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## On Defining Conversational Coordination and Rapport

### Abstract

The construct of rapport is arguably one of the central, if not the central, construct necessary to understanding successful helping relationships and to explaining the development of personal relationships. The role of nonverbal behavior in initiating and signaling rapport has its roots in the work of Watzlawick, Beavin, and Jackson (1967). Tickle-Degnen and Rosenthal substantially advance our knowledge about the relationship between rapport and nonverbal behavior in the meta-analyses presented in this and other articles (see Tickle-Degnen & Rosenthal, 1987). My purpose in this response is to offer a critical reaction to the conceptual analyses of this article and to end with what, I hope, are constructive suggestions.

## On Defining Conversational Coordination and Rapport

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The construct of rapport is arguably one of the central, if not *the* central, construct necessary to understanding successful helping relationships and to explaining the development of personal relationships. The role of nonverbal behavior in initiating and signaling rapport has its roots in the work of Watzlawick, Beavin, and Jackson (1967). Tickle-Degnen and Rosenthal substantially advance our knowledge about the relationship between rapport and nonverbal behavior in the meta-analyses presented in this and other articles (see Tickle-Degnen & Rosenthal, 1987). My purpose in this response is to offer a critical reaction to the conceptual analyses of this article and to end with what, I hope, are constructive suggestions.

For a construct to be a valuable one in scientific inquiry, it must meet a variety of criteria, some of which are internal to the construct itself and others external to the construct. The former concern questions such as conceptual precision, operational clarity, absence of tautology, among others. The latter, external criteria concern the utility of the construct in increasing our understanding of the processes under scrutiny. Let me take up each of these general criteria in turn as applied to the concept of rapport.

Rapport is defined as a feeling state experienced in interaction with another as interest, positivity, and coordination (or balance). These internal states can be manifested behaviorally through signs of interest and involvement, signs of positivity, and patterns of coordination, synchrony, or responsiveness in interaction, respectively. The authors make cogent arguments for the necessity of both the interest and positivity states being high for rapport to be present. What is not as clear is why the feeling of coordination must also be high. Certainly, the feeling of being "in sync" or in tune with another is a positive feeling state. But can people distinguish this feeling of positivity from the general positivity experienced in interacting with another? This raises the question of whether the third component is necessary or, rather, is simply another basis for general positivity and, hence, confounded with the first component.

If the argument is that actual behavioral coordination leads to positive feelings about the interaction and the other person, then that is a different claim, an important claim, and one not without empirical backing (see Cappella, 1988, for a review). Note that I am raising a question about the need for "felt coordination" as a necessary feature of the definition of rapport; I am not denying that behavioral coordination may give rise to feelings of positivity and mutual interest.

I also have some serious questions about the definition of behavioral coordination, both conceptually and operationally. Coordination could be defined in a wide variety of ways: micromomentary synchrony in movement and speech (Condon & Ogston, 1967), congruence of mean levels of behavior (Jaffe & Feldstein, 1970), adjustment of rhythms (Warner, 1988), responsiveness (Davis, 1982), topical coherence (Tracy, 1982), magnitude and direction of temporal adjustment (Cappella, Palmer, & Donzella, 1990), among

others. The authors offer their own operational procedures (to be discussed) but no conceptual definition of coordination that either includes or excludes these listed techniques.

The problem that the missing definition creates can be illustrated as follows. Suppose the behavioral activity of two conversants begins at moderate levels, and over the course of their interaction the activity levels trail off as the two become fatigued. Random, 10-sec samples will seem to exhibit coordination in behavior, not because the persons are adjusting to one another but rather because they are adjusting to an external force. Clearly, such baseline trends must be removed from the data before coordination (i.e., adjustment to the partner rather than adjustment to external, spurious forces) can be accurately assessed. Without a definition of coordination, we would be forced to accept spuriously caused similarity in behavioral levels as evidence of coordination.

Consider, as a second example, the escalation of hostile affect typical in conflicts between husbands and wives (Gottman, 1979). In one sense, the spiraling of action is a kind of coordination; each is responsive to the partner, matching and raising the ante. If this is an example of coordination, then it is certainly not a positive correlate of rapport; if it is not an example of coordination, then does coordination only apply to socially positive or neutral behaviors? If the latter, then how do we account for partners who are attracted to one another in part through adopting complementary patterns of dominant and submissive behaviors (Orford, 1986)? The absence of a definition of coordination does not allow researchers to assess the hypothesis that coordination is important to rapport in general; only in the context of the author's own operational procedures can the hypothesis be evaluated.

