A Research Agenda for Relational Leadership

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Introduction

We are happy to have this chance to comment on the book chapters in Part 2, as it allows us to further the debate on a topic that is important for the social scientific study of leadership. We decided to reflect these views in a conversational format in which Gail will lead off and John follows with his comments on the chapters and on Gail's thoughts. After two more exchanges, the chapter concludes with some joint recommendations.

Fairhurst's first letter to Antonakis

Dear John,

To make sense of the chapters that were our assignment, I created Table 1. Using Gronn's (2002) distinction between ontological, observational and analytic units, I classified

each of the six chapters accordingly. I also added Mary Uhl-Bien's (2006) *Leadership Quarterly* paper as it was often referenced in the chapters. Here's what this table tells me. *Ontological units* define the object of study; in this case, it is the leadership relationship (or aspects of it) for all of the chapters and Uhl-Bien (2006). Interestingly, for *observational units*, which define who or what an analyst observes, Uhl-Bien (2006) locates communication at the center of understanding relationships "because it is the medium in which all social constructions of leadership are continuously created and change" (p. 665). However, the chapters mainly emphasize self-reports and storytelling by individual leadership actors. *Analytic units* detail that which is to be deconstructed, measured, or explained, and here we see the panoply of data sources in the third column in which interviews figure prominently.

[INSERT TABLE 1 ABOUT HERE]

If one takes seriously the charge to advance a relational agenda, I see a major disconnect in this table between Uhl-Bien's (2006) charge to place communication at the center of relational study and the chapters' heavy reliance on individuals reporting about communication or some other aspects of the relationship in story or survey form. Please note here that I am not against individual interview or self-report data per se; I have used plenty of this kind of data myself. What I object to is the overwhelming dominance of the individual as the observational unit when the ontological unit is relational. This is because people do not relate and then talk, they relate *in* talk (Duncan, 1967; McDermott & Roth, 1978). Recalling Gregory Bateson (1972), the exchange of messages *is* the relationship.

How so? There is a content and relational aspect to each message we formulate (Watzlawick, Beavin, & Jackson, 1967). The content deals with the subject matter at hand; the

relational specifies *how* that content is to be taken. So if I said to you, "Gee, John, you're looking good today" and I smiled and looked approvingly, you would likely infer this to be compliment. However, were I to smirk and speak sarcastically, you might well feel insulted or ridiculed. Note that the content is exactly the same in both messages. But the relational aspect (i.e., how I have defined myself in relation to you) differs when I signal politeness and approval versus sarcasm and ridicule.

While there could be great variety in the content of our communications, relational patterns are thought to operate within a narrower band width and stabilize over time with our every move and countermove (Watzlawick et al., 1967). Here the key is *not* to understand individual behavior, in this case, how much I complement or insult you. The key is to understand how you and I act relationally; in Weick's (1979) terminology, not our individual "acts," but our "interacts" and "double interacts" (how you respond to me, how I respond to you responding to me, and so on). Moreover, As Hinde (1979) so beautifully relays in the case of husbands and wives, it makes a relational difference whether partners consistently kiss after they quarrel or quarrel after they kiss—even though the amount of kissing and quarrelling may be the same. In short, relationship definition coheres as a sequence, and the temporal patterns that mark relational systems are always co-defined (Rogers & Escudero, 2004; Rogers, Millar & Bavelas, 1985).

The implications for a relational leadership agenda are clear (and, I might add, have been for awhile if one will peruse the date of the publications cited above). In addition to the plethora of research currently available on cognition and "what leadership actors mean" when they account for and story their leadership relationships, there must be a greater shift to "how behavior means" or indexing relational patterns (Scheflen, 1974).

Interviews and surveys ask individuals to retrospectively summarize the patterns that mark the relationship typically *from the perspective of only one person in the relationship*, thus implying that a single relational reality can be assumed (Rogers et al., 1985). (For example, this has been fairly standard practice in LMX research because leaders are putatively prone to give socially desirable answers.) However, this is an empirical question that goes to the heart of Uhl-Bien's (2006) recommendation to place communication at the center of relationships. According to Rogers et al. (1985), "The structural web…spun by recurring relational patterns emerging from ongoing message exchanges…(are) redundant, stochastic behavioral sequences" that are constitutive of the relationship and measurable as such (p. 176). These sequences must acquire a greater share of the spotlight in relational leadership research because whether the system is relational, organizational, or institutional, systems can *only* emerge from repeated interactions that evolve into multi-leveled orders of patterns (Bateson, 1972; Rogers & Escudero, 2004). If "process" is to remain as one the key value commitments of the relational agenda (Uhl-Bien, 2006), then we must find ways of apprehending it beyond the static depictions of relational processes that most leadership scholars currently favor (Fairhurst, 2007).

