At the Center of the Action: Innovation and Technology Strategy Research in the Small Business Setting

by Justin Tan, Eileen Fischer, Ron Mitchell, and Phillip Phan

In this essay, we offer perspectives on the future of small business research. These comments cover a range of issues unique to the future of small-business-focused research from "somewhat-broad" to "more-narrow," and address: (1) the problems and promise of better theory building, (2) the range of opportunities for theory-building research, (3) new vantage points for theory-building using the "social responsibility" of small business as a research lens, and (4) the future direction of research in technological entrepreneurship. We conclude with a summary of this "look to the future," and call for the innovative and provocative research that can keep contemporary small business management research at the center of the academic action.

In big business change is small
In small business change is big
(Welsh and White 1981)

Introduction

Contemporary small business research is the beneficiary of a dramatic change in theoretical perspective. When, in their 1981 *Harvard Business Review* article, Welsh and White popularized the notion that "a small business is not a little big business," it signaled a movement from the static view of small business as being suboptimal because of scale limitations, toward the dynamic view of small

Justin Tan is professor of management and Newmont Chair in Business Policy in the Schulich School of Business at York University.

Eileen Fischer is professor of marketing and Anne and Max Tanenbaum Chair of Entrepreneurship and Family Enterprise in the Schulich School of Business at York University.

Ronald K. Mitchell is professor of entrepreneurship and Jean Austin Bagley Chair in Management in the Jerry S. Rawls College of Business at Texas Tech University.

Phillip H. Phan is professor in The Johns Hopkins Carey Business School at The Johns Hopkins University.

Address correspondence to: Justin Tan, Schulich School of Business, York University, 4700 Keele Street, Toronto, Ontario CANADA M3J 1P3. E-mail: jtan@schulich.yorku.ca.

businesses being agents of change (Audretsch 2001, 1995). As we look toward the future of innovation and technology strategy research in the small business setting, we anticipate the maturing of this dynamic view, which casts small business as a main character at the "center of the action" in technology and innovation.

As chronicled in Table 1, the literature that marks the milestones along this path of change since 1981 spans a period from the early 1980s to the early 21st century.

We note several themes in this literature that provide the outlines, we think, of a future research agenda for scholars in this domain. For example, over the past 25 years the emphasis has shifted away from trying to distinguish a "small" business from an "entrepreneurial" business, and also away from the descriptive research necessary to articulate the importance of small businesses to society. Instead, emphasis has shifted toward the attributes and strategies that enable small businesses to grow, to contribute to economic value creation, and to flourish at the center of the innovation and technology-based calculus.

For example, we know that during the early stages of new business formation all ventures are small, but necessarily high-growth (Figure 1). Hence, mid-range theoretical frameworks are necessary to explain the differences between small and high-growth firms or more importantly the divergence in growth paths that can occur as small firms mature.

The Future of Small Business Research

With the earlier observations as our foundation, we offer the following four related essays that provide perspectives on the future of small business research in the area of innovation and technology strategies. These comments cover a range of issues unique to the future of small-business-focused research from "somewhat-broad" to "more-narrow," and address: (1) the problems and promise of better theory building, (2) the range of opportunities for theory-building research, (3) new vantage points for theory-building using the "social responsibility" of small business as a research lens, and (4) the future direction of research in technological entrepreneurship. We conclude with a summary of this "look to the future," and call for more innovative and provocative research that can keep contemporary small business management research at the center of scholarly attention.

The Problems and Promise of Better Theory Building

For at least the last 20 years, the field of small business research has played a unique role in the development of entrepreneurship research. Przeworski and Teune (1969) suggest that ". . . the criteria of generality . . . imply that the same theories must be evaluated in different systemic settings and that social science theories can gain confirmation only if theories formulated in terms of the common factors constitute the point of departure for comparative research (p. 22)." By testing theories in small business settings, which were originally developed in some other context, we have thereby advanced our knowledge about the external validity, or generalizability, of current theory. For example, in organizational research, managerial paradigms developed and tested among large firms may be assessed and refined and new theories developed, where they are exposed to comparative testing in the small-enterprise setting—which presents wide variation in organizational features.

Somewhat problematically, however, this approach has yielded many diverse positions on the transferability of management principles into small businesses because of the wide variations in the quality of research execution. For small

Table 1
Small Business Research Examples—A 25-Year Chronological Snapshot

Year	Research Description	Reference
1983	The authors . . . build a framework consisting of five stages through which small companies pass.	Churchill, N. C., and V. L. Lewis (1983). "The 5 Stages of Small Business Growth," *Harvard Business Review* 61(3), 30–39.
1984	. . . although there is an overlap between entrepreneurial firms and small business firms, they are different entities.	Carland, J. W., F. Hoy, W. R. Boulton, and J. A. C. Carland (1984). "Differentiating Entrepreneurs from Small Business Owners—A Conceptualization," *Academy of Management Review* 9(2), 354–359.
1988	. . . (This paper examines) . . . how well . . . theories of small business management meet the requirements of good theory.	Damboise, G., and M. Muldowney (1988). "Management Theory for Small Business—Attempts and Requirements," *Academy of Management Review* 13(2), 226–240.
1990	. . . (small businesses) do change. but not necessarily in any prescribed sequence . . . future research should be focused on developing theories which better describe the heterogeneity of the small business sector . . .	Birley, S., and P. Westhead (1990). "Growth and Performance Contrasts between Types of Small Firms," *Strategic Management Journal* 11(7), 535–557.
1994	. . . across a broad spectrum of European countries . . . a shift away from large firms and towards small business has taken place . . .	Schwalbach, J. (1994). "Small Business Dynamics in Europe," *Small Business Economics* 6(1), 21–25.
1994	The article provides an inventory of the strengths and weaknesses of small firms in a dynamic context.	Nooteboom, B. (1994). "Innovation and Diffusion in Small Firms—Theory and Evidence," *Small Business Economics* 6(5), 327–347.
1995	The use of new management and production technologies is essential for most small businesses if they are to improve their competitiveness.	Julien, P. A. (1995). "New Technologies and Technological Information in Small Businesses," *Journal of Business Venturing* 10(6), 459–475.

