



Making the marriage work: the benefits of strategy's takeover of entrepreneurship for strategic organization

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It appears to us that strategy is succeeding in its takeover of the academic field of entrepreneurship. It is doing this by acquiring entrepreneurship's most important assets – faculty members. Of the entrepreneurship division's 2035 members, 1000 (49.1 percent) are also members of the Business Policy and Strategy (BPS) division. Further, a recent glance at the Academy of Management postings for entrepreneurship positions found that of the 157 job postings, about two-thirds also listed strategy as a teaching preference. Of these, strategy was listed before entrepreneurship 65 percent of the time. To us, this suggests that schools often believe the best way to deal with student (or generous alumni) demands for entrepreneurship courses is to hire someone trained in strategy to teach them. A recent bibliometric study of entrepreneurship research supports our observations, noting that 'most entrepreneurship scholars have received their training in strategy groups within existing management departments or in stand-alone strategic management departments' (Gartner et al., 2006: 324). Finally, it appears that there is burgeoning interest in the overlap between strategy and entrepreneurship research. Following on the heels of a popular edited volume titled *Strategic Entrepreneurship: Creating a New Mindset* (Hitt et al., 2002), the Strategic Management Society announced a new journal, titled *Strategic Entrepreneurship*, and scheduled an entrepreneurship research conference for May 2007.

Perhaps we should rail against this takeover. Others have expressed such outrage and argued for establishing entrepreneurship as a research domain that is distinct from other social sciences – especially strategy (e.g. Shane and Venkataraman, 2000; Venkataraman, 1997). But we are less worried by such concerns, and doubtful anyway that such hand wringing, however theoretically nuanced, is likely to have much effect. There is little evidence that in the face of competing paradigmatic assumptions – which include beliefs about appropriate domains – evangelical definitional missives about appropriate research topics (Donaldson, 1995; Pfeffer, 1993; van der Werf and Brush, 1989; Weick, 1999) engender a subsequent increase in focus. More importantly, we are not persuaded

by the perspective that social sciences or applied academic business fields benefit from non-overlapping domains.

We recognize that there is the potential for negative outcomes in any relationship that is not managed well, and we discuss some of these issues at the end of our essay. However, the purpose of the essays in this anniversary issue is to look towards the future of strategic organization scholarship. Therefore, our primary focus in this essay is exploring some of the ways that the study of strategic organization can benefit from the incorporation of entrepreneurship researchers and research. In our view, anything that elicits recognition of entrepreneurship's distinctive contribution within strategic organization is likely to sustain and improve the impact of entrepreneurship research, while also enriching strategic organization. The approach we take in this essay is to ground our discussion in issues and questions for strategic organization raised in previous 'So!apbox' essays. We offer some ideas about how entrepreneurship research might address those and other issues. We organize our discussion in two main parts: first, we discuss potential contributions of entrepreneurship to the theoretical development of strategic organization, and second, we discuss how entrepreneurship research may contribute to improving the practical usefulness of strategic organization.

Opportunities for theoretical integration

The domain of entrepreneurship research offers new theoretical opportunities for research in strategic organization. This is because whereas the focus of strategy research has primarily been on the performance of large, established firms, entrepreneurship has generally focused on start-up and growth processes. Indeed, entrepreneurship scholars have recently even begun to add a new, 'prenatal' stage to the organizational life-cycle by conducting research on firm 'nascence', that is, the processes and patterns that *precede* firm birth. Thus, perhaps unsurprisingly, the definition of entrepreneurship's research domain that appears to be the most useful for enhancing research at the intersection of strategy and organization involves conceiving the domain as research concerned with the creation of new organizations (Carter et al., 1996; Katz and Gartner, 1988).