Admittedly, this is a good news-bad news mandate. The good news is that a small group of social scientists in psychology, communication, and management have been doing this work for some time now. Much of this work requires the development of coding schemes that are applied to talk and action. Relational analyses have been a focus for psychologists in therapy contexts (Watzlawick et al., 1967), among husbands and wives (Bakeman & Gottman, 1986; Gottman, 1982), and in mother-infant interactions (E. Tronick, 2007; E. Z. Tronick et al., 1998). I wrote a recent review of the literature in the organizational sciences (Fairhurst, 2004a), in which coding schemes applied to organizational interactions can be found in the study of group

processes (Bales, 1950; DeSanctis & Poole, 1994; Gersick, 1988; Olekalns, Smith, & Walsh, 1996; Poole & DeSanctis, 1992), bargaining and negotiations (Bednar & Currington, 1983; Putnam & Jones, 1982; Weingart, Hyder, & Prietula, 1996), leadership interactions (Courtright, Fairhurst, & Rogers, 1989; Fairhurst, Green, & Courtright, 1995; Fairhurst, Rogers, & Sarr, 1987; Gioia & Sims, 1986; Watson-Dugan, 1989), police communications (Fairhurst & Cooren, 2004; Glauser & Tullar, 1985), interviewing (Tullar, 1989), and computer mediated interactions (Walther, 1995). Most of these appear in management journals.

The bad news, and I speak from experience here, is that this work is highly labor intensive (Fairhurst, 2004a). It often requires verbatim transcript preparation, coding scheme development and reliability tests, stochastic analyses, and more. Coding schemes can also fall prey to leader centrism, such as we see with Komaki's (1986, 1998; Komaki & Citera, 1990; Komaki, Zlotnick, & Jensen, 1986) work on performance monitoring, or produce stereotyped superior-subordinate behavior, such as we see with Watson's student sample (1982).

Finally, coding schemes are not always as sensitive as they need to be to capture more nuanced relational dynamics (Fairhurst, 2004b; Firth, 1995; Gronn, 1982). For example, in my work examining how control is enacted relationally in leadership relationships, I focus on how actors' control attempts are met with acceptance or rejection by the other. Each turn at talk is coded as to whether it asserts control, acquiesces or requests control, or neutralizes the control move of the previous utterance. The "interact," or two contiguous control moves, is the basic unit of analysis in the search for relational patterns. However, my relational control coding scheme performs much better in a high reliability organizational context than a high efficiency organizational context because control dynamics are much more explicit in the latter (Fairhurst & Cooren, 2004).

Fortunately, more interpretive and qualitative relational analyses are available, such as with Harré and Langrove's (1999) discursive positioning theory, which focuses on first, second, and third order positioning akin to the double interact, and Brown and Gilligan's "Listening Guide" (1992), which attends to the polyphonic, nonlinear voices within a narrative (A. D. Brown, 2006). Other forms of discourse analysis such as rhetorical analyses or an ethnomethodology-informed conversation analysis are certainly capable of detecting relational patterns; it's just that it isn't necessarily their focus (Boden, 1994). Importantly, most qualitative analyses sacrifice the ability to gauge the redundant and stochastic nature of relational patterns marking the system for more nuance and detail in the sequencing of *key* relational moves and counter-moves. Depending upon the research question, the trade-off either way can pay dividends.

As I close out my initial recommendation for a relational leadership agenda, let me recap what I have said. First, as I looked over the chapters that we have been given to read, I was bothered by their heavy reliance on the study of individuals to discern relational patterns. Again, I am not objecting to the use of interviews and surveys per se to study relational dynamics; I am objecting to their overwhelming dominance.

Second, more process descriptions of the leadership interactions are necessary if we are to move beyond understanding relationships as an entitative state (Hosking, 1988). Those process descriptions are not likely to come from methods that promote summary judgments. The essential argument here is that the *pattern* of leadership interaction itself is its own best explanation of leadership dynamics.

Third, quantitative methods needed to assess the redundant and stochastic patterns that characterize leadership relationships already exist not just in psychology and communication, but

in the organizational sciences and in the study of leadership (Fairhurst, 2004a). However, coding schemes have their own limitations. More qualitative methods to assess key relational moments in more nuanced exchanges exist in the discourse analysis literature; however, we don't have the track record here as we do for coded interaction analyses in organizations. If communication is indeed at the center of understandings relationships, then *both* types of approaches must be taken more seriously by mainstream leadership researchers. John, your thoughts?

Sincerely,

Gail

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Antonakis's first letter to Fairhurst

Dear Gail,

I enjoyed your first missive, Gail. On the whole I agree with most of your assertions;

studying relations means going beyond many of the current research paradigms that are being

used. The table you created makes sense and helps to tease out measurement and levels-of-

analysis issues. So, I agree that there is an overreliance on individual-level observational units,

retrospective ratings, and not enough focus on communication processes that undergird

relationships. I guess that what strikes me as odd after having read the chapters and your article

(Fairhurst, 2004a), as well as other articles (see later) is that researchers are not leveraging the

methodological and technological advances regarding measurement of relationships. There are

methods, or at least methods that researchers could extend to suit the purposes of this research

stream that could disentangle the antecedents and consequences of relational leadership.

Before responding to your thoughts, however, I must first admit that after reading the

chapters, I am still not very clear about what relational leadership actually means and what the

implications are for its study. I thought it would be helpful for me, and readers, if I revisited the

original ideas behind this research stream. Reading the chapter contributions and comparing their

frameworks and research approaches with what Uhl-Bien (2006) proposed made it difficult for

me to determine whether the relational leadership research agenda is actually being concretized;

perhaps as an "outsider" (i.e., coming from a quantitative and "traditional" leadership perspective focusing on transformational and charismatic leadership) I am missing something.