Table 1
Continued

Year	Research Description	Reference
1995	. . . entrepreneurial firms engage in more sophisticated planning than small firms overall . . . however, as perception of environmental uncertainty increases, strategic and operational planning decrease.	Matthews, C. H., and S. G. Scott (1995). "Uncertainty and Planning in Small and Entrepreneurial Firms—an Empirical-Assessment," *Journal of Small Business Management* 33(4), 34–52.
1996	Given that a firm has some sales of innovative products, the share of such products in a firm's total sales tends to be higher in smaller firms.	Brouwer, E., and A. Kleinknecht (1996). "Firm Size, Small Business Presence and Sales of Innovative Products: A Micro-Econometric Analysis," *Small Business Economics* 8(3), 189–201.
1996	This paper (suggests): (1) the effect of firm size on the causes and (2) the consequences of innovation or their focus on the role small firms play in reshaping the industrial landscape.	Thurik, A. R. (1996). "Innovation and Small Business—Introduction," *Small Business Economics* 8(3), 175–176.
1997	(This paper) examines the relationship between product innovation and growth in German, Irish and U.K. small firms. In each country the output of innovative small firms was found to grow significantly faster than that of non-innovators.	Roper, S. (1997). "Product Innovation and Small Business Growth: A Comparison of the Strategies of German, UK and Irish Companies," *Small Business Economics* 9(6), 523–537.
1998	This paper develops a structural model of the relationships between entrepreneurial characteristics, firms' strategic choices and performance. The results suggest a marked difference between the determinants of strategic initiatives related to management and control and those related to products, markets or managerial systems.	——— (1998). "Entrepreneurial Characteristics, Strategic Choice and Small Business Performance," *Small Business Economics* 11(1), 11–24.

Table 1
Continued

Year	Research Description	Reference
1999	The goal of this (paper) is to synthesize disparate strands of literature to link entrepreneurship to economic growth.	Wennekers, S., and R. Thurik (1999). "Linking Entrepreneurship and Economic Growth," *Small Business Economics* 13(1), 27–55.
2000	A panel analysis of 48 U.S. States for a ten-year period . . . revealed that states with higher proportions of very small business employment do indeed experience higher levels of productivity growth, and Gross State Product growth, while having less wage inflation and lower unemployment rates.	Robbins, D. K., L. J. Pantuosco, D. F. Parker, and B. K. Fuller (2000). "An Empirical Assessment of the Contribution of Small Business Employment to US State Economic Performance," *Small Business Economics* 15(4), 293–302.
2001	(We) considered ways in which entrepreneurs can be differentiated from small business managers . . . based on the entrepreneur's desire to grow the business rapidly.	Gundry, L. K., and H. P. Welsch (2001). "The Ambitions Entrepreneur: High Growth Strategies of Women-Owned Enterprises," *Journal of Business Venturing* 16(5), 453–470.
2001	. . . while inventions and innovations make significant contributions to the growth and competitiveness of national economies, there are problems in the U.K. surrounding independent inventors (often a small, one person business) and their marketing, where there has been failure to stimulate and exploit inventions compared to other industrialised countries. There are long term implications for economic competitiveness when new ideas are lost.	Wright, L. T., and C. Narrow (2001). "Improving Marketing Communication & Innovation Strategies in the Small Business Context," *Small Business Economics* 16(2), 113–123.
2001	This paper examines how and why the role of small business has become more important over time.	Jovanovic, B. (2001). "New Technology and the Small Firm," *Small Business Economics* 16(1), 53–55.

Table 1
Continued

Year	Research Description	Reference
2001	The role that small firms play in industrial organization has evolved considerably since the Second World War. This paper seeks to document how and why small business plays a very different role in industrial organization research today than it did some three decades ago.	Audretsch, D. B. (2001). "Research Issues Relating to Structure, Competition, and Performance of Small Technology-Based Firms," *Small Business Economics* 16(1), 37–51.
2001	(This paper) focuses on how antitrust enforcement helps preserve two freedoms: the freedom to engage in entrepreneurship, and the freedom to innovate.	Golodner, A. M. (2001). "Antitrust, Innovation, Entrepreneurship and Small Business," *Small Business Economics* 16(1), 31–35.
2002	Our study proposed and tested an entrepreneurial process model that examined the interrelationships among a small firm owner's personality, strategic orientation, and innovation.	Kickul, J., and L. K. Gundry (2002). "Prospecting for Strategic Advantage: The Proactive Entrepreneurial Personality and Small Firm Innovation," *Journal of Small Business Management* 40(2), 85–97.
2003	This study explores heterogeneity in how firms have achieved high growth . . . (and) identified seven different types of firm growth patterns. These patterns were related to firm age and size as well as industry affiliation.	Delmar, F., P. Davidsson, and W. B. Gartner (2003). "Arriving at the High-Growth Firm," *Journal of Business Venturing* 18(2), 189–216.
2003	. . . tested in a small business context, prospect theory (which) suggests that managers who are less satisfied may be more likely to introduce products with riskier characteristics, (t)he current study confirmed this finding that managers who were less satisfied introduced products into less familiar markets and required more resources.	Simon, M., S. M. Houghton, and S. Savelli (2003). "Out of the Frying Pan . . . ? Why Small Business Managers Introduce High-Risk Products," *Journal of Business Venturing* 18(3), 419–440.