There are at least two obvious advantages to developing theory within this domain. First, start-ups are plentiful, and most have never been studied by anyone. Although the time entrepreneurs spend starting and running their companies is substantial, many researchers nonetheless find them willing and eager to talk about their great passion: the organizations they have conceived and nursed into being. As one founding manager reported to us while getting comfortable on a couch in his office, 'this really is my weekly therapy session'. Second, start-ups are a 'relatively compact system of activity' (Baker et al., 2003: 256). Since they are smaller and less complex than larger, more established firms, it is easier to identify and describe causal patterns and meanings. Thus many – though certainly not all – of the theoretically important processes described (but

frequently 'black boxed') in strategy and organization theory research can actually be observed as they play out in young organizations.

A recurring issue in strategic organization reflected in previous 'Solapbox' essays is the relative value of focusing on individual-level vs organizational-level processes. Hambrick (2004: 94) argued in his 'Solapbox' essay that 'we need to reintroduce the human element to our research', because 'human beings have been largely discarded from a great part of strategic management research'. And in their essay, Felin and Foss (2005) express frustration that much of the strategic organization literature assumes the taken-for-granted status of organizations and pays too little attention to the individuals that constitute organizations and the potentially consequential differences among them.¹ They also point out that many attempts at 'multi-level' studies have been unsatisfactory. As a reaction to what they see as the organization-centric presumptions and 'methodological collectivism' of current research, they call for a strong form of methodological individualism (Heath, 2001; Popper, 1945; Watkins, 1957), one that assumes the primacy of individuals in understanding strategic organization.

We appreciate Felin and Foss's frustration and their attempt to provide some corrective for what they see as the one-sided dominance of methodological collectivism in research in strategic organization. But what they express as epistemological and ontological commitments, we see as empirical questions of fundamental importance.² The question is not whether individuals or organizations have primacy, but rather what are the processes and conditions that allow, or require, one or the other to take on more or less primacy.

The creation and development of new organizations offers an extraordinary laboratory to study the processes through which individual-level elements give rise to organizations that may subsequently either extend or effectively annihilate the strategic consequences of individual differences. To us, the question of individual vs organizational primacy is first a question of the processes through which the beliefs, goals and strategies of would-be entrepreneurs are expressed and furthered by the organizations they build. As Hannan and Freeman (1989: 81) noted, early on the new firm is a malleable extension of the founder's personality. Research has backed the contention that origins matter, and shown that differences among founders' cognitive models are surprisingly consequential in shaping the firm's organizational structure and employee interactions long after founding (Burton et al., 1999; Hannan et al., 1996). But what are the processes that maintain or sever this close tie between personality and organization? What leads an organization to continue reflecting its founder's goals, when and why does it adopt others' goals, and how and when does it effectively take on a life of its own such that strategies are driven by the organization's routines, capabilities and resources in combination with institutionalized expectations about what an organization should be and do, rather than by any particular set of individuals? Perhaps equally important, how and through what processes do new firms remain open to strategic change based on the individual goals and attributes of employees who join well after founding?

Entrepreneurship researchers have a long track record of frustration in identifying trait-like individual differences that are consequential for *any* part of the entrepreneurship process (Aldrich and Wiedenmayer, 1993; Gartner, 1989). Recent research shows a little more promise in this regard (Baron, 1998; Baum and Locke, 2004) but the difficulty of finding the individual differences that matter much, even in the malleable circumstances of nascence and start-up, should give pause to anyone expecting to detect how individual differences influence strategy and practices in large, established firms. Indeed, not only does bureaucracy constrain individual differences among employees, increasing evidence suggests that employment in a bureaucracy may suppress later individual entrepreneurial behavior, perhaps through individual cognitive changes (Dobrev and Barnett, 2005). Research at the intersection of entrepreneurship and strategic organization can help to explain how individual differences remain important or disappear, and provide insights into making informed choices and managing the process.

Martin Ruef's (2003) 'So!apbox' essay – which promotes a sociological view of strategic organization – may seem an odd place to find common ground with the methodological individualism supported by Felin and Foss (2005). But he, like they, is talking about the value of looking beyond presumptions of goal homogeneity. Ruef argues that strategic management's traditional focus takes for granted the dominance of what Weber called formal means–end rationality. In his account, strategic organization should embrace a broader framework in which strategy is not a priori defined narrowly as instrumental action oriented toward profitability (see Hoffman's [2004] 'So!apbox' essay for a similar point). Ruef notes that a wide variety of both goals and values might shape strategic organization, and that understanding when and how this occurs are important topics for future work.