Uhl-Bien (2006) defined relational leadership it as "a social influence process though which emergent coordination (i.e., evolving social order) and change (e.g., new values, attitudes, approaches, behaviors, and ideologies) are constructed and produced" (p. 655). Later, she defined it as "both *relationships* (interpersonal relationships as outcomes of or as contexts for interactions) and *relational dynamics* (social interactions, social constructions) of leadership. . . . [and] on the *relational processes* by which leadership is produced and enabled" (p. 667). Another definition of relational leadership is that it is a "process by which social systems change through the structuring of roles and relationships" (p. 668).

As you noted too, Uhl-Bien (2006) suggests that "relational perspectives do not seek to identify attributes or behaviors of individual leaders but instead focus on the communication processes (e.g., dialogue, multilogue) through which relational realities are made" (p. 664). Thus, the heart of relationship leadership is, according to its architect, communication. My confusion about the construct stems from several points. For instance, it is unclear to me at which level of analysis the theory operates and the way in which the chapter authors suggest to test the theory is concordant to the theoretical level of operationalization. Communication process occurs between actors: One to one, one to many, many to one, or many to many. It is not clear to me that, with the heavy individual-level emphasis, there is an alignment of the theoretical level with the level at which the authors propose to test their constructs; failure to consider this issue can lead to spurious findings (Schriesheim, Castro, Zhou, & Yammarino, 2001; Yammarino, Dionne, Uk Chun, & Dansereau, 2005). At this time, I agree with you that communication is not at the center of a relational-oriented theory although it should be, as should be the study of "wholes of

individuals," which is also possible to do from a quantitative perspective too (Muthen & Muthen, 2000; Muthen & Shedden, 1999).

So, there is a need to study wholes, communication, and to model the levels correctly. Such a goal is an ambitious one because it is clear that relational leadership is a multilevel theory. As such, the communication processes, their antecedents and consequences must consider these multilevel effects both in theorizing, observing, and testing. Relational leadership's cousin, LMX, which is a dyadic theory of leadership, fell into the trap of not correctly specifying observations in its empirical tests. I hope that this fate will not befall relational leadership. For example, LMX's unit of analysis is, theoretically, the leader-follower dyad (i.e., the level of analysis at which the phenomenon operates). The following quotation, in a recent article by Schriesheim, Castro, Zhou, and Yammarino (2001, p. 525) highlighted the gravity of not employing the correct level of analysis:

"Although it seems clear that the unit of analysis is the leader–subordinate relationship (Graen & Uhl-Bien, 1995) and that the level of analysis should therefore be the dyad, no LMX research has employed this level (Schriesheim et al., 1999). Instead, data have typically been collected from either just the subordinate or just the boss; when data have been collected from both, they have not been used in any type of dyadic analysis. Consequently, we believe that all the extant research is fundamentally uninformative about the LMX process because it has not studied the exchange at the dyadic level of analysis. . . . Future research, and paying careful attention to aligning the theory and the level of analysis at which LMX predictions are tested therefore seem urgently needed."

Thus, apart from the levels issue, I think that there is not enough "process theorizing." Going back to the Uhl-Bien (2006) conceptualization of relational leadership, as I understand it,

certain antecedent conditions and contextual factors, operating at multiple levels of analysis (e.g., individual followers and leaders, groups, organizational, environmental, time, etc.) engender certain evolving social orders over time, which lead to certain outcomes. This all happens because of multi-way communication processes; these are the core of the theory, whose effects are carried from the independent variables (whether they are individuals, variables, or something else). However, as you have noted, there is not much of such process-oriented theorizing around communication in the chapters and too much of a focus on individuals.

As for specifying antecedent conditions, I am reminded of House and Aditya's critique of (1997) LMX theory where they noted that "The distinguishing feature of LMX theory is the examination of relationships, as opposed to behavior or traits of either followers or leaders" (p. 430). They note further: "While it is almost tautological to say that good or effective leadership consists in part of good relationships between leaders and followers, there are several questions about such relationships to which answers are not intuitively obvious" (p. 431). House and Aditya (1997) note further that, "A specification of the attributes of high-quality LMX—trust, respect, openness, latitude of discretion—is as close as the theory comes to describing or prescribing specific leader behaviors. The theory implies that any leader behavior that has a positive effect on LMX quality will be effective. However, precisely what these behaviors are is not explicitly stated, as the appropriate leader behavior is dependent on anticipated subordinate response" (p. 432).

Thus, from a research point of view, "relations" are endogenous variables—they depend on other factors, and it is important to model these factors completely to better understand the process model that leads to dependent outcomes (Antonakis, Bendahan, Jacquart, & Lalive, in press). The consequences of the above are really important for correct empirical testing. Briefly,

if x is endogenous, then its effect on y cannot be correctly estimated unless the antecedent exogenous conditions of x (e.g., z, q, or what have you, whether they operate on the individual, group, organizational, or another level) are included in the model (Antonakis et al., in press). By exogenous we mean that the factors vary randomly and do not depend on other factors or omitted variables. That is, the exogenous factors must not correlate with the error terms of the systems of specified equations; if they do, then they have the same problem that the endogenous variables do, which means the effect of x on y cannot be identified (for those who are interested to learn about this problem, and the remedy, see Antonakis et al., in press). We discuss this identification problem extensively in a recent review piece, where, unfortunately, we found that much of the leadership literature is stuck with correlating endogenous variables with other endogenous variables. As we show in Antonakis et al. (in press), such descriptive correlations are not useful because they confound the effects of omitted variables. That is, the correlations could be overstated or understated or could be of a different sign from that of the true population correlation. Thus, what is important for me to mention is that using "relations" (in whichever form it is measured) to predict other outcomes is not very useful per se and that any coefficient capturing this relation has no inherent statistical meaning.