Table 1
Continued

Year	Research Description	Reference
2004	(This paper suggests) that: (1) if only risk capital is injected, it flows straight to low-quality entrepreneurship . . . and (2) (s)ound legal systems, capital markets, and other structural features are necessary prerequisites for technopreneurship . . .	Venkataraman, S. (2004). "Regional Transformation through Technological Entrepreneurship," *Journal of Business Venturing* 19(1), 153–167.
2005	This article focuses upon the emergence of Virtual Teams that increasingly form the competitive core . . . (and) identifies and considers the stages and processes specific to Virtual Teams.	Matlay, H., and P. Westhead (2005). "Virtual Teams and the Rise of E-Entrepreneurship in Europe," *International Small Business Journal* 23(3), 279–302.
2005	The purpose of the study was to draw from the narratives a list of empirically grounded growth-related attributes that are associated with rapid-growth firms. The findings of the study resulted in the advancement of a conceptual model of the attributes of rapid-growth firms in four areas: founder characteristics, firm attributes, business practices, and human resource management (HRM) practices.	Barringer, B. R., F. F. Jones, and D. O. Neubaum (2005). "A Quantitative Content Analysis of the Characteristics of Rapid-Growth Firms and Their Founders," *Journal of Business Venturing* 20(5), 663–687.
2006	The intensity of small-business owners and the environmental difficulties they encountered were investigated as predictors of growth intentions in Turkey . . . found owner intensity to be significantly related to the three growth plan factors of technology improvement, resource aggregation, and market expansion.	Kozan, M. K., D. Oksoy, and O. Ozsoy (2006). "Growth Plans of Small Businesses in Turkey: Individual and Environmental Influences," *Journal of Small Business Management* 44(1), 114–129.

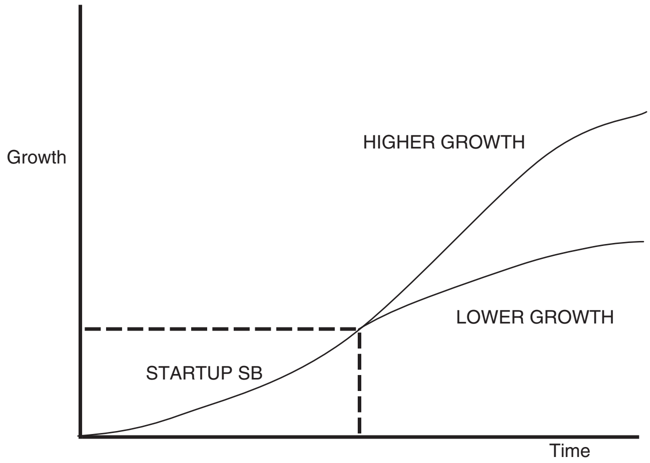
Table 1
Continued

Year	Research Description	Reference
2006	This study explores the homogeneity of small firms that have achieved and sustained high growth . . . We find that, controlling for location and performance, the high-growth small firms in our population experience similar management challenges regardless of the specific firm size, revenue level, or industry.	Chan, Y. E., N. Bhargava, and C. T. Street (2006). "Having Arrived: The Homogeneity of High-Growth Small Firms," *Journal of Small Business Management* 44(3), 426–440.
2007	This article examines to what extent recent empirical evidence can collectively and systematically substantiate the claim that entrepreneurship has important economic value . . . (e.g.) that entrepreneurs have a very important—but specific—function in the economy. They engender relatively much employment creation, productivity growth and produce and commercialize high-quality innovations.	van Praag, C. M., and P. H. Versloot (2007). "What Is the Value of Entrepreneurship? A Review of Recent Research," *Small Business Economics* 29(4), 351–382.

business research to move forward along the learning curve of its scientific development, and to take small business research to the next level with more prominent intellectual stature, more rigor needs to be added in theory building, method development, and hypothesis testing, and with more attention to issues that have been largely overlooked, such as technology, innovation, and corporate social responsibility.

Although some may favor promoting small business research as distinctive field of study that is isolated from companion disciplines, it is clear to us that what is needed instead is to draw from various disciplines and research traditions where theories and research methods have been more developed, and utilize small business as a context on which to test existing theories and to build new ones. For example, taking small business research as a "subfield" of management research, we could take what we already know and examine the extent to which this knowledge is applicable in the small business setting and, if not, ask ourselves: "Why?"; "What are the moderating factors and contextual factors that basically alter or moderate this relationship?"; "How, if at all, we can modify the existing theories that will help explain issues in small business areas?"; and "What is the extent to which

Figure 1
A Small vs. High Growth Comparison



we need to introduce a new theoretical prospective from still other disciplines?" Additionally, we need to ask how we can modify measures and refine research methodology so that we can better explain the phenomenon in question.

