

## The Spawning of More "Little Ideas"

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*A Behavioral Theory of the Firm* (Cyert & March, 1963) has had such a significant influence because of the quality of the ideas embedded in the theory. Behavioral theory has a number of pieces and components; it's not a grand theory but instead a collection of "little ideas." There are so many wonderful, powerful ideas: bounded rationality, search that is local, biased and problem-based, satisficing, performance relative to aspirations, slack, sequential attention, organizational coalitions, decisions based on rules and routines. Every time I reread *A Behavioral Theory of the Firm* for a doctoral seminar, I am energized and excited by the number of ideas and the still unrealized potential. Each idea, and the cumulative impact of these ideas, leads us to think about the world in a fundamentally different way.

Each of the "little ideas" has spawned its own stream of research: organizational learning, performance feedback, theories of attention and power. When I was program chair for the Organization and Management Theory division in 2011, 20% of all submissions were in one of these areas. These are a set of ideas with a broad and enduring influence on our field, and they are embedded in so many of our theories. Finally, these ideas have had such a deep influence on the field because of the range of methodological options. In behavioral theory, you see scholars doing simulations, qualitative, and quantitative work. I do not think there are very many theories where you see that range of empirical techniques, and it deepens our understanding and broadens the reach of the ideas.

As for unresolved questions in behavioral theory, there are many. The fundamental critique of economics—that it is not enough to be true but not valid because as scholars we need to understand how things work in organizations—suggests a need for a deep understanding that we are still developing. In my opinion, there are three areas where I see the need for more research. First, we need more work on coalitions and how, when, and why coalitions change in organizations. This will allow us to better understand how decisions get made, as well as to connect decision making over time in organizations. Willie Ocasio, John Joseph, and Vibha Gaba are all doing exciting work in this space.

Second, in the performance feedback and learning areas, we need a deeper understanding of how organizational decision makers pay attention to and learn from others. A good deal of work focuses on how organizations learn from their own experience, and certainly there was an explosion of network and diffusion research that looks at how experiences of others are imitated and learned from. Several of the other participants in this dialogue—Peter Madsen, Vinit Desai, and David Maslach—are taking this research to the next level. But in the performance feedback arena, there is much more to be known about how reference groups are formed, how social aspirations are chosen, and the social comparison processes used by organizational decision makers. Both the beauty and the problem of the theory is that there is so much in there, so many "little ideas" that need further development. Some of the early interpretations of the work have become part of our taken-for-granted assumptions and part of our "truth" but those assumptions need to be verified. My current favorite example is the idea that organizational decision makers aspire to do better than the average of their reference group. This was not in Cyert and March (1963), but it was how social performance

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feedback was measured for 20 years. It is only recently that scholars have begun to reexamine that operationalization and suggest that, for many firms, striving for top performance is a more relevant aspiration. For example, Tom Moliterno (with Nikolaus Beck and I) and Zur Shapira (with Elizabeth Boyle) have been tackling these questions.

Finally, we need to better understand how novelty arises from mistakes and mutations. I very much agree with Jim on this. This will help us better understand the irrationality behind rationality and the "technology of foolishness." Jerker Denrell and Chengwei Liu are doing some great work in this spirit. Perhaps because I am a Californian, the "California Jim" who spent time at Irvine and Stanford most inspires my imagination. For example, the idea that Don Quixote is a text to discuss leadership (as it was in his fabulous Organizational Leadership class), and the implications of that, is not fully explored in our scholarship. That is probably because it does not fit well with our current focus on strategic management and improving performance, but organizational research should not lose sight of this legacy.