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A Fundamental Conceptual Distinction…Gone Unnoticed

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I do not believe that I have ever prepared a manuscript without a strong sense of enthusiasm regarding the importance of the work and the contribution it offered. Indeed, the eager anticipation of the impact that I am convinced a paper-in-preparation will have regularly facilitates its reaching fruition. In some instances, that anticipation has been justified. The work has been read and cited, and hindsight shows it to have nudged the field's thinking in the direction I had envisioned. On occasion, a work's impact has far exceeded even the wildest expectations I could have ever imagined. However, the opposite has also been true. What I viewed at the time (and in a few cases continue to view to this day) as a brilliant conceptual insight or remarkable empirical finding has languished in the literature, largely unnoticed by the scientific community.

One of the languished has been especially striking, even poignant to me, for a long time now. Hence, when my colleague and friend Bob Arkin, in his role as editor of this volume, posed the challenge of identifying my "most underappreciated" work, little or no latency was involved. (A good deal of my more appreciated work speaks to the telling nature of responses offered with quick latency, so I place considerable trust in my effortless reaction.) The answer clearly had to be my dissertation work.

In all likelihood, the poignancy to which I referred stems from this having been my dissertation. When introducing a class to a significant piece of research, I have always taken considerable delight in identifying it as some now eminent scientist's dissertation. Somehow, that little tidbit of trivia seems to personalize the science and, at the same time, illustrate its cumulative nature. The passing comment connotes that even very recognized scholars were once students earnestly committed to the completion of a PhD thesis. I am quite sure my own dissertation has never been featured in this way (other than the lecture equivalent of a self-

citation, that is). Indeed, I suspect that only a very small number of people, few beyond my

advisor and my spouse, could even identify what I did as my dissertation research. Although

published in the *Journal of Personality and Social Psychology* (Fazio, 1979), the article

reporting this research never garnered much attention. As of 2010, it had averaged all of 1.19

citations per year.

Yet, I continue to look back at the work with considerable pride, as well as with the

counterfactual musings of what might have been. Its essence concerned what I termed the

"construction-validation distinction" – collecting information about an object for the purpose of

constructing a judgment of it, versus testing the validity of a judgment already reached. The

distinction centered on the nature of information being sought, information about the object

versus information about one's judgment of the object. I described the construction motive as

parallel to a researcher's conducting an exploratory investigation with the intent of gathering

data that would facilitate the generation of hypotheses, whereas the validation motive was akin to

conducting an empirical test of a hypothesis.

The distinction was prompted by, and embedded within, social comparison theory. At the

time, Festinger's theory was enjoying a resurgence of interest, evidenced by the publication of

Suls and Miller's influential edited volume reviewing theoretical and empirical perspectives on

social comparison processes. Among those developments was the one that most influenced my

own thinking – the attributional perspective espoused by Al Goethals and John Darley (1977).

With the benefit of invaluable input from my advisor Joel Cooper, the dissertation project linked

the construction-validation distinction to the long-standing issue in the social comparison

literature regarding choice of comparison other.   Drawing upon Goethals and Darley's analysis

of the value of triangulating evidence and the characteristics of a comparison other most likely to

provide such triangulation, I argued that a validation motive would be best served by someone who showed evidence of agreeing with one's judgment despite their being rather dissimilar on dimensions related to the judgment. As Goethals and Darley had noted, agreement from dissimilar others, by virtue of the different perspective they bring to bear on the judgment, provides evidence that one's own judgment emanates from the entity in question and not some personal idiosyncrasy. In contrast, a construction motive, I argued, might be best served by engagement with a similar disagreer. The commonality in terms of relevant attributes suggests that such a person should be agreeing with one's own judgment. The disagreement implies that the person may possess some information about the entity that you yourself lack.

This reasoning was supported in a series of three experiments, ones for which I fondly remember the process of data collection. Indeed, these had to rank among the occasions that I most enjoyed the experimenter role. The procedure involved the elaborate staging of a ruse to convince participants of the presence of co-participants, which required such foolishness as stomping loudly along the corridor in heavy boots, repeatedly opening and closing the doors to empty booths, and talking to nonexistent people. Moreover, the experiments, which purportedly concerned "the perceptual judgment of motion and distance," focused on the autokinetic effect as the judgments of interest. As Sherif had demonstrated decades earlier, estimating the distance that a dot of light appears to move in a completely dark room is fraught with ambiguity. Presumably to simulate different ways in which judgments are made, participants undertook a series of trials in which they offered independent judgments, followed by a series in which they were informed of each other's judgments at the end of each trial (during which agreement was manipulated). This was to be followed by a "coalition series" during which pairs of participants

would discuss their judgments over an intercom system prior to reaching a joint decision. Partner selection was the major dependent measure.