Accordingly, we suggest that "good research" in the small businesses setting should be guided by "good theories" and executed with "good methods." Weick (1989) suggested three criteria: (1) generalizability, (2) simplicity, and (3) accuracy, by which such questions might be answered. We observe that there is rarely any extant theory or method that can satisfy *all* three criteria simultaneously. As each method has its own advantages and limitations, there are inevitably tradeoffs. For instance, in recent years, complexity theory rooted in natural science and social network theory originating in sociology, have drawn more and more attention in mainstream management research, because the complex network and complex adaptive system perspectives offer new lenses for observing the coevolution between environment and firm strategy. According to Weick's criteria, complexity theory, for example, which relies on computer simulation, provides high level of generalizability and simplicity, but compromises on accuracy. In contrast, the traditional case-study approach can be highly accurate with detailed records and descriptions, but has inevitable problems in satisfying the simplicity and generalizability criteria.

A simple simile helps to illustrate the point that mismatches continue to be problematic. Let us consider research phenomena to be like nails, and that our task is to drive them into boards. Then in scientific research, theories and methods are the hammers. We choose different hammers for different nails, so they can work well together. However, what we increasingly observe in management research in general, and small business research in particular, is that students are trained with certain theoretical perspectives and research methods, and they hold tightly to these hammers looking for proper nails that fit their hammers.

Though we recognize the importance of theory building and theory testing, the "theory-to-theory" formula, if not utilized properly, may mislead researchers to cling to a given hammer while searching to find a suitable nail, and forget that different hammers are used to drive matching nails and must therefore be chosen according to the size and shape of the nail. By attending to the match between theory-hammers and methods-nails, we are more likely to generate significant and novel research.

The mismatch issue is observable when, for example, the current norm is to favor empirical research dominated by survey and statistical analyses. Such a research tradition essentially encourages researchers to focus only on "nails" that fit the hammer. Furthermore, one reason management research (including small business research) has been viewed by other academic disciplines as being too "soft" and "less scientific" may have to do with the situation that most of the empirical research presently being generated is resistant to repetition and verification. One source of this problem is the bias within the field of management against repetition of previous research. As scientific research is a cumulative process, the existence of this bias is unfortunate. As Popper reminds us, "we should not take even our own observations seriously, or accept them as scientific observations, until we have repeated and tested them" (Popper 1959, p. 45). Given such biases, many researchers, instead of focusing on phenomenon and on dependent variables, have used theory and method as starting points and have focused on adding new independent variables to existing empirical results. As a result, the research results cannot be replicated, compared and verified, and the field becomes increasingly fragmented.

To alleviate this problem, scholars studying small businesses should also look for phenomena with implications for theory and practice, and draw from theories and utilize methods appropriate to solve the problems observed (i.e., find hammers that fit the nails). If no "hammer" in management field can drive the "nail," we then must borrow "hammers" from other disciplines or design appropriate new "hammers" with them to then drive the "nail." For instance, industry clustering is widely believed to foster small business creation and facilitate technology transfer and innovation. To take this line of research to the next level would require more fine-tuned understanding on the emergence, formation and evolution of clusters. This will require more dynamic models and temporal data in order to reveal the underlying mechanism at different stages of cluster evolution (Tan 2006), and this is where computational simulation may lend its unique strengths. In the meantime, as computational modeling often relies on a set of assumptions, which may compromise accuracy, using the case method will enrich our understanding about the "initial condition" and the simulation process, and compensate for the missing details.

Similarly, how small businesses manage social responsibility is highly dependent on stakeholder-environmental characteristics (Mitchell, Agle, and Wood 1997). As a result, "initial conditions" in corporate social performance (CSP) models matter tremendously, and the social performance of small businesses may therefore be highly "path dependent." However, environments are dynamic and change over time, and managers do not simply react to the environment as they manage the stakeholder relationship; they learn from the environmental changes and "enact" the environment proactively (Tan and Tan 2005). Consequently, researchers may need to add the temporal dimension and examine the evolution of corporate social performance over time in stages. Thus, there are many opportunities for theory-building research that need attention in the small business setting.

The Opportunity for Theory-Building Research

One of the most significant opportunities now facing those doing entrepreneurship research in general, and innovation and technology strategy research in small business contexts in particular, is to engage in more systematic pursuit of theory-building research using the setting of small entrepreneurial firms as a context to generate theory. This theory-building opportunity is particularly important to recognize to the extent that Welsh and White (1981) are correct when they suggest that small, entrepreneurial businesses are not just little big businesses, but are rather distinctive agents of change relative to larger, older firms.