How can this research stream advance? Apart from the statistical issues I have identified above, there are many exciting contributions being made at the interface of psychology and information sciences that could be useful for researchers measuring communication—particularly real-time, naturally-occurring and open-ended communication. Many recent advances have been made in open-ended analysis of semantic meanings in text (see Foltz, Kintsch, & Landauer, 1998; Landauer, 1999; Landauer, Foltz, & Laham, 1998; Landauer, Laham, & Derr, 2004; Landauer, Laham, Rehder, & Schreiner, 1997). There are methods that

can even code human emotions (Picard, Vyzas, & Healey, 2001; Sorci, Antonini, Cruz, Robin, Bierlaire, & Thiran , 2010) or other social interaction processes (Paradiso et al., 2010; Pentland, 2010a, 2010b). These new technologies are currently available and can really help to advance research in relational leadership and in other domains and organizational behavior. More importantly, these methods can start to unify disparate research fields and to begin to model and quantify the unquantifiable.

So to recap, I largely agree with you; beyond the "communication problem" though, I think that it would be important to theorize, observe, and test in open-ended and causally-defensible ways so that the relational leadership research agenda can one day be reified. Sincerely,

John

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Fairhurst's second letter to Antonakis

Dear John,

John, as I read your response, I flashbacked to my earlier quantitative training. Indeed, had I stayed solely with this view of the world, I might be arguing for the same agenda as you. But something happened on my way to becoming a leadership communication scholar. I not only switched from studying surveys and 7-point scales to the routine work conversations of leadership actors, I switched from a being a quantitative interaction analyst to a more qualitative discourse analyst.

It wasn't consciously planned, and it didn't happen overnight. Ironically, it was my experience with LMX that triggered my shift to a more qualitative and constructionist discourse

stance. LMX founder George Graen was at my university for many years, and several of his students invited me to be on their dissertation committees. As a communication analyst, I knew LMX must be negotiated in social interaction (because telepathy has yet to be proven, but unconscious mirroring is a definite possibility as your last group of references suggest), and that retrospective summary judgments of the relationship were a far cry from relationships-as-they-happen.

So I designed a study in which I would measure LMX conventionally and collect routine work conversations from each participating dyad. As mentioned in my opening remarks, my coding scheme measured the control moves of every turn-at-talk; indeed, it seemed ideally suited to pick up the shared control and mutual influence of high LMXs and the more restricted control and unidirectional influence of low LMXs (Graen & Scandura, 1987; Graen & Uhl-Bien, 1995). To my surprise, there was no clear relationship between LMX and the interactional data (Fairhurst et al., 1987), but there were other findings (Courtright et al., 1989). In a second much larger study, LMX continued to be a no-show (Fairhurst et al., 1995).

However, I had made some modifications to the coding scheme between the first and second study and spent a good deal of one summer checking my coder's treatment of the interaction. It was there I discovered a world largely unknown to me until then. I found relational markers that were small, subtle, varied, and ubiquitous. They were part of the messy details of unadulterated speech that surveys and 7-point scales—and many coding schemes—tend to sweep under the rug like so many particles of dust. It was better to gloss them by folding them into "styles," "types," "patterns," or "qualities" and to view relationships in snap-shots of meaning instead of its ongoing negotiation.

Not only was my coding scheme not sensitive enough to the relational dynamics in play, it became difficult to imagine *any* coding scheme fitting the bill because of their endless combinations. Incidentally, the company I studied was the epitome of corporate America, blue chip in every way. Unlike the aforementioned high reliability organizations where time and safety issues push control out into the open, this is not the case in their high efficiency counterparts where subtlety, strategy, and even sleight-of-hand are the order of the day. This is *not* because folks are necessarily dishonest but because they carry the weight of multiple, co-occurring communication goals—an identity to manage, relationships to define, and tasks to complete *all at once,* especially in highly charged political environments. The broad strokes that leadership theory paints, and still paints, leaves much to be desired in capturing these dynamics. Suffice it to say that in discourse analysis, I found the tools to understand LMX and its relationalities (Fairhurst, 1993; 2007; Fairhurst & Chandler, 1989; Fairhurst & Hamlett, 2003).

So the bottom line is that because of my journey and experience with LMX, I only *partly* share your concern for levels of analysis. Deidre Boden (1994) in *The Business of Talk* captures some of my thinking in this regard:

> In the study of organizations especially, scholarship has become highly fragmented
> by virtue of a near-obsession with so-called 'levels of analysis'…Driven almost
> entirely by considerations that are rooted in methodological constraints rather than
> empirical evidence, quite a number of talented researchers critique or ignore each
> other's findings and theories based on essentially socially constructed, if
> methodologically tidy, distinctions that are features of data sets and statistical
> convention rather than properties of the real world. These many and separate levels
> are then treated as 'structure' and assumed to shape the behavior of microscopic

human actors. (p. 3)

The alternative that Boden articulates and that many discourse analysts like me embrace is to ground structure-in-action and to let *actors* demonstrate how levels of context reflexively interrelate. Let me address both of these points in more detail before I discuss their deliverables for a relational agenda.

