks to determine theoretical factors based on a scientific (here, transcultural or universal) analysis of variables (see Pike, 1967). Based on the etic research of Blake and Mouton (1964), Pruitt and Rubin (1987), and Thomas (1976), five basic conflict styles have been generated. That the ROCI-II and OCCI measures were derived from Western theories and imposed on Eastern cultures exemplifies the typical approach to cross-cultural conflict research, which depends on imposed rather than derived etic analysis (Berry, 1989). As a result, we do not know whether other (non-Western) styles exist for managing and resolving conflict. For example, Wall and Blum's (1991) idea of third parties having a role in dealing with conflict is not reflected in these measures, yet these researchers found the use of third parties

to be a socially appropriate means for managing some conflicts among Chinese.

An imposed etic approach involves taking theories or hypotheses applicable to one culture and imposing them on another culture without knowing if the theory or the related measures are appropriate for the other cultures studied. In the area of conflict research, an imposed etic (Berry, 1989) is often relied on, assuming that the meaning of conflict is similar across cultures. A derived etic approach reflects careful observation and analysis of a variety of cultures to determine all the relevant variables that should be considered when studying a phenomenon across an even broader variety of cultures. A derived etic approach is needed to determine the meaning of conflict and conflict situations within each culture so that comparable situations within the cultures, ones that have similar meaning to the participants and that involve similar relationships and levels of emotion, can be employed in testing theory.

In studies that ask participants to recall a conflict situation, participants from different cultures are rarely asked to describe the recalled situation in sufficient depth to allow comparability across the cultures to be determined; the participants are also unlikely to be asked about the relevant state variables that define the situation, such as the level of emotion among the participants. As a result, conflict may involve qualities of anger and confrontation in one culture, whereas, in another culture, conflict may involve a rift in the relationships such that participants avoid rather than confront or communicate with each other; emotional salience may differ between the cultures because of the level of contact between the parties.

To determine whether the meaning of conflict and the variables representing the conflict process are comparable across the cultures, we need to do more than back-translate scales (see van de Vijver & Leung, 1997). The process of back-translation often involves having one party translate versions of a questionnaire from English, the language in which the original study was prepared, into the language of the target culture, then having a second person retranslate the new version back into English. Even careful translation and back-translation result in distortion of meaning (see Barnett, Palmer, & Al-Deen, 1984). Under the rubrics of bias and equivalency, van de Vijver and Leung (1997) discuss the issues involved in reducing distortion in translation. To do this task well requires more effort, more resources, and more time than most scholars anticipate, and even this extensive process does not guarantee comparability in meaning.

An alternative way of considering the problem of comparability in meaning is to use variables of sufficient abstraction so that they conceivably may apply cross-culturally. Then and only then can we examine whether there are cross-cultural process differences. In other words, the research task requires bootstrapping, cycling from theory couched in abstract variables to in-depth interviews with cultural informants to creation of measures that may involve culturally specific operationalizations to statistical analyses to revamping theory. Thus, the issues discussed below follow, and also precede, the issues discussed above.

Are the Samples Comparable?

Between-culture samples involve considerations that are different from those of within-culture samples. Convenience samples, consisting, for example, of university students, or snowball samples, consisting, for example, of friends and friends of friends, do not necessarily generate comparable samples across cultures. Any good article or book on intercultural research will raise this issue (see, e.g., Johnson & Tuttle, 1989; Tafoya, 1984; van de Vijver & Leung, 1997). Certainly social class, ethnicity, and other demographic differences are likely to affect the social norms people use for managing conflicts. Research among ethnic and cultural groups should measure and then attempt to control for socioeconomic differences, including education level, income relative to the per capita income of the nation the sample is from, occupational prestige, and the like (see, e.g., Massett, 1999). Controlling for these factors allows researchers to at least attempt to differentiate cross-cultural from (within culture) sociological factors, thereby being able to make more valid claims about how cultures compare. Without such measures, claims about culture are likely to be masking the influence of class, population density, education, and so on.