Glaser and Strauss (1967), in their highly influential book on developing grounded theories, observe that in order to effectively understand phenomena in particular contexts—such as the context represented by small entrepreneurial firms in comparison with large established firms—it is necessary to build theories that are in the first instance "grounded" in the context under consideration. Yet to date, it is arguable that relatively little of our energies as researchers studying small entrepreneurial firms have been devoted specifically to theory building. To a much greater extent, we have tested, and occasionally marginally refined, theories developed to explain the behavior of larger firms. For example, considerable research on small entrepreneurial ventures has adopted the resource-based view or its variant, the dynamic capabilities perspective (e.g., Brush, Edelman, and Manolova 2008). Similarly, theories of top management teams and their impacts on firms' behaviors and outcomes have been adapted to explain the performance of small entrepreneurial firms (e.g., Lester et al. 2006) as have theories regarding social networks and structural holes (e.g., BarNir and Smith 2002).

Clearly, programmatic research that extends extant theories to explain variance in outcomes in small businesses is crucial. But if small entrepreneurial firms really are a distinctive context, we should expect to see some original theories or concepts emerging from within the field. And indeed, there are already some notable notions that appear to have been created and advanced by researchers working within the small entrepreneurial firm context. For example, the concept of opportunity identification appears to have been advanced largely by those studying emerging entrepreneurial ventures, and is regarded by some as among the central pillars of study of entrepreneurship (e.g., Shane and Venkataraman 2000). The related but distinct notion of effectuation (Sarasvathy 2001) seems also to have been advanced based on consideration of entrepreneurial ventures, as does the concept of the "international new venture" (Oviatt and McDougall 1994). Other theories that have been significantly refined if not originally identified through the study of small or new ventures are theories of entrepreneurial orientation (Lumpkin and Dess 1996) and bricolage (Baker and Nelson 2005). Yet the number of theoretical contributions generated by those studying such firms remains relatively small.

As evidence of this point, Table 2, adapted from Mullen, Budheza, and Hafermalz (2008), indicates the portion of research that was specifically devoted to theory building between 2003 and 2006 in three of the most impactful entrepreneurship journals, *Journal of Small Business Management, Entrepreneurship: Theory and Practice* and *Journal of Business Venturing*. As this table indicates, 66 percent of published articles were devoted to primarily to theory testing, judging by the fact that they deployed quantitative methods suitable

Table 2
Proportion of Conceptual and Qualitative Research vs. Quantitative Research Published in Major Entrepreneurship Journals, 2003–2006

Journal	Article Design	2003	2004	2005	2006	Total	
		Percent	Percent	Percent	Percent	Count	Percent
JSBM	Conceptual	7	29	3	14	18	11
	Qualitative	7	6	3	0	8	5
JBV	Conceptual	27	41	23	24	55	27
	Qualitative	5	10	3	6	16	8
ET&P	Conceptual	55	63	57	26	66	47
	Qualitative	0	0	5	16	7	5
Totals	Conceptual	27	44	28	23	139	28
	Qualitative	5	6	4	8	31	6
	Quantitative	68	49	69	70	328	66
	Total	100	100	100	100	498	100

for such purposes. In contrast, only 28 percent of published papers were conceptual and presumably therefore devoted to theory building. An even smaller portion, only 6 percent, were qualitative in nature, which is particularly significant given Glaser and Strauss's assertion that inductive research based on the analysis of qualitative data is critical to the creation of theories that credibly account for systematic variation in distinctive contexts.

Evidence of the paucity of published research that is specifically dedicated to theory building is also apparent if we examine publications in major journals that devote a portion of their page space to entrepreneurship research. *Academy of Management Review* (*AMR*) publishes only articles aimed at contributing new theory. In the past five years, it is arguable that only seven of the roughly 150 articles published in *AMR* have provided theoretical insight on phenomena of special relevance to small entrepreneurial firms. In *Academy of Management Journal* (*AMJ*), which publishes only empirical papers, we might expect to find relevant theory development in those that are based on qualitative data collected in small entrepreneurial firms. Over the last five years, it appears that five papers clearly fitting this description have been published in *AMJ*. In *Administrative Science Quarterly* (*ASQ*), the number appears even smaller: over the last five years, only three papers developing theory inductively from qualitative data gathered in small or emerging firm contexts were published in *ASQ*. It is vitally important to note that there is no implication here that entrepreneurship research, conceptual, qualitative, or otherwise is being unfairly rejected or otherwise ill-served by these three journals. Rather, the point being made is that there appears to be a paucity of theory-building papers concerned with entrepreneurial phenomena being submitted to these journals.

These observations thus lead to the question: Where do the richest opportunities lie for more theory-building work that will inform our understanding of entrepreneurial phenomena in general and of innovation and new firm technology strategy in particular? The answer offered here is as follows. It is surely necessary for us as a community of scholars to engage in writing both conceptual papers and inductive papers based on qualitative data. However, a somewhat stronger case can be made for the latter, simply because many conceptual papers which start with extant theories and adapt them may be accordingly constrained in the extent to which they fully address the distinctive phenomena that are to be found in the small entrepreneurial firm context. This assertion is made simply because the theories inductive conceptual papers often adapt are those that have evolved with a focus on explaining actions in large, established firms. Thus, inductive conceptual work may often start with an a priori set of ideas that is somewhat more limiting than that entailed in inductive theory building based on qualitative data.

