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Title: The buzzing, blooming, (potentially) confusing field of theory development in entrepreneurship research.

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This collection of essays is organized around the topic of theorizing in the field of entrepreneurship. It is motivated by authors with an interest in seeing entrepreneurship scholarship move beyond explaining phenomena or research contexts to considering the unique contributions that a theory of entrepreneurship might make. As editors, we were keen to take this interest forward at Strategic Organization. We see discussions over conceptual boundaries, and how the existing state of knowledge constrains or enables the development of those conceptual boundaries, as key to the development and furthering the research agendas of maturing research fields (e.g., Cunha & Putnam, 2019; Jarzabkowski, Seidl, & Balgun, 2022; Schad, Lewis & Smith, 2019).

The initial stream of papers was motivated by some strong claims in an essay by Richard Arend (2023), about the failure to generate field-specific theorizing in entrepreneurship. Arend makes a critique of other studies of entrepreneurship as his point of departure, on the basis that they apply theory from other areas to theorize. He calls for a unique-to-the-field theory of entrepreneurship, based on identifying and labelling a common ground for entrepreneurship studies that will render it malleable to consistency, replication studies, and theoretical consensus.

Three other papers in the collection are, at least partially, responses to these claims of a failure in field-specific theorizing, each advancing other ways to consider theory development in the field of entrepreneurship. Their arguments are varied. Wood and Fisher (2023) disagree that drawing from other theoretical fields is detrimental to the development of entrepreneurship theory. Drawing from the philosophy of science they call for a collectivist-evolutionary approach, building on frameworks from different areas, alongside an entrepreneurship focus, to drive the theory development process. Arguing that the search for a unique theory of entrepreneurship is “a folly”, Wry, York and Waldron (2023) express strong views about the purpose of entrepreneurship research. For these authors, the knowledge created should be evaluated by its usefulness for entrepreneurs themselves, for other stakeholders, and for society as a whole. In following this purpose, scholars will also lay the foundations for a more unique or identifiable theory of entrepreneurship. Acknowledging the significant amount of work already done, Roundy and Lyons (2023) suggest that the field has benefited from an entrepreneurial ecosystem view but has paid less attention to the micro-foundations of entrepreneurship. They advance such a micro-foundations agenda, focused upon the entrepreneurs that interact with these ecosystems.

While the other two essays do not directly respond to the Arend piece, they nonetheless, are part of this wider agenda for entrepreneurship theory development. Keyhani (2023) asserts the notion of strategic entrepreneurship. He proposes that the logic of strategy scholarship, with its focus on competition and performance, is essential to give purpose and direction to entrepreneurship studies, even as the dynamism, frame-breaking, and value capture that is the focus of entrepreneurship studies can reinvigorate strategy scholarship. Felin and Foss (2023), in turn, present arguments about firm specific theories being the source of a different form of (corporate) enterprising: the development of ecosystems. They argue that this

literature is currently short of micro-foundations which explain why and how firms would develop capabilities and strategic directions, and that firm-specific theories could provide such a micro-foundation. Interestingly – and hence the inclusion in this issue – as Felin and Foss note, similar arguments could be drawn on to explain the evolution of *any* enterprising endeavor more broadly (Camuffo et al., 2020; Felin et al., 2020; Felin and Zenger 2017)

We applaud this diversity of views, noting that the development of a field is often beset by precisely these types of competing claims. Indeed, we are reminded of the article, based on the late, great Andy Van de Ven's (1999) OMT Distinguished Scholar address. Andy lauded the buzzing, blooming and confusing world of organization and management theory, in which paradigm wars abounded, and different and sometimes opposing efforts to claim theoretical territory were key to driving theory development. We are delighted to see some of this buzzing, blooming and confusing world of entrepreneurship theory development play out in this collection of essays. We hope that it will motivate further empirical and conceptual research in the field and that some of this will play out on the pages of *Strategic Organization*.

As a final thought – and with no intent to weigh in on any side of the debate – we want to offer some considerations of what we believe could be a measure for good theorizing in entrepreneurship, above and beyond the many important and accurate arguments brought forward in the essays that follow, as well as earlier musings on this topic (e.g., Shepherd 2015). To do so, we follow Lakatos (1970) to suggest his logic of a progressive problem-shift as a useful evaluation logic. As a logic of predictions beyond currently existing evidence, a progressive problem-shift implies the “degree to which the series of theories [comprising a research program] leads us to the discovery of novel facts” (p. 118).

In our view, the logic of a progressive problem-shift offers several important insights. First, one may suggest that entrepreneurship need not necessarily be seen as *one* theory, but as a series of research programs. In turn, as long as additional studies in any research program still produce additional insights that lead to increasing explanatory power, such work should be supported, and a field should continuously remain open to such work.

Second, we believe that Lakatos's work also highlights how it may be useful to find some agreement on what is the hard core of entrepreneurship research – commonly held beliefs, assumptions, and predictions – and what is a protective belt of auxiliary hypotheses.

Otherwise, a research program is hard to specify, and progress difficult, if not impossible, to assess. However, that need not imply we need a hard core for *the* theory of entrepreneurship – just as most scholars of strategic entrepreneurship would likely agree that specifying *the* theory of the firm would be a daunting endeavor.

We look forward to seeing these continuous discussions, extensions, and possibly rejections of such theories, and we hope that – as in any field of science – their continued buzzing and blooming will eventually result in decreasing confusion.

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