We agree, and suggest that research in entrepreneurship has consistently provided empirical support for the continued importance of values and goals besides maximizing short-term profitability in shaping entrepreneurial activity. For example, studies across a number of nations have consistently shown that values such as the desires for autonomy, to express creativity for its own sake, to pursue innovation and to be one's own boss typically dominate financial goals in motivating most entrepreneurship (Ageev et al., 1995; Litvak and Maule, 1976). One study of start-up motivations across 11 countries (Scheinberg and MacMillan, 1988) uncovered 38 motives for entrepreneurship that reduced to six distinct dimensions, only one of which was related to financial results or wealth. Other research has shown that entrepreneurs may continue to operate their firms despite a consistent pattern of substandard financial returns, presumably because continuing to run the firm has strong personal strategic value for them (Gimeno et al., 1997), and research on family businesses has suggested that altruism and other characteristics of close interpersonal ties may be important elements of strategic organization in some family firms (Miller and Breton-Miller, 2003; Schultze et al., 2003).

This research suggests that entrepreneurs' actions are oriented through means–ends rationality (Weber, 1947) to a variety of valued outcomes via expression of a wide spectrum of individual and social values. More subtly, and perhaps importantly, it provides evidence that entrepreneurs sometimes behave as they do because it is a direct expression of their personal values, not because of the financial incentives involved. Indeed, even in the hothouse world of venture capital (VC) backed start-ups, a focus on profits may not always form the strategic core. As one serial technology entrepreneur reported to us (after several drinks), 'I want to do what I want to do, and making enough money to keep the VCs happy is just a way to let me keep doing it.'

Unfortunately, a great deal of current research and teaching in strategy neglects this richer depiction of organizations' leaders as 'homo multifacetus' in favor of the cognitively simpler homo economicus, thereby constraining our understanding of how individuals influence organizations' strategies, structures and growth. Perhaps Hoffman's (2004: 216) point is correct, and 'cultural biases in business schools encourage organizational research away from issues that are critical of business interests or run contrary to the accepted orthodoxy that profit maximization is the only true end goal of organizations' (see also Hinings and Greenwood, 2002). Despite the findings of the studies cited earlier, we are concerned that, perhaps partly as a consequence of strategy's takeover of entrepreneurship, the increasing valorization of the venture capital model of entrepreneurship and corresponding reliance on the rationalized myths of job creation and economic development³ have nonetheless begun to artificially restrict and damage the unique value of entrepreneurship research in addressing this problem.

The basic moral premise of much of strategic management is indeed the promise that strategy makes organizations more competitive and in so doing improves overall social welfare (Smith, 2003 [1776]). Supporting goals shared by most entrepreneurship and strategy research include job creation, wealth accumulation and economic dynamism and efficiency. However, strategic organization can and should embrace a broader picture of social betterment. For example, recent developments in so-called 'social entrepreneurship', which includes both 'for profit' and 'not-for-profit' organizations, begin to throw into relief how strategic organization can help to create a wide variety of valued outcomes seldom imagined in narrower views of strategy. Thus, while we think that entrepreneurship research can lead the way in understanding the role of values and varied goals in strategic organization, it has not, to date, lived up to this promise.

In addition to providing new avenues for research on strategic organization at the individual and firm levels, the integration of entrepreneurship into strategy also offers promise for new theorizing at the interorganizational level. In particular, it allows for exploration of strategic issues that have been hampered by the problem of left censoring, and opens up research on resource-based competition to consideration of how firms that are resource constrained compete and survive.