Conversely, theory building that takes full account of the small entrepreneurial firm context is precisely what qualitative research—or at least qualitative research in some traditions—is good for. As those conversant with the many diverse traditions of qualitative research know, some variants eschew explicit theory building and instead provide rich descriptions of lived experiences or narratives analyses of discourse that offer insight into the socially constructed nature of phenomena without claiming to develop theory per se (see Prasad [2005] for a useful account of a range of non-positivist traditions of qualitative research). Qualitative research that has goals other than theory building has much to offer, as has been argued elsewhere (e.g., Gartner 2007). Here, however, our concern is with qualitative research that is explicitly concerned with theory development.

Broadly speaking, there are two major traditions of qualitative research that have an explicit goal of theory building. The first is positivist qualitative research, and a considerable portion of the research that has been published in the entrepreneurship journals as well as in such journals as the *AMJ* and the *ASQ* fits recognizably within this tradition. In the field of management, this research tradition gained early credibility through the work of Eisenhardt (1989) who described a method for analyzing qualitative data that was much influenced by Glaser and Strauss (1967) and who advocated explicit attempts to build theory of a positivist nature via systematic analysis of qualitative data collected from multiple case studies of organizations.

The other major tradition of qualitative work which also aims explicitly to build theory and which informs a considerable portion of the qualitative research in management and entrepreneurship journals draws influence from sociologists such as Anthony Giddens (e.g., Giddens 1984), Pierre Bourdieu (e.g., Bourdieu 1984), and Michael Burawoy (e.g., Burawoy 1998). These sociologists take for granted a socially constructed reality, but argue that there are patterned regularities in a given sociohistorical setting. For convenience, we refer to this as the "structurationist" tradition: those working with the structurationist tradition make the case that phenomena of interest can be analyzed so as to build context specific theories which have an acknowledged temporal and social situatedness.

Though there are differences between these traditions, both are vital to advancing the enterprise of building theory that will help us understand patterned regularities that occur in small entrepreneurial firm contexts. Yet, as was already noted, relatively little work in either of these traditions is being published in the journals noted. Why might this be the case? Two answers seem to make sense. First, most management scholars are still given methodological training only in quantitative methodologies. Few schools have courses in qualitative methods, and those that do often combine philosophy of science with exposure to qualitative methods, effectively minimizing the extent to which doctoral students received training in the diverse approaches to gathering, analyzing and building theory from qualitative data. As a consequence, there are few who are trained to create or review theory-building qualitative research. Too many aspiring qualitative researchers submit manuscripts that fail to offer clear research questions, fail to take prior research into account, and that fail to convince readers that they are contributing to the literature (Suddaby 2006; Gephardt and Rynes 2004), effectively diminishing the chances that their papers will meet the standards of the peer-reviewed journals. Equally problematic, too few reviewers know how to constructively critique qualitative papers they receive.

The second reason that there may be a paucity of theory-building qualitative research is that even though there are many texts that instruct students of qualitative research on the diverse ways of collecting and analyzing qualitative data, there is less guidance available on what kinds of "theoretical products" may be generated from the analysis of qualitative data. In the following paragraphs, some observations are offered regarding the various ways that qualitative research may be structured when the goal is theory building.

One of the most obvious and popular forms of theory generated from the analysis of qualitative data is a propositional inventory. Such inventories are particularly common in, but far from exclusive to, qualitative research in the positivist tradition. This approach to contributing to theory has much to recommend it, in that the propositional format mirrors the hypothesis format familiar to those conversant only with quantitative research practices. Moreover, propositions may be crafted so as to be testable, which again makes for a rapprochement between quantitative and qualitative approaches to theory development and refinement. A recent example that illustrates this rapprochement very effectively can be found in Martens, Jennings, and Jennings (2007). In their paper, Martens et al. first generate a series of propositions regarding narrative techniques that entrepreneurial firms may use and that are likely to be influential on resource providers. They then test these propositions in a quantitative study of IPO issuers.

Perhaps the second most common type of theoretical contribution is the process theory. Process theories are particularly common in the structurationist tradition of theory-building qualitative research. Langley (1999) highlights diverse strategies for building theory from process data, and stresses the particular strengths of qualitative data for such purposes. A recent paper that has contributed to our understanding of processes in small entrepreneurial firms is Baker and Nelson (2005). In the paper, Baker and Nelson provide a detailed analysis of the steps through which entrepreneurial firms go in creating resources via a process of bricolage.

Two other types of theoretical contributions that are perhaps less well recognized, but that are also extremely important, can be discerned in a variety of qualitative papers. One of these is concept or construct development or refinement. The term concept is being used here to connote a network of interrelated set of constructs. Why are concept and construct contributions so important? Precisely because the distinctiveness of small, entrepreneurial business contexts means they are likely to be fertile grounds for identifying constructs or concepts that have not surfaced in research conducted in other business settings. When an entire conceptual network of interrelated constructs is introduced, it can frame or reframe the way a phenomenon is understood. When a theoretical contribution focuses more narrowly on one or two constructs, it can refine existing nomothetic networks in a manner that makes them more able to account for the kinds of relationships that exist in types of businesses than were previously taken into consideration. An example of a theoretical contribution that is comprised of concept and construct development can be found in a recent paper by Graebner and Eisenhardt (2004) where they inductively identify an alternative to existing concepts of acquisitions that contrasts with the common notion of a takeover as an acquisition. They also identify a range of constructs that are relevant to dynamics within the "acquisition as courtship" concept.