Some of the experiments also involved the manipulation of similarity or dissimilarity with respect to "perceptual style." This was accomplished via the provision of bogus test profiles on what were purported to be perceptual tests of depth versus lateral perception. To foster concerns with construction versus validation, participants' perceived level of information about the distance that the light might move was manipulated. Participants were led to believe that they would be assigned to one of three conditions involving varying degrees of information about the light's likely movement: range information for each trial, range information for the trials as a whole, or no information at all. They then found themselves assigned to one of these latter two conditions, thus being relatively advantaged or disadvantaged compared to some others in the group. This factor influenced participants' preferences for a partner. Those who had been led to believe that they were somewhat advantaged informationally found the "dissimilar agreer" more attractive than those who had been disadvantaged, presumably because this partner satisfied a desire for validation. In contrast, those with no information preferred the "similar disagreer" to a greater extent than those who had been provided with partial information, presumably because this partner was likely to possess useful information about the light's movement.

I certainly thought the findings were exciting and, hence, I expected the article to have more impact than it did. Why did it fall flat? A number of factors may have played a role. In hindsight, the surge of interest in social comparison processes appears to have been brief, already having passed its peak by the time the article was published. Moreover, I now recognize how unusual it is for any solitary article to produce a substantial impact. Typically, any well-cited article generates attention because of its featured role in a broader program of research.

Significant advances tend be achieved by research programs, not by one-shot articles. And, therein lay those counterfactual musings. What if I had pursued that line of research? At the time, I was heavily engaged in a research program on attitude-behavior consistency and the conditions, especially the attitudinal qualities, that promoted such consistency. Indeed, this work pre-dated my interest in social comparison processes, and I had a good sense of the future direction it needed to take. It was one of those lines of work for which the next study to be done, the next question to be asked, the next hypothesis to be tested, consistently seemed to emerge naturally from the previous findings. That was one of the reasons I found it so engaging. In contrast, I seemed incapable of identifying the next step in the social comparison work.

Here the counterfactuals grow all the stronger for me. What if I had been smart enough to recognize the real promise of the dissertation research? With the benefit of hindsight, I now realize that I could not discern where to take the social comparison research precisely because I had categorized it as social comparison work. That seems to have blinded me to its true value. There was an aspect of the work far more fundamental than any questions regarding choice of comparison other, and that was the very distinction between construction and validation. These are not just "motives for social comparison," as I had entitled the dissertation and the *JPSP* article, but much more general motivations relevant to judgmental processes. Construction and validation are central to issues of information acquisition, hypothesis generation, hypothesis testing, the conclusion that one has arrived at a sufficiently valid judgment, and the confidence one places in that judgment. At its heart, the construction-validation distinction highlights the importance of the point in a judgmental sequence at which individuals feel that they have enough information about a question to begin to entertain specific answers, i.e., the point at which they are sufficiently satisfied with the information they have acquired to begin to test hypotheses. It

is at this point that the focus of information acquisition shifts from the object to the judgment of the object. Moreover, there is a dynamic interplay between the two motivational goals. Satisfactory construction promotes the desire for validation, and failures to validate are likely to prompt a return to a construction state and a search for additional relevant information. Given all that the field has come to know about the confirmation biases that often accompany hypothesis-testing, the shift from construction to validation also represents the onset of a less open-minded perspective regarding the issue.

Over the years, as I witnessed the advances in our knowledge regarding such matters as hypothesis-testing, meta-cognition, confidence, and openness to new information, I frequently thought of the distinction between construction and validation. As a result, I now see the conceptual framework as much more central than I had envisioned earlier, and certainly as extending far beyond the consideration of social comparison processes. So, if any lesson were to be gleaned from what I personally wish to regard as a "hidden gem," it might be the value of focusing on *the fundamental*, i.e., the aspect of the conceptual reasoning with the broadest and most far-reaching implications. What may be most likely to offer a significant, long-term contribution and, hence, what may most merit the investment of time and effort, is the question, the level of analysis, or the finding that is the more fundamental. That consideration should carry substantial weight when making decisions about the potential directions a research program might take. Always look toward the more fundamental.

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