THE FOLLOWING NOTE WAS POSTED AS A "REACTION" ON THE SOCIOLOGICAL SCIENCE WEBSITE ON JULY 13, 2015. IT WAS SUBSEQUENTLY REJECTED BY THE SOCIOLOGICAL SCIENCE EDITOR-IN-CHIEF.

THIS REACTION WAS IN DIRECT RESPONSE TO THIS REACTION BY PROF MICHAEL HANNAN WHICH WAS IN TURN A RESPONSE TO THIS REACTION OF MINE WHICH REACTED TO THE FOLLOWING PAPER:

Conceptual Spaces and the Consequences of Category Spanning

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I appreciate Prof Hannan's engagement with points a and d in my "reaction" to his paper with Kovacs.

I confess that I did not understand Prof Hannan's clarification re point d (in his point 3), so hopefully we'll get an opportunity to discuss that at some point, as well as points b and c in my "reaction."

The remainder of Prof Hannan's response—his points 1 and 2—responds to my point a. There seem to be three issues, none of which should overshadow my appreciation for the useful measurement strategy and findings that Kovacs/Hannan present in their paper.

The first issue has to with the extent to which the past literature had supposed "that all kinds of spanning... have the same consequences" and specifically whether the paper does enough to clarify how the measurement strategy it proposes relates to the measurement strategy adopted in prior papers. (In addition to the precedents that have already been noted in our exchange, I would also add Smith [ASQ, 2011; see how "typicality" is measured] and Zuckerman [ASQ, 2000; "relatedness"] and Zuckerman et al [AJS, 2003; "concentration"].) I will let readers decide for themselves whether these (and perhaps other papers of which I'm unaware) are precedents and whether the onus is on the reader (as Prof Hannan suggests) or the authors (as I suggest) to clarify how the proposed measurement strategy relates to such precedents.

A second issue concerns my assertion that the past literature had not "relied" on measures that disregard distance between categories. After reading Prof Hannan's response, I realize that I could have been clearer. Prof Hannan shows that many past papers have used such a measurement strategy. So how could I have claimed that I can't think of one paper that did? Good question. In fact though, I didn't say that no one had employed such a strategy, just that they hadn't *relied* on it. (Note that I don't have a particular reason to defend these papers since I have never used such a measurement strategy.) In particular, I don't think anyone in the literature would doubt that incorporating info on distance between categories can sometimes increase explanatory power; the question is whether the gains in explanatory power outweighed the loss in parsimony in the particular cases they were analyzing. It is notable in

this regard that Prof Hannan does not go far as to say that we should have less confidence in the lessons drawn from those papers. This is the sense of "reliance" that I thought that Kovacs/Hannan's strong motivating statements implied. But perhaps I misread their intent. Alternatively, perhaps this was their intent but I am giving the literature too much credit when I suppose that scholars were not relying on the idea that distance between categories is unimportant.

Finally, Prof Hannan also raises an interesting issue with regard to PTZ.

(Prof Hannan raises two additional issues with respect to PTZ—whether the data in PTZ are sufficiently "systematic"; and whether the story in PTZ is just an instance of "oppositional cultural codes" as already explicated in Hannan-Polos-Carroll. I would just ask reader to judge for themselves whether the evidentiary base in PTZ supports the claims that it makes and how PTZ situates those claims relative to the prior literature.)

In particular, I am intrigued by Prof Hannan's remark that we should give priority to "general considerations" over "specific information" when there seems to be a conflict between them. My own view is that this is indeed a useful strategy insofar as our goal is to make predictions using variables that are distant from the underlying mechanisms. But this is not the goal that I'm usually pursuing in my research, and I think I am not alone in this regard. My interest is not in identifying *general associations* between two or more variables but in identifying *general mechanisms*, the effects of which are often dominated by other mechanisms such that they are often unobserved in associations between variables. Instead, general mechanisms are best revealed in "strategic research sites" whose properties and opportunities for measurement allow us to observe them.

For example, in our paper on "middle-status conformity," Damon Phillips and I emphasize a point that is often lost on users of the theory—i.e., that given the scope conditions on the theory, the parabolic relationship between status and conformity will be observed only in a "narrow range of contexts (P&Z 2001, p.382)." That is, we do *not* expect the parabola as a general rule. At the same time, *the mechanisms* involved—i.e., that conformity [with respect to certain norms but not others; see PTZ] will (a) increase with an actor's desire to be recognized as a member of social category; and (b) decrease with an actor's sense of security as a category member—are quite general.

Conversely, once we see in PTZ (due to the choice of research site and the data's ability to shed light on mechanisms) that a purportedly general mechanism is absent in a specific case where some theory would lead us to expect it, this one case is sufficient to raise questions about the purported generality of that mechanism and suggests that prior results may be consistent with a different/revised theory.

Actually, one reason I was surprised by the general considerations/specific information remark is that I think this is the very approach that Kovacs/Hannan were pursuing in their paper. That is, they seem to be suggesting that we should revise our understanding of general mechanisms

based on results from one study (on two industries). The key reason is that they are able to deploy a measurement strategy that takes us closer to the general mechanisms. My point b was not intended to question this general strategy but rather their characterization of what our prior understanding was. But perhaps I misunderstood the strategy, and it is clear that we will need to agree to disagree regarding the prior understanding in the literature.

Thanks again to Prof Hannan for engaging and to Prof Kovacs as well for having stimulated this engagement. And thanks to Sociological Science for facilitating.