This methodological discussion has tremendous implications for theoretical development. If we are concerned with the belief and behavior systems of a population (e.g., the norms for handling conflict in the workplace, in the family, or among friends; the sanctions for transgressors of these norms; the conditions under which specific requests are appropriate), then sociological factors make a difference, but do we classify this difference as "cultural"? Because developmental effects (e.g., worldwide urbanization; increased participation in formal education) modify cultures, any differentiation between cultural and non-cultural factors is a snapshot of one point in time. We try to get a handle on this differentiation by two methods: using comparable samples, which are difficult to obtain, or relying on statistical controls. The former involves using samples that may be unrepresentative of the larger population, but similar in some characteristics to the other culture to which its members are compared. The latter is likely to "overcontrol" by removing effects that are or will be tied to cultural differences. This conundrum requires not better methods, but better theory to investigate processes and attribute effects to their appropriate sources.

Do the Samples Use the Same Processes With the Same Variables for Dealing With Conflict?

Once conflict situations are determined to be comparable across cultures in terms of the severity of the conflict, the relationship between the parties, and the level of emotion involved, then researchers can begin to determine whether the processes involved in managing the conflict are comparable. Having relatively equal means on a set of variables does not suggest that the processes that generated these means are the same, or that the means reflect the same point in the process, or that the process has equilibrated. Rather, the ability to make claims about processes involves determining the functional form of relationships between variables. In doing so, two central questions about functional forms emerge.

The first question is whether the same functional form relating a set of variables applies across the cultures under consideration. The same functional form means the same equation (statistical or mathematical), with the same variables, with (for statistical models) the error term entering into the equation in the same way (e.g., additively vs. multiplicatively), and, finally, with the same estimated parameter values.

Suppose conflict and dissatisfaction are both amount scales (see above). And suppose that, for one group, unit increases in the level of conflict at time-0 cause 2-unit increases in dissatisfaction one time unit later (i.e., at time-1):

$$\text{DISSATISFACTION}_{t1} = 2 \times (\text{CONFLICT}_{t0}).$$

For another group, dissatisfaction at time-1 increases as the square of the level of conflict at time-0:

$$\text{DISSATISFACTION}_{t1} = (\text{CONFLICT}_{t0})^2.$$

In this case, although the variables that are included in the two equations are the same, the process that relates them is different.

Differences in functional form may be approached as a theoretical question, a measurement question, or an analytic question. The first treatment of this issue, as a theoretical problem, concerns whether the variables being used in the analyses are sufficiently general and can be thought of as meaningfully tied to a process in the same way. For example, if one were assessing the level of conflict across cultures, one might use relatively concrete operationalizations that differ by culture or are even idiosyncratic to particular cultures. However, these particular operationalizations may hide important theoretical differences. For example, conflict in one group may be operationalized by the amount of anger (a psychological variable) exhibited by an individual. In another group, conflict may be operationalized by the level of language intensity employed in interaction (an interactional variable). These measures are theoretically distinct—they are at different levels of analysis. Thus, they enter into the conflict→ dissatisfaction equation differently, by definition resulting in different functional forms. Theoretical considerations suggest that the variables descriptive of the conflict process should be at the same level of analysis and should be at the locus within a network of relationships.

Second, finding that different functional forms apply to different cultures may reflect issues of measurement. As an example, consider the following situation. Researcher A and Researcher B both study the effects of level of anger (A) and level of conflict (C) on retaliation (y). Researcher A proposes and finds that the two independent variables relate in a power-law fashion (i.e., the independent variables, each raised to a power, form a product that determines the level of the dependent variable). Specifically, ignoring the error term, Researcher A finds

$$\hat{y} = b_0 (A^{b_1} * C^{b_2}). \tag{2.1}$$

Researcher B, using different measures (indicated with asterisks), proposes and finds that the effects of these independent variables are additive:

$$\hat{y}^* = b_0^* + b_1 A^* + b_2 C^*. \tag{2.2}$$

It would seem that these different functional forms reflect different processes. However, without understanding how the researchers' different measures relate, the differences may be more apparent than real. Logarithmically transforming a product converts it to a sum:

$$\ln(X_1 * X_2) = \ln(X_1) + \ln(X_2).$$

So, we are able to transform Equation 2.1 by taking the natural logarithm of both sides (again, ignoring the error terms):

$$\ln(\hat{y}) = \ln(b_0) + b_1 \ln(A) + b_2 \ln(C). \tag{2.3}$$

If $\hat{y}^* = \ln(\hat{y})$, $b^*_0 = \ln(b_0)$, $A^* = \ln(A)$, and $C^* = \ln(C)$, the two seemingly different equations (Equations 2.1 and 2.2) are the same.

The moral of this story is that, if we were unaware of the different scaling rules employed by different researchers (Researcher A and Researcher B), we may believe that the researchers found support for different processes. After all, one researcher found a multiplicative relation between anger and conflict in predicting retaliation, whereas the other found an additive relation. However, transforming the measures demonstrates that the apparent differences were merely scaling artifacts: The differences in the measurements resulted in apparent differences in functional form. To deal with this issue, either all researchers need to use the same measures (unlikely in the social sciences), or researchers need to provide the rules that relate (translate) their measures to standard ones. In this way, the measures may be calibrated against the standard, and thus, to each other. As a result, debates over differences in functional form may devolve into differences in measurement rules. If the two researchers in the hypothetical example above graphed the relation between A^* and A, they would see that these two measures of anger were perfectly, though nonlinearly, correlated. In a fundamental sense, the measures are the same.

In the physical sciences, investigators employ fundamental measures for variables such as time, distance, angle, and mass. However, most other measures are derivative (i.e., they are ratios and differences) of fundamental standard measures. In human communication research, different investigators typically employ their own measures, which are not calibrated against any standard. As a result, it is very difficult to determine if process differences are merely scaling differences.

The third way of considering differences in functional forms is to treat these differences as an analytic issue. Using the above example, we can enter the two predictors and the dependent variable into a program like SHAZAM (White & Bui, 1988), which performs a Box-Cox analysis (see Bauer & Fink, 1983). Using maximum-likelihood estimates under the assumption that the population regression residuals are normal and homoscedastic, the program finds the optimal power transformations for the variables. With this analytic approach, Equation 2.1 and Equation 2.2 could be shown to represent equivalent functional forms.

Given a specified functional form relating a set of variables, is the process revealed by this functional form the same across the cultures under consideration? This question is whether the parameters relevant to the functional form are the same in different groups. Quantitative data may be analyzed within the general linear model. Time-series analysis and various forms of panel analysis and cross-sectional analysis require

assumptions about process to be informative; when these assumptions are met, each method can provide comparable information about the parameters of the process (Coleman, 1968). (Other methods, such as the analysis of categorical data using a Markov chain and its variants and log-linear analysis, can also be used to provide parameter estimates for theoretical processes; due to space limitations such methods will not be discussed here.)

Process differences across groups appear as differences in the relationship of inputs to outcomes. (This appearance is a necessary but not sufficient condition; we show in the next section that differences in the relationship of inputs to outcomes can indicate that the same process is at different stages in different groups.) Suppose we are looking at the process that relates relational distance between members of a dyad (X, the independent variable) and conflict avoidance (y, the dependent variable). What do process differences look like? First, conduct separate regressions, one for each group. If the slopes relating the variables are different (ignoring differences due to differential reliability between groups and assuming statistical or substantive significance of the difference), then the processes are different. Second, if one were to use "group" as a categorical variable (with two groups, assume a single dichotomous variable coded as a dummy {0, 1} variable, G), process differences appear as an interaction of group with the independent variable ($X * G$), because that interaction term indicates slope differences:

$$\hat{y} = b_0 + b_1X + b_2(X * G) + b_3G$$
$$= b_0 + (b_1 + b_2G)X + b_3G.$$

So, when $G = 0$, the slope relating X to $y = b_1$, whereas when $G = 1$, the slope becomes $b_1 + b_2$. (This analysis can be extended easily to multiple groups and multiple independent variables.)