One of the standard critiques of much empirical strategy research attempting to study process issues is it arbitrarily, or for purposes of data convenience, plucks a sample of established firms at a particular point in time and makes arguments assuming that nothing occurring prior to the period of study influences the dynamics under consideration. While convenient for constructing data sets and widely tolerated for the sake of allowing scholars to get their own, as well as others' studies published, it is nonetheless a highly questionable assumption. Because entrepreneurship research focuses specifically on the genesis of organizations (Baker et al., 2003; Gartner, 2004; Katz and Gartner, 1988) and their introduction to public markets (Certo et al., 2001; Gulati and Higgins, 2003; Pollock and Rindova, 2003; Stuart et al., 1999), it avoids the left censoring problems endemic to much strategy research. There is an actual time zero. Thus, entrepreneurship scholars working within the domain of strategic organization have been able to explore topics such as the processes and issues surrounding the separation of ownership and control (Beatty and Zajac, 1994; Boeker and Karichalil, 2002; Wasserman, 2003), the role of market intermediaries in organizational legitimation (Pollock and Rindova, 2003) and reputation building (Rindova et al., 2007), the effects of embeddedness on IPO (initial public offering) firm valuation (Pollock, 2004) and survival (Fischer and Pollock, 2004), the effects of formal structure on new firm performance (Sine et al., 2006) and the effects of environmental and institutional factors on the dynamics of industry creation (Sine and David, 2003; Sine et al., 2005).

Further, perhaps one of the hottest topics in strategy research over last 15 years has been the role of resource control and organizational activities in establishing competitive advantage (Barney, 1991; Eisenhardt and Martin, 2000; Mahoney and Pandian, 1992; Peteraf, 1993; Prahalad and Bettis, 1986; Prahalad and Hamel, 1990). In particular, the resource-based view (Barney, 1991; Mahoney and Pandian, 1992; Penrose, 1959) and the concept of core competence (Prahalad and Hamel, 1990) have received excessive amounts of attention. These perspectives focus on how firms that control valuable, unique and inimitable resources, and/or engage in sets of activities and routines that are, at best, difficult to identify and impossible to imitate, can lead to competitive advantage and superior profits.

However, these perspectives are overly narrow and somewhat elitist in that they describe the strategic activities of just a tiny fraction of all organizations, since by definition only a small number of firms can possess these assets. Thus, these theories actually have very little to say about how most organizations compete and survive. How, for example, might start-up companies that control few resources benefit from these arguments as they struggle to establish toe-holds in their respective markets? Recent research on entrepreneurial bricolage, which focuses on how organizations compete and make do with the resources at hand, has begun to explore this fertile and largely undeveloped territory (Baker, 2007; Baker et al., 2003). This research challenges the open systems presumptions that underlie much of the work in strategy derived from analysis of industry structure, and also begins to explain some of the processes of resource combination that are

presumed to underlie resource-based competitive advantage (Baker and Nelson, 2005; Garud and Karnoe, 2003). Thus, entrepreneurship research speaks to the heart of issues of central importance to strategic organization, and offers new approaches and insights that can challenge and invigorate these literatures.

Opportunities for a practice perspective

A second broad theme recurring in the 'Solapbox' essays concerns the appropriate role of practicality and usefulness in shaping and evaluating research on strategic organization. At the most general level, the gist of the argument is that strategic organization is simultaneously a descriptive, theoretical and applied field, and that at least some researchers would like to have their theoretical work be of direct practical relevance. Whittington (2003: 122), for example, notes:

I have been teaching strategy and organization for about 15 years, but I know very little about how to *do* strategizing and organizing. When called in some small way to help with others' strategy and organization-making, I have hardly anything to say about how they should carry out the actual work of producing new plans and designing new structures.

Whittington's comments lucidly express the limitations of our work that frustrate many scholars.