The authors might respond that their operational definition of coordination responds to these objections and to other problems. Their molar definition of coordination uses judges (or participants) to make evaluations of clips of interaction on scales estimating whether the participants are in sync or in tune with one another. The advantages of this approach are its efficiency in comparison to more molecular approaches, its use of context to assist judgment, and its simultaneous inclusion of interactional meaning with process concerns.

The disadvantages are equally serious. The spuriousness problem is not solved. Judgments of coordination, whether by participants or observers, could be confounded with judgments of positivity if judges' implicit theories of social interaction are that positive interactions are ones in which the people are in sync. If this is the case, then the judges would be assessing positivity and not synchrony, and the correlation to rapport would be an artifact.

To this, the authors might reply that the Bernieri, Resnick, and Rosenthal (1988) study shows that (a) artificial interactions created on tape to appear as if real and (b) interactions between mothers and strange infants are judged less synchronous than (c) actual interactions between mothers and their own infants. However, I do not think that this evidence is definitive. First, is it possible that the children who were

paired with the stranger exhibited some sort of apprehension or anxiety over being paired with an unfamiliar person and that this (possible) behavioral difference in the infants explains the differences between the true and the switched and double-crossed dyadic clips? The children tested were at an age when stranger anxiety is relatively high, and the coders could have cued into facial, gestural, or vocal differences between these infants and the others who were interacting with their own mothers.

My second concern focuses on the differences between the altered-time-frame clips and the true dyadic clips. For example, suppose, in the first 5 sec of Minute 1 of the actual interaction, the infant is fretting and that during the first 5 sec of the comparably paired minute (say, Minute 3) the mother is cooing and looking quite happy (i.e., exhibiting no signs of distress or concern). Such a combination, even if multiplied only once or twice across the 50-sec clips, creates an odd-looking interaction that may produce lowered ratings of global synchrony because of gross mismatches in emotion expressed, or activity exhibited.

Obviously, I am guessing about the character of the time-altered clips, but this rival explanation needs to be addressed. The only way that I can see to address this rival hypothesis and others that imagination might concoct is to actually code the behaviors of the mother and infant in the clips and see if the pattern of synchronous behaviors matches that provided by the raters. Many of us who study nonverbal behavior using molecular codings rather than ratings would be happy to move to the less costly rating procedures if it can be shown that coding and rating produce the same conclusions (if not the same detailed results).

In sum, the absence of a conceptual definition of coordination and possible weaknesses in its operational definition require further conceptual and empirical work before this component of the definition of rapport can be accepted.

The authors set out to capture the "nature" of rapport, to use their term. Such an enterprise is reminiscent of what Hempel (1952) called "real" definition and implies directly that rapport has a nature which is discoverable and that there exists some position that privileges such a definition. But on what epistemological grounds could one assess the success or failure of a natural definition? All such attempts are inherently circular and potentially tautologous. If rapport is defined as a tripartite feeling state involving interest, positivity, and a balanced or smooth interaction, how can such a claim be evaluated? Appeals to standard criteria of convergent and discriminant validity will not suffice because such appeals actually hide the implicit theoretical claims concerning what rapport should and should not correlate with and, in turn, presume what the nature of rapport is, tacitly hypothesizing its relationships with other constructs.

My own view of the scientific process is that it is constructive. That is, scientists do not discover the nature of entities through observation, natural language analysis, or the phenomenal experience of participants, rather they construct nature by the kinds of concepts and categories that they impose on it through their definitions and theories. Science is not the passive reporting of direct observations but the construction of observation, and hence reality, through theory. Such a view is held more by practitioners and historians of science (e.g., Polanyi, 1958) than by philosophers of science.

This view of concept formation as a constructive process

does not imply that any and all constructs are equally viable candidates for acceptance by the scientific community. Neither does it imply that the process of construct formation is nominal so that any definition is as sound as any other. Rather, theory intervenes again. Viable concepts are those constructions of the social and psychological world which enter into networks of propositions (i.e., theories) in such a way as to increase our understanding (i.e., prediction, causal explanation, and control) of the phenomenon under scrutiny. Thus, the evaluation of theoretical concepts in a constructivist science must be undertaken within a set of theoretical linkages, not within the framework of the concept alone.

In short, whether one attempts to carry out the process of construct explication as a realist or a constructivist, one must assess the value of the explication in terms of the theoretical success of the explication and not just in terms of the plausibility, utility, precision, historical adequacy, and so on of the construct in isolation. Thus, I next turn to evaluating the construct of rapport in terms of the theoretical ties it is proposed to have with other constructs.

