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A Difference We Can Call Our Own

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A Difference We Can Call Our Own¹

Jake Harwood

The article contends that communication scholars' most influential work is often not identifiably "communication" research. This phenomenon is a result of: (a) theory, which

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emphasizes message effects rather than message content, and (b) method, which has failed to provide valid, detailed, and shared methods for the quantitative examination of message content. It is suggested that a stronger focus on message content is required if communication is to maintain a disciplinary identity and a unique disciplinary contribution.

Keywords: Communication; Message Content; Message Effects; Theory; Method

The fact that we are having this discussion indicates that we have not made that much of a difference. If communication scholarship had brought about peace in the Middle East or reduced the divorce rate by 50%, this dialog would not be occurring. Medical journals do not feature discussions of whether antibiotics have “made a difference,” and structural engineering journals are not too hung up on whether physics is important in stopping bridges from collapsing. We, however (much like other social sciences) are studying behaviors and cognitions that are determined by multiple fuzzy forces—individual, relational, and societal—and we are doing so with tools that are certainly less accurate and perhaps less sophisticated than those available to physicists. This is not unique to communication—other social sciences could have a similarly difficult time pointing to direct concrete outcomes of their research that have really changed society (although social psychologists, for instance, might find the exercise somewhat easier than we do). As the original contributors to this forum made clear, there is excellent research that we can point to as having made a difference.

However, we have not made as much of a contribution as we might; more important, we have not made a contribution that is as disciplinarily distinct as we might—a difference we could call our own. Reading the work of the fine scholars cited in Hummert’s and Frey’s opening pieces, for example, one would be hard-pressed to identify the feature that makes that work “communication scholarship.” Much of it would be well suited for publication in psychology, gerontology, sociology, or health journals, and indeed that is where much of it is published. Ask whether communication scholars have produced identifiably communication research published in communication journals that has had a substantial societal impact, and the evidence is considerably slimmer. I elaborate briefly on two potential explanations for this.

Theory

Journal editors in the field of communication (including me) are rather hung up on theory. Consequently, communication scholars tend to focus their work theoretically, and train their students to do likewise. We have learned a great deal from such work and we should not stop doing it. However, in the focus on theory we may have lost something. Careful, objective, descriptive analyses of the content of messages—messages being the fundamental object of study which distinguishes communication

from other disciplines—are rare in our discipline. When they do occur they are often given little attention and not published in our flagship journals. In the interpersonal area, there is considerably more work looking at self-reports or psychological residuals of communication (e.g., satisfaction), rather than looking at actual messages (I am guilty on this charge). Content analysis among mass communication scholars is more common, but tends to get treated as a second class activity, in part because of it being “insufficiently theoretical” (a quote from reviews I have written and received). Meanwhile, message effects studies often leave the reader wondering what the message was. Our field is working with a priority structure that does not incentivize work which might yield useful information about the fundamental object of our study. Two solutions are apparent; either we acknowledge that there may be good work which is not theoretical and we convince editors to publish it, or we think about how message content might be made more central to our theories—a bit more on that latter point shortly.

Method

A second reason why we do not always do a good job of studying messages is because of their complexity. The flow of communication is analytically mind-boggling, both in its micro-level complexity and its volume. Still, as the field claiming dominion over messages, the challenge of understanding and analyzing message content should be a significant puzzle with which we are constantly struggling. Instead, amidst the complexity I sense that we have remained methodologically rather stagnant on this front with few new tools emerging for examining message content. Content analysis continues to rely on painstaking and one-off manual coding systems developed for specific research projects—these limit our ability to handle large volumes of messages or to build comparative bodies of knowledge over time. Computer-assisted text analysis may be the only tool available for examining huge databases of messages, but it is a peripheral interest in our discipline. The development of standardized, valid, and shared methods for analyzing message content seems to be substantially behind where it should be. Perhaps the *Communication Methods and Measures* journal offers a ray of hope here.

In closing, I argue that we have not studied real communication enough, in part because detailed analysis of messages is not rewarded, and in part because we lack good tools. My lens here is that of a quantitative social scientist. I readily acknowledge that scholars in conversation analysis, rhetoric, discourse analysis, and other qualitative areas have long been engaged in a deep and sophisticated focus on message content. Their work does not provide precisely the tools that would be useful for quantitative scholars attempting to develop, say, causal models, but rhetoricians et al. may provide ideas for how we might address message content in more sophisticated ways. As we do so, we should think carefully about developing strong social science theory that is focused on message content. We tend to think that theory must incorporate precursors or effects of messages. But what about theory that specifies how elements of messages relate to each other, or that describes sequential

development within messages; what about a theory that specifies how half-hour sitcoms are organized in terms of narrative and dialogue? If research based on such theories made a difference, then we could most certainly claim that difference as our own.

Note

- [1] This essay is part of a joint *Communication Monographs* and *Journal of Applied Communication Research* special project titled, "Has Communication Research Made a Difference?" The other responses to the joint forum can be found in *Communication Monographs*, Volume 77, Issue 4 and the *Journal of Applied Communication Research*, Volume 38, Issue 3.

Making a Difference: (Re)Connecting Communication Scholarship with Pedagogy¹

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If communication research is to make a greater difference in society, change is needed in the ways that communication scholarship is taught to and produced by students. To do so, I contend that changes must occur in two areas. First, change must occur at the undergraduate level where students must be given the opportunity to become directly engaged with scholarship through service learning. Second, change is necessary at the graduate level where students should be challenged to use and conduct applied research. Overall, I argue that for research to make more of a difference in society, change must first be made inside the academy. If students learn how to apply and conduct research that makes a difference, they can become the vehicles to take that research from the academy into society.

Keywords: Applied Communication Research; Service Learning; Action Research; Undergraduate Education; Graduate Education

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