While Whittington (2003) worries over the practical aspects of strategy scholarship, Hoffman (2004) holds strategy out as an exemplar of practical usefulness and impact – at least relative to organization theory research. Hoffman argues that because of its grounding in the social science and humanities disciplines, organization theory research strongly prefers questions of theoretical relevance over questions of practical importance and worries very little about integrating the two.⁴

The good news here is that entrepreneurship is perhaps the most applied of the management fields, and strategic organization, through its acquisition of entrepreneurship, has a wonderful opportunity to gain practical relevance and value. The bad news is that entrepreneurship itself has not got the balance between the theory and practice perspectives quite right. In contrast to Hoffman's (2004) characterization of organization theory as too heavily weighted toward the theoretical, some have argued that entrepreneurship as a field may be too heavily weighted toward practice (Brush et al., 2003).

This slant may be due, in part, to how many universities have handled the teaching demands of the subject. Teaching entrepreneurship requires a high degree of practical applicability, and many schools have responded by relying heavily on successful entrepreneurs without a background in or commitment to research to teach their courses. Such 'professors of practice' may be very popular and effective teachers, but their overuse relieves pressure to hire and support research faculty in entrepreneurship. More importantly, heavy reliance on non-research faculty promotes the separation of teaching and research by filling

classrooms with people who have limited knowledge of and virtually no commitment to expanding the body of entrepreneurship research. In addition, in our observation, some researchers, especially those experienced in or otherwise drawn to the *phenomenon* of entrepreneurship, come to focus on the lack of obvious applied value in much of the entrepreneurship research in more highly rated journals, and lose respect for and motivation to do such research. Indeed, someone once remarked to us that the kind of research published in 'A' journals has no economic value, and claimed that he now feels 'an ethical and fiduciary duty' to aim his research at other outlets. Unsurprisingly, these practices create a divide between research and practice that is reinforced through a lack of respect for one another's contributions by those on either side.

But now we may have painted too stark a picture. Recent years have brought to entrepreneurship an increasing number of scholars with deep interests in both theory and practice, many of whom were trained to do strategy research. Some, but not all of these researchers have prior experience working in or with entrepreneurial ventures. Because the thin archival record deposited by many start-ups requires entrepreneurship researchers to 'get their hands dirty', many entrepreneurship researchers – even those without relevant prior experience – may gain an understanding of practical issues through direct research involvement in new ventures. We think this emergent model for doing theoretically interesting and practically useful entrepreneurship research has great promise that strategic organization should be careful not to squander. But how can scholars of strategic organization avoid wasting this opportunity? We suggest that if strategic organization wants a model that balances theory and practice, it could learn something from the medical model of research, education and practice.⁵ We focus here on three elements: boundary creation and maintenance, practitioner-researchers and usefulness-driven research questions.

First, the medical profession has built and maintained a set of education-based professional standards regarding who is and who is not part of the profession and qualified to practice (Abbott, 1988). The same sorts of state sanctioning and tight professional control are not available to faculty in strategic organization, but we do wield non-trivial influence. As entrepreneurship research and teaching become part of the family of strategic organization, it is incumbent on us to resist the profligate use of non-research oriented faculty. It is very difficult to maintain institutional respect for the need for teaching to be based on scholarly underpinnings, or the complementary professional recognition of the need to develop practical underpinnings for scholarly research, if scholarship and teaching occupy opposing sides of a chasm.

Second, much valuable practical medical research is conducted by practicing physicians in teaching hospitals. The entrepreneurship centers and outreach activities of various kinds associated with many business schools provide a similar research opportunity. Unfortunately, these centers are too often seen as providing purely 'service' activities designed to create support from and connection to the local business community. Indeed, in conversations with colleagues, we

have been surprised by comments indicating a sense of fatalism regarding the use of entrepreneurial 'outreach' as a foundation for theoretically interesting and practically important research. Once again, this is a mistake that can easily be overcome as entrepreneurship joins with the more established and potentially protective assets of strategic organization. Among a research-oriented faculty, entrepreneurship outreach activities should be viewed and managed primarily as a research venue, with service to the community a corollary to this work. In our observation, this simple and obvious opportunity has been widely ignored and seldom exploited. The willingness and even eagerness of entrepreneurs to engage with entrepreneurship centers, and to implement interventions crafted by entrepreneurship faculty and students, represents a research opportunity largely without equivalent in other management fields. Entrepreneurship faculty are routinely given access to start-ups and to these firms' most private information and plans, and they are also often able to initiate specific and substantial interventions in firm activities. Thus, start-ups provide a useful laboratory for studying many of the research questions central to strategy and organization.