Grounding structure-in-action. When reading the mainstream literature in leadership, I often get the sense that "relations" are something that befalls leadership actors rather than what they do. While this approach follows leadership psychology's emphasis on experience, it under-theorizes agency, making it easier to overlook the negotiated nature of relationships. In addition, as Boden suggests, "Framed as external, constraining, and *big*, the discrete actions of situated actors are treated as "effects," that is, as indicators, expressions or symptoms of social structures such as relationships, informal groups, ….and the like" (p. 12, emphasis original). In short, relationships structure social interaction, which is only half of the story.

What is the alternative? Like Boden, I favor Anthony Giddens' (1979; 1984) "duality of structure," which suggests that social structure is both a medium and outcome of social interaction. Here "structure" is short for, "society," "culture," or "our shared history," which boils down to a set of rules and resources that leadership actors draw from to deal with the matters at hand. As such, these rules and resources are also products of the interaction that actors' either reaffirm or modify depending upon how they have used them. In this way, agency is restored to leadership actors, yet kept within limits, because rules and resources are simultaneously enabling and constraining (e.g., see Fairhurst, Cooren & Cahill, 2002; Howard & Geist, 1995). Importantly, the study of leadership is grounded in task accomplishment, not "floating ethereally" above it as is often the case in the literature (Robinson, 2001; see also

Gronn, 2002). One key way to advance a relational leadership agenda is to ask about the resource base from which leadership actors draw, a point I will return to below.

Knowledgeable Leadership Actors. Garfinkel (1967), Giddens (1984) and others object to the widespread derogation of the lay actor in the social sciences. This view is based in Garfinkel's ethnomethodological argument that action is organized from within. This means that actors are knowledgeable agents who reflexively monitor the ongoing character of social life as they continuously orient to and position themselves with respect to the contingencies of the moment—looming deadlines, abundant resources, up-against-the-wall constraints, value commitments, role expectancies, and or other situational features that come into view. Note that what is paradoxical to the researcher may well be reasonable to the actor. Actors' language choices thus become a window on human agency because "actions and the interpretations of their meanings are inseparable and occur simultaneously in the course of their production" (Boden, 1994, p. 47). Moreover, actors can be responsible agents and still not fully comprehend or intend the full nature of unfolding events (Giddens, 1984; Ranson, Hinnings & Greenwood, 1980).

To attribute knowledgeability and reflexivity to leadership actors is to pay attention to how they account for their worlds. These accounts routinely surface in naturally occurring work conversations, but interviews can elicit them as well. Problems, breaches, surprises, or unmet expectations occasion in actors a need to explain, justify, or reconcile the "facts" of their world and their place in it. Such sensemaking is the stuff of accounts, and it is here that actors are most likely to indicate, and perhaps even negotiate, exactly which levels of context (and their features) factor into the action. Thus, I am arguing that levels of context need to be made *practically relevant* by the actors involved because the researcher's birds eye view of the world is just that—

a removed view of the actors' world given to (sometimes sweeping) generalizations about a host of contingencies (statistical or otherwise) thought to structure the course of events.

But, you might ask, have I not just contradicted myself given Table 1 and my earlier concern for the chapter authors' heavy emphasis on interviews and individuals as the unit of analysis? I would argue "no" because I am looking to study accounting practices in the sequencing of interaction in which levels of context are made *practically relevant,* most often, over multiple turns at talk where text becomes con-text and sensemaking is a collective achievement, not solely an individual one (Fairhurst, 2007). Put simply, the reporting of interview data must literally bring researchers into the interaction—treated as another "actor" if you will—to examine the ways in which *they too* make certain levels of context more or less salient through the questions asked and answered. Such a view is consistent with extending that which is "relational" to greater reflexivity in the actor and analyst relationship (Bradbury & Lichenstein, 2000; Dachler, 1992; Uhl-Bien, 2006).

With an orientation that grounds structure-in-action and attributes knowledgeability to leadership actors, there are two important ways to advance a relational agenda for leadership study. The first is the "resources" question mentioned above, and it is here that I depart from Boden's (1994) ethnomethodological argument. I prefer instead to focus on a poststructuralist alternative, Foucault's (1980; 1990; 1995) view of Discourse (capitalized to mark its distinctiveness from more standard use) as historically-grounded systems of thought—and its use by discursive psychology as a linguistic resource for communicating actors (Potter, 2003; Potter & Wetherell, 1987; Wetherell, 1998). Through Discourse, we see how *culture* influences the definition and formation of leadership relationships, and we know this because actors will invoke familiar-sounding terminology, metaphors and stories, habitual forms of argument, and

customary categories to name and understand "the relationship here and now" (see also

Fairhurst, 2007; 2011). The footprints of culture are in the linguistically familiar, much as Bennis

and Thomas (2002) found in their analysis of "geeks" and "geezers." These are two groups of

leaders, each marked by their own historical eras, orientations to work relationships, and *ways of

talking about their worlds*. The ways in which culture and its Discourses influence the formation

and maintenance of leadership relationships have not been a particular focus in the mainstream

literature (Fairhurst, 2007); however, its promise is best understood after explaining the second

research agenda.

A second research agenda for relational leadership draws again from Giddens (1984) who

argues that to put actors in charge of their own affairs in ways marked by both freedom and

constraint, they must continuously manage the tension between agency and constraint (structure).