Finally, if the researcher happened to ignore possible process differences between groups, a single regression might have been performed, without entering group as an independent (dummy) variable (or as a set of dummy variables). In that case the process differences will appear as a non-normal (here, with two groups, bimodal) distribution of errors around the regression line, and the errors will also have unequal variances around the line (i.e., there will be signs of heteroscedasticity). To demonstrate this idea, we have created simulated data with the following characteristics: X goes from 1–100, G is a dichotomous variable that = 0 if X is even and = 1 if X is odd, and $\hat{y} = .50\,X + .25(X * G)$. Thus, the process for the two groups (i.e., the group represented by $G = 0$ and the group represented by $G = 1$) is different: When $G = 0$, the slope for $X = .50$, and when $G = 1$, the slope = .75. Conducting a single regression that includes both groups, the (single) slope for $X = .625$. Figure 2.1 shows that the single regression line (with slope = .625) goes through the middle of the scattergram, and the residuals from this single line (the points above and below the regression line) have a bimodal distribution and their spread increases as X increases. Note that other patterns of heteroscedasticity are possible.

Unfortunately, the results of most culture and conflict studies do not provide enough information to reveal whether the slopes differ as discussed above. In general, the ability to detect slope differences and other differences in functional form is optimized when (a) the scales for the continuous independent variable(s) and dependent variable have many possible values (recall that count and amount scales have, in principle, an infinite number of values); (b) there is a broad range of scores on the continuous independent variable(s); (c) there is a broad range of scores on the dependent variable; (d) the measures are reliable; and (e) we have a large sample. Determining the existence of interactions between group variables and (continuous) predictors is a job both for the methodologist (Allison, 1977) as well as the theorist (Blalock, 1965). If we are comparing cultures and they are at different temporal locations in the conflict process, we may find parameter differences that do not reflect "true" process differences. We now discuss this issue.

Are the Cultures at the Same Place in the Process Under Investigation?

Finding slope differences would seem to make a prima facie case for process differences between groups. However, in most of the studies on culture and conflict, we do not know if

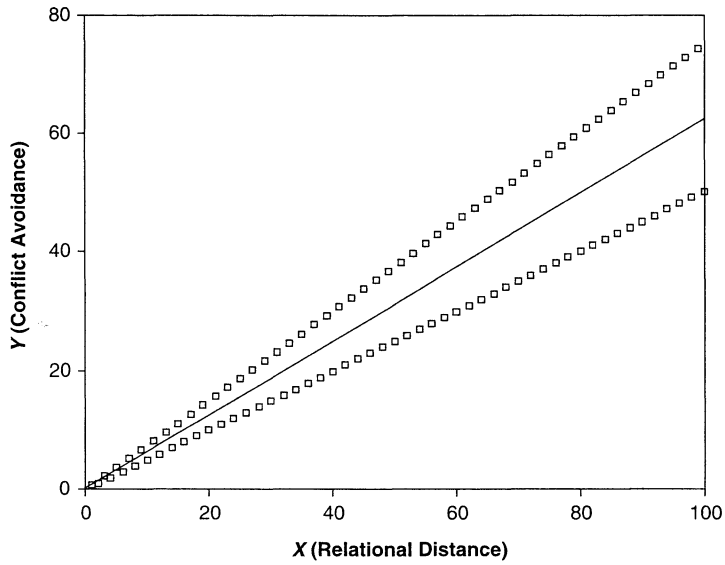

Figure 2.1 Hypothetical relationship between relational distance (X) and conflict avoidance (Y) when group membership is a dichotomous dummy variable and group (G) interacts with X. $\hat{y} = .50\,X + .25(X * G)$.

different groups are at the same place in the trajectory of the conflict process.