I am afraid that the utility of the construct of rapport in improving our understanding of interactional processes and relationship development remains distant. The key theoretical claims involving rapport are found in the hypotheses of Tickle-Degnen and Rosenthal's Figure 1, the discussion of situational factors, and the relationship between feeling states and behavior. Their Figure 1 represents the relative weighting of the three components of rapport for different times of interaction (i.e., increased familiarity). The first problem with these hypotheses (and most hypotheses about interaction and relationship stage) is that the predictor variables do not separate affect for, from knowledge about, the other. When one compares strangers to friends on differences in interaction, one is comparing zero-knowledge, neutral-affect partners to high-knowledge, positive-affect partners. That is, knowledge and affect are confounded. The hypotheses of their Figure 1 would probably not be generated if the "late" or "familiar" interactions were ones of enemies rather than friends. The admonition here is simply that stages of relationships differ on several criteria, and if interactional differences between stages are to be understood, then these criteria must be sorted out.

The hypotheses of Figure 1 implicitly raise another important theoretical issue which the authors address. The behavioral signs of rapport will not always accompany the experience of rapport, due either to situational factors or to the mismatch between feeling and behavior. Because the association between rapport and nonverbal behavior is central to their analysis, discussion of the conditions under which the association will be amplified or attenuated seems a necessary, if major, task. The analysis of situational factors reduces to claims of the sort: When situations promote goals whose achievement requires attention, then the relationship between rapport and nonverbal attention will be greater. Such claims neither direct research in subtle ways nor offer informative explanations. In effect, that which promotes attention gets attention.

The authors are also well aware of the fact that affective state and behavior are not isomorphic with one another; the same nonverbal behavior sometimes functions in very different ways in response to feeling states. The study by Ickes, Patterson, Rajecki, and Tanford (1982) showing that smiling may be the result of two quite different feeling states, one

positivity and the other anxiety, is a representative case. If feeling states and their behavioral manifestation can shift in direction of association as a function of personal and situational moderators, then the theory will be very limited in scope until the way that moderators operate to alter the feeling-behavior relationship is specified. The authors do limit the scope of the theory holding that "the context of an interaction is one in which the individuals have . . . friendly, cooperative goals." The much more difficult question of the effect of moderating conditions on the feeling-behavior association is left unaddressed.

In sum, the theoretical value of the construct of rapport offered here is limited in its current stage of formulation. The components of rapport, both internal states and behaviors, may interact differently with relationship stage when the affective and knowledge components are separated. The role of situational and other moderating factors is acknowledged but the nature of their interaction with behavioral, internal, and relational states is not given serious theoretical consideration. The utility of the construct of rapport as defined by Tickle-Degnen and Rosenthal will depend on these theoretical advances.

#### Note

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## Rapport Is Not So Soft Anymore

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There once was a paper whose core  
Described three dimensions of rapport.  
Positivity, attention, and  
Coordination got mentioned,  
But there's got to be something more.

When one of the authors of this commentary was growing up, she was often fed a warm and tasty dish that made her feel good inside. Sadly, though, the dish was called "cornmeal mush" and so she was reluctant to extol its virtues to her more tony friends. Recently, though, cornmeal mush has been rediscovered and redubbed. Under the decidedly more suave name of polenta, it appears on the menus of the most chic restaurants, and the author can now mention offhandedly to her friends, "Oh, yes, my mother made that all the time."

The history of the concept of rapport is a bit like that of cornmeal mush. For a long time, rapport was just so mushy that serious scientists were a little embarrassed to be associ-

ated with it professionally, much as they may have enjoyed a bit of indulgence in private. But with the infusion of a bit of rigor and vim from the scientific stoves of Tickle-Degnen and Rosenthal, rapport doesn't seem quite so soft anymore. For this we should all be grateful.

The salvation of the scientific respectability of rapport has come none too soon. In the literature on survey research, for example, ominous titles such as "Interview Rapport: Demise of a Concept" began appearing more than a decade ago (Goudy & Potter, 1975/1976). But the articles bearing these titles were burying the old rapport—the cornmeal mush. The polenta was yet to come. One of the ingredients that renders the new rapport improved rather than just new is its construal as a genuinely interactive phenomenon. In a way, the idea that rapport necessarily refers to something about two or more people, and never just one, seems glaringly self-evident. Yet earlier conceptualizations of rapport often fell down on just this point. In the Goudy and Potter survey-research study urging the abandonment of the concept, rap-