Baxter (2011) recasts this argument in terms of the struggle between competing, often

contradictory Discourses in her theory of relational dialectics. Drawing from the dialogism of

Bahktin (1981), Baxter (2011) argues that Discourses not only animate talk, but often compete

with one another, more or less in zero-sum terms. She says, "(W)hat something means in the

moment depends on the interplay of competing (D)iscourses that are circulating in that moment"

(p. 3). More to the point, *"relationships* achieve meaning through the active interplay of

multiple, competing (D)iscourses" (p. 5, emphasis added).

In practical terms, the interplay of Discourses source dialectical tensions that create

simultaneous pulls to fuse with and differentiate from the other. Thus, relational bonding not

only implies fusion, closeness, and interdependence, but also separation, distance and

independence (Baxter & Montgomery, 1996). It is the working out of these tensions *through*

Discourse and in communication that forge the relationship and directly calls into question the assumption that relationships are generally stable (see also Lee & Jablin, 1995).

In the past, I've used relational dialectics (Baxter & Montgomery, 1996) to write about what those tensions might be for LMX (Fairhurst, 2001). Briefly, they include connection-autonomy where connection is as central to the LMX as autonomy is to individuals' identities. In openness-closedness, a second tension, the former is a prerequisite for LMX bonding, yet creates vulnerability necessitating the latter. Predictability-novelty is a third; too much predictability can create rigidity that ultimately necessitates novelty or change. Baxter's point is that the *strategic responses* to these tensions (and others) in communication form the basis for understanding *how* relationships are forged. For example,

> Member latitude in decision making is a form of autonomy that has been reframed as connection in high LMX relationships…However, the management of the autonomy-connection dialectic over the life cycle of the leader-member relationship has rarely been viewed as an ever-evolving negotiated process between opposite poles (Fairhurst, 2001, p. 420).

Indeed, other LMX actors may favor one pole to the exclusion of the other, alternate between them, or vary their tension management strategies using other contingencies (Seo, Putnam & Bartunek, 2004). Lee and Jablin's (1995) work suggest that even when LMX relationships are in a "maintenance" phase, there is potential volatility to be managed. Relational bonding can escalate or deteriorate, for example, over disagreements or conflicts that impact how open or closed LMX members choose to be in those moments. Finally, the predictability-novelty dialectic may take shape in far fewer scripted episodes and "secret tests" (Graen & Uhl-Bien,

1995) in high LMX relationships than low ones due, in part, to the interactional freedom (and, by implication, the potential for novelty) that trust engenders.

If attention to dialectics tells us more about *how* the LMX relationship is brought off, attention to the Discourses impacting LMX answers the all important "what" question as in, "*What* kind of LMX relationship are we talking about?" (Fairhurst, 2007). While it is hardly news that high LMX leaders and members likely draw from the more collaborative Discourse of teams (in which status differences are suppressed) versus the more authority-based Discourses of low LMX members, the role that gender, ethnicity, age, or education/training Discourses may play in the negotiation of medium LMXs is particularly interesting given that performance issues alone do not always decide LMX quality (Fairhurst, 1993).

The promise of relational dialectics is a more complex understanding of relational bonding, transformation, and disconnects processes. Moreover, the study of dialectical tension, contradiction, and paradox and their management are growing in popularity in the organizational sciences (Tretheway & Ashcraft, 2004; Collinson, 2005; Ford and Backoff, 1988; George, 2007; Mumby, 2005; Seo et al., 2004; Zoller & Fairhurst, 2009). I believe they offer similar promise for the study of leadership relationships.

John, I worry that I only have touched the surface of what is necessary to explain. But let me conclude by saying that I understand and appreciate that the function of most leadership theorizing has been to predict and causally explain leadership phenomena. Like Baxter (2011) and Boden (1994), I use theories more heuristically to make leadership relationships intelligible and open to insight in ways we would not otherwise have had. Traditional approaches want to answer cause-effect, *why* questions, while my goal is to answer *how* (as in, "How are leadership

relationships brought off?") and *what* questions (as in "What kind of leadership relationship are we talking about?").

Sincerely,

Gail

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Antonakis's second letter to Fairhurst

Dear Gail,

I appreciate your thoughtful comments Gail. Given that my strengths are in theory-building and testing, I would like, however, to still make the case that studying relationships *per se* should be accompanied by a systems model of theorizing and testing; this approach is what I think will advance LMX research.

I will motivate my argument with an example I use to teach my students about causality. I will call it the *clay-pigeon causality conundrum* (cf. Ketokivi, Bonardi, & Antonakis, 2010). I will go over this example in detail so that I can better explain the problem of "endogeneity" in the hope that researchers will better see why it is a good idea to develop process models that include exogenous variables (so as to truly understand whether relationships matter for organizational outcomes). I will discuss this example because I think the major point I tried to make was missed in my first contribution (because first explanation was rather short).

I would also like to make it clear that I write purely theoretical pieces (e.g., Antonakis & Atwater, 2002; de Treville & Antonakis, 2006; de Treville, Antonakis, & Edelson, 2005); however, I also do "hard-core" quantitative research to test theories (e.g., Antonakis, Avolio, & Sivasubramaniam, 2003; Antonakis & Dalgas, 2009; Antonakis & Dietz, in press; Fiori & Antonakis, in press). I see value too in gathering qualitative data and have taken doctoral-level coursework in qualitative case-study research; I do qualitative research too--at this time quantified content analysis (Antonakis, Angerfelt, & Liechti, 2010). So, I try to practice an inclusive and "ecumenical" scientific view of research though I lean heavily towards quantifying what I observe. As Maxwell--whose pedigree in the qualitative community requires no bruiting about—notes, "there are legitimate and valuable uses of numbers even in purely qualitative research" (2010, p. 476). My only goal is to use the best-available methods for the task at hand to improve our explanation of naturally-occurring phenomena. I am open to any kind of scientific method of inquiry that builds useful theory, which is the ultimate aim of science. I would be fine with any type of research, irrespective of the flag-waving paradigm that:

1. has carefully sampled their units to ensure that they are not sampling on the dependent variable;

2. demonstrates that they have sufficient units of observations to ensure that they understand and accurately model the phenomenon that they are observing;

3. can document that data were measured or coded and reported in a reliable manner; and

4. models causal explanations correctly.