To analyze where a group is in a process, we must first establish that the process is stable. Statistical analysis of any process must assume that there is stability to the process, either in terms of the original variables and time points being used, or in terms of transformations of those variables or time points. Such assumptions are labeled differently in different analytic techniques, with terms like *stationarity* and *invertibility* in time-series models or the term *equilibrium* as used more generally. But it should be noted that a system whose variables do not appear to be in equilibrium (and whose trajectory appears to be unbounded) may, with appropriate treatment, be used to represent a system in equilibrium. For example, suppose conflict as measured appears to have an exploding trajectory. Over time the level of conflict appears to move as follows:

1, 4, 9, 16, 25, 36, 49, 64, 81, 100, ...

(This series obviously consists of the squares of the positive integers, but it will serve to make our point.)

How can we, with appropriate treatment of these "data," represent the process as one that is in equilibrium? If we take differences of adjacent values in the series, we get:

3, 5, 7, 9, 11, 13, 15, 17, 19, . . .

and if we take differences a second time, we get:

2, 2, 2, 2, 2, 2, 2, 2, ...

This example shows that a system that appears explosive may be represented as a corresponding system that is in equilibrium. Although this is an obviously contrived example, the point is that we can evaluate processes assuming that they have stability in some representation, and not necessarily in terms of the original variables.

If different cultures have parameter values that are time (or, more precisely, time-in-process) dependent, and the processes within the cultures can be aligned, we may conclude that the cultures are at different points in the same process. (To be clear, we are discussing the conflict

process over time. We do not mean that the conflict gets worse; rather, we mean that the conflict process is continuing over time.) If this were the case, perhaps the relation between conflict and dissatisfaction would look like the following:

$$\text{DISSATISFACTION}_t = b_1[1 - \exp(-b_2 t)] \text{CONFLICT}_t,$$

where "exp" is the exponential function and $b_2 > 0$. Note that when $t = 0$ (at the start), the coefficient $b_1[1 - \exp(-b_2 t)] = 0$, but as $t \to \infty$, $b_1[1 - \exp(-b_2 t)] \to b_1$. In other words, the effect of conflict on dissatisfaction is initially 0, but increases over time, approaching the value b_1.

Cultures may be assumed to manage conflict differently merely because the conflict system is at a different point in the process in different cultures. Thus, we may see that, in one culture, the coefficient relating conflict to dissatisfaction is about 0, whereas in another culture it is close to b_1. But the assumption that the process is different in these two cultures ignores the time dependence in the coefficient: It just may be that the process is identical.

"Aligning" the trajectories might reveal that a particular process is cross-culturally valid. To assess possible phase differences, we almost always require data at many points in time to create equilibrium among the variables of interest (by, e.g., differencing time-series data; see Hibbs, 1974). If the points in time are equally spaced but reveal cross-cultural differences in process parameters, we may still be able to determine if the processes are the same (Coleman, 1968), assuming that the points are sufficiently frequent in time to avoid the problem of aliasing (i.e., the spurious finding of a low frequency wave that arises because the original data are not sampled at a sufficient number of time points [Croft, 2005]; Arundale, 1980, applies this concept to communication).

Conclusion

This chapter is a very focused presentation of issues that we believe are critical for studying conflict and for understanding the extant literature. We have focused on various issues related to types of data, sampling issues, and specific issues related to cross-cultural conflict research. We are aware of the many issues not dealt with here; most important, we have not discussed research ethics, either in terms of internal matters (e.g., deception and potential harm to participants) or external matters (e.g., the sponsors and beneficiaries of the knowledge garnered from the research). For these matters we suggest Lewis (1975) and Sjoberg (1967). We expect that we can elaborate the comments we made here in the next *Sage Handbook of Conflict Communication*. Nevertheless, we hope the issues addressed in this chapter will stimulate careful thinking about research design and sampling that will result in research that contributes sound theoretical insights about conflict and communication.

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