An important caveat must be provided, though. Unlike junior faculty in related disciplines, junior entrepreneurship faculty are frequently expected to participate heavily in the growth and management of these entrepreneurship centers, often without compensating reductions in teaching responsibilities, and usually to the detriment of their research and prospects for tenure (Brush et al., 2003). This is due, in part, to the lack of senior entrepreneurship faculty in many departments (Finkle and Deeds, 2001). Thus, it is incumbent upon senior strategy scholars to take a more active role in the development and direction of these centers so that their scholarly mission can be fulfilled and junior faculty can grow from their associations with these centers, rather than become scarred by them.

Finally, we have been using the terms 'theoretically interesting' and 'practically useful' to describe what we see as reasonable goals for research supporting a practice perspective in strategic organization. To us, 'theoretically interesting' research is a somewhat different concept than the traditional scholarly notion of 'theory-driven' research. Research may be theoretically interesting even if it is not theory-driven. Medical research conducted in hospitals is aimed at addressing a huge variety of questions. Many of these may indeed be fairly described as 'theory-driven' in that they are based on more or less sophisticated hypotheses about how certain treatments will affect patient outcomes, or in the sense that they seek to contribute to our fundamental understanding of health or a disease state. As important, though, is the large amount of research driven by practical needs to find, develop, improve or decide between treatment options for patients. This research may be undertaken with little regard to how it might contribute to any fundamental or theoretical understanding of broader issues.

The analogy for strategic organization research (and applied social science research more generally) suggests reducing the privileged status of 'theory-driven' research while simultaneously legitimizing and accepting the value of

engaging in more 'puzzle-driven' research, of the kind that starts with the question, 'Why is it that ...?' (Davis and Marquis, 2005; Hambrick, 2005). In this view, theorists of strategic organization need to build theories that are capable of coming to terms with the meaning and findings of practice-driven research, and then be capable of explaining and making new predictions about such research. As Gartner (2006) put it recently, 'the primacy of facts should drive the search for theory that can make sense of those facts'. Complementary ideas are found in the 'activity-based' approach to strategy (Jarzabkowski, 2005; Whittington, 2003). In this way, theory-driven research becomes just one part of a better-integrated cycle of knowledge creation and application (Vermeulen, 2005). Indeed, such a scholarly grounding in 'what works', conceived in the broadest terms to embrace the myriad strategic purposes of organizations, may be the most likely antidote to paradigm proliferation.

The risks and rewards for entrepreneurship

Thus far, we have focused on the benefits of the union between strategy and entrepreneurship for strategic organization. We believe this takeover can yield benefits for entrepreneurship scholarship, as well. However, we also recognize the potential dark side for entrepreneurship research to this marriage. The remainder of our essay focuses on these issues.

Strategy brings to the altar a set of assets useful to entrepreneurship, including scholarly legitimacy and its accumulated experience with the process of gaining legitimacy as a new scholarly domain (Ghemawat, 1997; Hambrick, 2004), a large and growing cadre of well-trained scholars capable of doing rigorous and theoretically grounded entrepreneurship research, and the ability to protect junior scholars from egregious demands for practical engagement. Unfortunately the threat to entrepreneurship extends, in part, from these very assets.

Part of strategy's success and its rapid increase in institutional legitimacy has derived from its focus on a single set of outcomes related to organizational financial performance. While we have suggested that strategy research would benefit greatly from embracing a much broader domain of uses, functions and outcomes, it is easy to imagine the combination of inertia, socialization and personal preference among strategy researchers leading to an active defense of the field's current domain. If relative financial performance becomes the *sine qua non* of entrepreneurship research, then the acquisition has failed: strategy will have squandered most of its opportunity, and a primary contribution of entrepreneurship will have been squelched. This threat is real; strategy research has itself suffered from a relatively rapid narrowing of focus through the rising dominance of assumptions promulgated by research grounded in economics (Ghoshal, 2005). The life that entrepreneurship can bring through a broadened understanding of human goals and values pursued through organizing could easily be suffocated if the calloused heel of *homo economicus* is allowed to stand too heavily upon entrepreneurship's throat.