I guess that most would agree that the point of science is to build theories; all we should care about is the rigor of the theory-building process and, where relevant, the rigor with which observations are used. It is imperative that I get my point across this time about the problem of endogeneity in a vivid and easy-to-understand way, because I think that the viability of the LMX construct (and other constructs too) depends on researchers understanding the problem of endogeneity. Whether they use a quantitative or qualitative mode of inquiry is irrelevant. I will focus my efforts on explaining the problem of endogeneity and how it relates to LMX because all scientific endeavors are about, or should lead to theory building; endogeneity, whether in the empirical or theoretical form, is but two sides of a coin, so understanding it from an empirical point-of-view will hopefully make the theoretical problem more salient.

Unfortunately, as a recent review that my colleagues and I undertook demonstrates, most researchers in management and in related areas do not quite realize the insidious effects of endogeneity (Antonakis, Bendahan, Jacquart & Lalive, in press)—that it threatens theory-building endeavors follows as a direct consequence. Although our review was applicable to quantitative research, qualitative researchers are not immune from the criticism we made, and in particular because their ultimate goal should be to build theories too. As we all know, theories are causal explanations of phenomena, though many researchers shy away from using causal language when providing an account of a phenomenon and prefer to couch their causal language

using other suggestive terms. However, they should not (cf. Pearl, 2000; Shipley, 2000), and they should focus on explaining the causal mechanisms behind what they state.

All theoretical explanations have implicit or explicit causal claims by definition. When a researcher states that *x* is associated with *y* and goes on to explain *how* (i.e., the mechanism linking *x* with *y*), she is making a causal claim. If she does not, the claim that is made cannot help science advance. Associations or patterns do not help science advance, as I will explain in a little while. What I wish to show is that claiming that *x* and *y* are correlated, related, or associated (or to use more qualitative vernacular co-occur or match-up or what have you) is not very useful to society if this relationship is due to other *unmodeled* causes. Although I think more could be gained by quantifying what we observe, quantification is irrelevant for the point I am making. What I care about most is that the theory we use to explain a phenomenon or a process is accurate; that's all. So researchers should use LMX in whichever way they wish to as long as they deal with the endogeneity problem upfront.

Note also that it has been claimed that quantitative research is "variance-driven" whereas qualitative research is "process-driven" focusing on "how" and "why" questions (Maxwell, 1996; Yin, 1994); thus, relations between variables are (apparently for some researchers) irrelevant for qualitative research. At the end of the day, however, both qualitative and quantitative researchers make implicit or explicit claims about how variables are *causally related* to one another, whether in a process or a conditioned relationship. Qualitative researchers claim to want to generalize to the theory (and thus avoid using causal language of the quantitative type); however, implicitly what they state is causal given the fact that they develop chains of events and patterns of occurrences (cf. Yin, 1994).

For example, if *a* is linked to *b* (and then one goes-on to explain the *"how's"* and *"why's"* of this relationship), then there must be some causal process that has engendered the link between *a* and *b*. That is, if the researcher claims that they find *b* whenever they find *a* then *a* and *b* are somehow associated. Indeed, Maxwell (2010, p. 477) notes explicitly that the point of qualitative research is to uncover "actual causal mechanisms and processes that are involved in particular events and situations." To uncover the causal mechanisms one must first implicitly or explicitly articulate *what* is causally linked. Only then can the *how* and *why* questions be adequately answered. If I can get qualitative researchers to buy-in to this point (that explanations of chains or processes imply first showing associations), then the arguments I will make below will follow logically. However, it seems to me that this type of causal thinking is not the province of qualitative research, not because qualitative researchers cannot handle causal thinking, but because causal thinking of this kind is has not yet become apparent to qualitative researchers (as it is still not apparent to most quantitative researchers). For instance, here is a typical account of what process theorizing is apparently all about (Maxwell, 2010, p. 477):

> Process theory . . . deals with events and the processes that *connect* them; its approach to understanding relies on an analysis of the processes by which some events *influence* others. It relies much more on a local analysis of particular individuals, events, or settings than on establishing general conclusions and addresses "how" and "why" questions, rather than simply "whether" and "to what extent." [italics mine]

What does *connect* or *influence* mean actually above? It seems to me that it means "linked"—a connection can only be a link or an association. If two entities are connected they are bound somehow; when one is found so is the other. Ditto for "influence," which is even closer to making a causal claim (however, we are not concerned about causality at this point but

by the fact that process theorizing implies associations or relations). Also, if *a* influences *b* then each time *a* is present *b* will be present; or, each time *a* changes its form or intensity, *b* changes too. Having established (I trust) that qualitative research is rife with implicit or explicit causal associations too, I move to the next aspect of my argument: *That associations are not helpful for establishing theories*. I say this explicitly too about quantitative research (cf. Antonakis et al., in press), so please do not take this as critique of qualitative research only.

