Celebrating Organization Theory

The Editors

At the *Journal of Management Studies* – just like in most other top-tier scholarly journals – when assessing a manuscript, editors and reviewers place considerable emphasis on theoretical relevance and contribution (Clark et al., 2006; Whetten, 1989). What theoretical debate do the authors intend to contribute to? What theoretically-derived questions drove the investigation? How do the findings extend current theories? Providing a convincing answer to these questions will make a difference between the acceptance or rejection of the manuscript.

Establishing the theoretical relevance of published studies is important to maintain some coherence in an otherwise fragmented field of research, where multiple, at times, conflicting perspectives coexist (Corbett et al., 2014; March, 2007). In recent years, however, management scholars have started to interrogate themselves on whether this emphasis on theory may be unintentionally stifling the emergence of new ideas, and curbing our capacity to address issues of practical relevance (Davis, 2010; Hambrick, 2007).

‘Where are the new theories of organization?’ ask Suddaby et al. (2011) in a recent special issue of the *Academy of Management Review*. Is current emphasis on theory-driven research really acting as a straitjacket, encouraging incremental investigation, at the expense of the development of new and original perspectives? Is this emphasis artificially restricting the scope of our studies, and slowing down, compared to other disciplines, our capacity to address issues of practical relevance (Davis, 2010; Hambrick, 2007).

In their contribution, Michael Lounsbury and Christine Beckman ‘celebrate’ the vibrancy of organization theory (Lounsbury and Beckman, 2014). The field of management studies, they claim, is much more geographically interconnected than it used to be, and academic conversations are now carried out in a truly global community, spanning...
across the boundaries of the main professional associations. Lounsbury and Beckman reply to the criticism of those who observe that management research is still largely conducted within the framework of a ‘handful of paradigms’ (Davis, 2010, p. 693) – for example, institutional theory, population ecology, and behavioural theory – developed between the mid 1960s and the late 1970s (Davis, 2010), by highlighting exciting new developments that have considerably advanced these perspectives.

Rising interest in institutional logics (Thornton et al., 2012), social categories (Hsu et al., 2010), strategy-as-practice (Vaara and Whittington, 2012), etc. – they claim – does not simply reflect a reorientation of scholars’ attention to less explored areas. These new lines of inquiry – Lounsbury and Beckman observe – have encouraged a reconsideration of the fundamental conceptual premises of their respective theoretical perspectives, and opened up opportunities for new waves of empirical work that have considerably increased our understanding of organizational phenomena.

Importantly, they argue, these perspectives are also increasingly acknowledging each other’s relevance, as scholars are increasingly exploring opportunities to fruitfully combine different perspectives, rather than pitting them one against the other and testing their relative explanatory power.

Gerry Davis (2014) is less optimistic. Is the cumulative volume of articles produced by a field of research – he asks – the best way to define theoretical progress? Developments in information and communication technology have now made available massive databases about thousands of organizations around the world. Yet, Davis laments, organization theories do not seem to have become more accurate nor more general. Nor do they seem to have become better at addressing important questions about the world.

While academic conversation seems thriving, Davis observes, it is less clear whether this conversation is really improving our capacity to solve important problems in society. Are theory-driven questions – asks Davis – really questions worth answering? Or is there a risk that excessive emphasis on theoretical relevance leads to self-referential research aimed at improving our way to ‘talk about things’, rather than learning ‘something new about the world’? Shouldn’t organization theorists use their unique conceptual and methodological skills to tackle ‘epochal challenges in social organizations’? Shouldn’t our journals find room for – if not encourage – research that is problem-driven, and not only theory-driven?

Both points are well taken. It is hard to deny the important theoretical developments occurring in the dominant paradigms in organization and management theory. While milestone articles from the late 1970s keep being cited by most recent research, it is also undeniable that the conceptual apparati we use to interpret organizational and social phenomena have become more sophisticated and fine-grained. However, if we look at our ‘JMS classics’ – articles that are still today recognized as seminal and sparking new conversations about competitive communities (Porac et al., 1989), knowledge-intensive (Starbuck, 1992) or high-reliability organizations (Weick, 1988) – one could argue that the path-breaking nature of these articles might be tied to their capacity to distance themselves from prevailing assumptions and conversations, and to produce rich, novel, and insightful conceptualizations of important organizational phenomena that had been largely overlooked by previous research. We believe that this Point–Counterpoint is an
important intervention, which will stimulate further debate on future directions for theory and research in management studies.

REFERENCES