A final type of theoretical contribution, often offered in conjunction with other theoretical components, is a typology. Though it can be (fairly) argued that a typology is not a theory, there is a strong case to be made that a good typology is a theoretical contribution. Miller (1996, 1986) has offered an insightful analysis of what constitutes a good typology. In brief, good typologies can distinguish between related but distinct types of a construct or process, and identify clusters of related contingent variables or contextual influences that co-occur with specific types. A review of qualitative papers indicates that typologies are often developed and prove useful for contrasting existing constructs with new ones, or contrasting patterns of relationships observable in contexts with differing characteristics. An example can be found in Fischer and Reuber (2004) who identify different types of industry environments in which entrepreneurial firms may operate, and corresponding differences in the ways that customer engagement may benefit firms or detract from their performance.

Though this enumeration of strategies for building theory from qualitative research may not be exhaustive, it should provide readers with a sense of the range of strategies that exist for theorizing from qualitative research, and encourage more efforts of this much needed type. To achieve the kinds of theory that will improve our understanding of small entrepreneurial firms and contribute more broadly to the literatures on innovation and technology strategy, we will benefit from being both more sophisticated and more systematic in our approach to theory development.

Theory building can also be encouraging by posing new research questions about small business, and by revisiting research that has been predominantly conducted with samples of large businesses. Examples of each are illustrated in the third and fourth sections of this paper. In section three, new research questions are posed about what the social responsibilities of small businesses may be, and whether better ways of ensuring businesses meet these responsibilities can be devised. In section four, the literature on technology-based entrepreneurship is examined with a view to understanding how theories of the relationship between technology and entrepreneurship may need to be revised to take into account small versus large business contexts.

A New Research Vantage Point: Small Businesses' Social Responsibility to Live and Die with Meaning

For provocative research, we need uniqueness—especially new constructs that upon examination, and through greater understanding, actually make a difference in our thinking. Fortunately, in this Special Issue, we have been given a charge by the *JSBM* Editorial Board, to consider the "future of small business research" . . . to think together about the future of small business research from new and challenging perspectives.

To invoke new vantage points, we begin with Milton Friedman's (1970) assertion that the social responsibility of business is to increase its profits; and we modify and extend this notion to include the social responsibility of small business. We have termed it: "small business social responsibility." And furthermore, in the spirit of "futurity," we invite readers speculate together with us concerning what the notion of "small business social responsibility" might mean to the future of small business research. In particular, we ask readers to consider the question: What if we were to envision the social responsibility of small business to: (1) not live in vain and (2) not die in vain.

What would enacting this vision entail for new research vantage points?

We observe that most of the "action" in the life cycle of businesses (business "births" and business "deaths") actually happens in "small" business. As to births: most businesses start small. Very few are full blown at 20 or 30 thousand employees—at least in Western market economies. And as to business deaths, we observe that: by the time it is "over" there are not too many people left to lock up and turn out the lights. So what we have implicated in this small business phenomenon is a very interesting element about "smallness" that may give interested scholars and practitioners an opportunity to conduct research we have never thought about before—which could directly examine the implication of small businesses' "not living in vain" and "not dying in vain."

Such a suggestion implicates at least two new comprehensive research initiatives. The first: live with meaning, suggests venture creation should start with life in mind. We wonder how many small-business new venturers in fact do start with the life of their business in mind. We might expect that all venture initiators intend for their small business to live; but unfortunately (as it appears from failure-rate statistics, e.g., Bresnahan [2005]), many do not actually know *how* to do it, and therefore fail. To our knowledge at present, there is no such thing as a small business "APGAR,"[1] which asks every new venture founder critical "live-with meaning" questions, such as: To what extent does this business have a pulse (e.g., is there a viable business model)? To what extent can it process economic oxygen (e.g., transactions)? etc. So, as one suggestion that arises from this new stance, we might suggest the need for all concerned to better understand the things we need to know to *start with life in mind*. For example, once such research topic would be to better understand what it means to assess new small businesses early and often. This is not something that is yet well-enough done, nor is it well-documented in the research literature. We acknowledge that in the practitioner community, some authors (e.g., in the popular press) have put forward business plan evaluation systems and success recipes; and that small business development center checklists and a variety of helpful hints are available. And we also acknowledge that the bankers and the venture financing community also have check-list-type tools. But at the present stage of development, we observe that living-with-meaning-assessments deal primarily with analyses that comprise a relatively narrow sliver of what is in fact involved in sustaining the life of a small business.

Methods that ought to be suggested, tested, explored, and developed would, for example, examine key factors: they would establish new venture analysis standards to answer questions such as: What should this business be able to do before we "plug it in"? And we should then be able to apply these standards consistently: to, for example, build a track-record database. This makes sense within the larger business community where the uses of best practices comparisons are common. So as a beginning point for provoking new, unique, and helpful small business research we simply inquire: Where is the "live-with-meaning" best-practices database? And we observe, in answer, that to our knowledge there is not one (yet). Part of the "futurity" of small business research is to get on with creating such data, and then to undertake the systematic analysis that can add much more meaning to the life of new small businesses.