The clay-pigeon causality conundrum: Suppose that a philosopher from the ancient times were transported in the flesh and skin by a time machine to the present day. She is brought to a field in the countryside and asked to make observations about a naturally-occurring phenomenon. She has no prior theories or expectations about what she will be observing; she is an objective bystander with no *a priori* theory about what to expect to see and what caused it. Now, on one side of the field is a shooter and on the other side a clay pigeon thrower; however, both are not observable to her because they are hidden behind thickets. Suddenly the philosopher sees a disc of sorts streaking across the horizon. Then she hears a deafening "crack" and, instantaneously, the disc disintegrates to smithereens. The process is repeated several times. In fact, almost every time she hears the "crack" the disc shatters. Being the keen observer that she is, the philosopher links the sound (*x*) to the disc disintegrating (*y*). Even in the absence of the counterfactual (i.e., what would happen to *y* if *x* was not present), she infers that it is highly probably that *x* must have caused *y*. She then goes on to build a theory about *how x* probably caused *y* and *why* this may have occurred. After much thought and insight she supposes that the sound waves from the loud crack shattered the disk so she develops a nice theory around that. Based on what she has seen, this is a plausible anecdotal account of what has happened and it can even be nicely quantified with a chi-square test. The philosopher, however, is wrong.

Now, here is an example of observing an association that is quite clear and appears quite evident (and can even be tested for statistical significance); however, it is a specious association. Does this help us to understand the phenomenon at hand? No, because it is not the sound (x) but the birdshot (z) that caused the disc to shatter (y). Thus, in the absence of modeling the common cause z, it appears that x causes y; however, x and y both simply co-occur as a function of z. In fact, there is no relationship between x and y once the true causal relationship among the variables is accounted for by z. Important to understand here is that x (and of course y too) is endogenous; x does not vary independent of any unmodeled causes of y (i.e., z, which is the exogenous variable causing x is excluded from the model). Accounting for this endogeneity would show that the residual correlation between x and y is naught, *even though* the observed correlation between x and y is not zero. A detailed example with some basic algebra and Monte Carlo simulations explaining this problem is documented in Antonakis et al. (in press). Again, it is irrelevant to me whether the theoretical explanation follows from an inductive or deductive process, and irrelevant too whether an association between variables is expressed theoretically, or observed and modeled qualitatively or quantitatively.

Thus, finding associations (in the quantitative or qualitative sense) between endogenous variables is not a useful endeavor per se. As with the gunshot noise, LMX is endogenous. It is caused by something. Thus, studying LMX and then "correlating" LMX, whether in the quantitative or qualitative sense with other dependent variables or explaining processes that rely on correlations or associations is not a useful endeavor unless the "something" that is behind the scenes is better understood. If the philosopher had poked around a bit, she might have discovered the shooter and then realized what was truly going on. Then her account of the *how* and *why* of the disc disintegration would have been dramatically different, and of course more accurate!

I hope that my point of view regarding the fact that *how* and *why* questions depend on understanding processes and associations between entities or variables is clear. Whether one chooses or not to quantify these associations is not the issue here. To go back to what I said before, the problem that House and Aditya (1997) identified had to do with causes of LMX, as well as the problem of simultaneity (simultaneous causality). I would take the critique further and note that the whole system in which the relations occur need to be modeled. To accomplish such an ambitious goal one has to go beyond the simple 7-point questionnaires or studying the single-sided (leader or follower) views of LMX. That is why I pleaded in my first missive that researchers must take advantage of the technological advances that have been made to reliably gather (and ideally quantify) naturally occurring data that can capture the entities that really matter for understanding LMX.

Sincerely,

John

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Conclusion

We enjoyed this exchange and found each other's perspectives interesting and useful for

advancing relational leadership. So where does this leave us, given that we come from

contrasting views of science? On several points, we have to agree to disagree. However, here's where we do see some agreement for a relational leadership agenda:

1. The papers we have been asked to respond to are generally marked by an overreliance on individual-level observational units when the ontological unit of study is the leadership relationship. Analysts must do more to match their analytic and observational units with the ontological unit under study.

2. The papers show a heavy reliance on retrospective ratings and not enough focus on communication processes that undergird leadership relationships. As such, there is not enough process theorizing about relational communication insofar as leading (and managing) are concerned. Analysts are likely going to have to leave their comfort zones in order to do this type of theorizing and research.

3. Coding schemes are one way to capture relational processes; however, they have their own limitations (e.g., they may lack sensitivity to more nuanced relational dynamics) that must be recognized. Furthermore, technological advancements must be harnessed to capture naturally-occurring and dynamic open-ended data.

4. The meanings for the term "relational" are multifarious. Analysts must carefully specify their use of the term and then align their mode of scientific inquiry accordingly.

5. Systems-wide contextually-relevant data (broadly defined), whether it is qualitative or quantitative, should be gathered to better understand the relational leadership phenomenon.

To conclude, although we have contrasting views of science, we agree that the point of science is to develop good theory. There is also little value in perpetuating a war between quantitative and qualitative methods; each perspective has its advantages and disadvantages.

While we may never see a day when researchers will use a unified "post-paradigm-wars" mode of scientific inquiry to study leadership, the drive to understand leadership and its complexities is our common bond.