In addition, it is easy to imagine strategy researchers, given their struggles to move their own field away from its atheoretical origins in the war stories of business policy classes and the products of management consulting firms (Ghemawat, 1997), actively rejecting the usefulness of practical scholarly engagement with the community of entrepreneurial firms. Indeed, the dysfunctional division of labor between theoretical and practical research would only get worse, with entrepreneurship playing the subordinate, practical role. Again, strategy research has made it plain how often – especially when the primary assets are human – acquisitions destroy value. Based on this logic, we should be very, very worried for the future of entrepreneurship.

So, why do we worry less than some others about these threats? There are three primary reasons. First, entrepreneurship scholars are a diverse lot, organized into at least eight different partially competing groups, or clans (Gartner, 1990), with identities built around very different notions of what entrepreneurship is and how it should be studied. Both organization theory and recent military history suggest that the decentralized form of organization enacted by entrepreneurship researchers is difficult to fully suppress. Second, for a variety of reasons having to do with the field's history and struggles for legitimacy, many entrepreneurship scholars are not 'fully socialized' into the academy. If strategy fails to nurture what entrepreneurship offers, entrepreneurship researchers will create spinouts and a new wave of institutional change. Finally, as we have argued throughout this essay, encouraging and protecting entrepreneurship scholarship is in strategy's own self-interest; a motivation that strategy clearly understands well. As Ovid advised, 'If you would marry suitably, marry your equal.' We remain optimistic that entrepreneurship will create and sustain advantages from its marriage with strategy; we hope that strategic organization will do the same.

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Notes

- 1 We note that they seem to ignore the example of top management team research, which, while admittedly often using demographic proxies for individual characteristics, does attempt to explain some aspects of strategic organization in terms of specific, influential individuals.
- 2 Another way to express our position is to note that we are sympathetic to the epistemological claims of methodological individualism, whether reflecting the Weberian (1978) valorization of the subjective underpinning of social action or the more general idea that any social explanation benefits from 'micro underpinnings' in the form of explicated assumptions about individual actors (Watkins, 1957). However, along with many others (e.g. Barnard, 1938; Durkheim,

- 1938; Goldstein, 1958; Lukes, 1968) we reject most of the ontological commitments of methodological individualism, such as those that imply that societies, organizations and other collectivities don't actually exist in a way that allows us to study them usefully as social objects (Heath, 2001). This includes claims such as Felin and Foss's (2005: 446) approving adoption of Coff's (1999) argument that 'firms do not appropriate (or perhaps even create) value; only individuals do'.
- 3 We label the widespread beliefs about the relationship between entrepreneurship and the two outcomes of economic development and job growth 'rationalized myths' because they are frequently bandied about as unvarnished truths by proponents of entrepreneurship, despite a great deal of contingency and equivocation in the serious literature examining the underlying claims.
 - 4 The sense of relative positioning among fields strikes us as interesting in itself. For example, in his 'Solapbox' essay, Hambrick (2004) decries the splintering of the field of strategic management and fears that it will disintegrate and its various research agendas will be taken over by other fields, including sociology. At the same time, at a recent meeting of the American Sociological Association, one of us engaged in conversation with a very senior scholar who bemoaned the extreme factionalization and lack of coherence in contemporary sociology. And Shane and Venkataraman's (2000) essay about a distinctive domain for entrepreneurship feared mostly that entrepreneurship would be taken over by the very field that Hambrick fears is disintegrating.
 - 5 We are not saying that the entire medical research, teaching and practice model fits strategic organization or that we should try to adopt such a model wholesale.

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