We next inquire about *dying with meaning*, and ask: what is intended by the idea that a small business should "die with meaning"? Possibly a first step would be that, should a new venture fail, this failure should count for something other than grief and embarrassment and trauma. We consider this to be an important social and economic issue. Some sources suggest that 80 percent of new businesses fail (Bresnahan 2005). Reynolds (1995) delved further into this reporting puzzle, and suggests that if one rigorously compares statistics, removes double-counting, etc., the success/failure ratio of new small businesses may be more in the neighborhood of 50/50. Let us therefore make a simple comparison that places this failure-rate into another context. If, for example, you went to the car lot and bought your new Lexus, and as you were about to turn the key, the salesperson said, "By the way there is a 50 percent chance it won't

[1]An "APGAR" is a simple, repeatable method to quickly assess the health of newborn children on five simple criteria on a scale from zero to 10. The five criteria (**A**ppearance, **P**ulse, **G**rimace, **A**ctivity, **R**espiration) make up the acronym APGAR.

start," what would you respond? You may say: "But I paid all this money!" And the salesperson might counter: "Well still, that is the best we can do right now; take it or leave it." When we apply such a scenario to small business (That's the best we can do!—and frankly at present it *is* the best we can do); this begs a future question for small business research: is such a *status quo* good enough?

So we therefore return to the thought-provoking idea: dying with meaning. What would be involved for a small business to die with meaning? Our sense is that small business research needs to uncover an entirely different way of thinking. It might mean, for example, that we need to better understand entrepreneurial expertise. These research questions have been under study for just about 15 years (e.g., Mitchell 2005, 2003, 1996, 1994; Mitchell and Chesteen 1995; Mitchell and Seawright 1995; Mitchell et al. 2009 Forthcoming; Mitchell, Mitchell, and Smith 2008; Mitchell et al. 2007, 2002, 2000). So, to help us to see how the study of entrepreneurial expertise leads us to better understand the idea that a small business might *die with meaning*, let us as an example, consider the questions: What is entrepreneurial expertise, how is it acquired, and how is it applied?

Recent work (e.g., Mitchell, Mitchell, and Smith 2008) confirms that failure recognition creates a kind of opportunity creation mindset that only develops as a result of a new venture failure. We invite readers to imagine the potential payoff: If we can understand how to compose, classify, and create entrepreneurial expertise (e.g., Mitchell 1994), we can then begin to aspire to develop the entrepreneurial opportunity-creation mindset as a national asset. In this respect, then, *dying with meaning* does not mean that a business terminates and the entrepreneur walks off this stage embarrassed, angry, and with few remaining relationships (business or personal). Rather (based upon the further development of this example line of research), when the venture dies, society might develop a more productive response and take the view that the learning store of expertise has grown, is valued, and prepares many such individuals for greater opportunity-creation effectiveness in the future. Such a remarkably new response would convey to an entrepreneur-in-training, the following affirmation: "I now have this very big part of my mind that understands all kinds of things not to do," but also the message: "most things that I know—as an entrepreneur who terminated a business—are still recyclable!" "A few decisions may have caused the venture to become disabled, to have to exit, but most of that knowledge is still there." "It can be tweaked." "It's a national asset!"

And, to continue in the spirit of provoking new research pathways, we further inquire as follows: What if we then do the research needed to begin to develop the opportunity-creation entrepreneurial mindset as a national asset? Can we consequently aspire to use the bankruptcy courts evermore effectively: for example, as a gatekeeper and allow us to be able to salvage the national treasury of entrepreneurial expertise. Presently, we observe that a person/business shows up at bankruptcy court to admit that s/he/it ran out of cash. Unfortunately, we forget the other side of the equation—the things that were added to the national balance sheet such as experience, expertise, and the lower probability of mistake repetition in a follow-on venture.

If we were to therefore study how to use the bankruptcy-courts function as gatekeeper, we would need research to support a new profession called, for example, "venture forensics," to isolate the source of a business failure, validate the expert-capital still available to the national venturing treasury, and thereby create opportunity-creation entrepreneurial mindsets. Debriefing processes have a long tradition of applicability in scientific exploration. Under such a scenario, we might envision that—based upon the extensive new and unique small business research generated to fill this need—neither individual entrepreneurs nor their businesses would be released from bankruptcy until conducting a debriefing mandated by the courts (as gatekeeper acting for society). Such a debriefing process would be an asset-creating process; and its conceptualization suggests that we further inquire concerning examples of processes such as the ones we have been suggesting to assist with small businesses "living with meaning" and "dying with meaning."

We first present an example of living with meaning familiar to one of the authors: the NVT (New Venture Template) project (Mitchell 1998, 1995) that is a system for helping new small businesses to live with meaning—to start with life in mind. Using a comprehensive literature review and in-depth case study methodology, Mitchell investigated the causes of new venture failure much beyond the standard venture capital questions: (e.g., "is this venture going to make money and can these entrepreneurs run it?"). The data were collected and made available by the WBI (Wayne Brown Institute), which holds capital-raising events, in the Intermountain West, Silicon Valley, and in Hawaii for the Pacific Rim. Over the years this method has been used to assess many new ventures early and often, and also to assist in removing known constraints to venture survival. Tracking of the results shows at least a tripled hit rate (Mainprize et al. 2003). The process is conducted as follows.

The WBI, randomly assigns ventures to development teams of venture capitalists. Team A uses the standard methods which basically ask the questions: "Is the venture going to make a profit?" and "Can the management team run the venture over time?" Team B uses the NVT, which "drills down" two more levels: from two elements (Is the venture a "business"?/profitable; and Can you keep it?/management), within which are nested six elements (levels of: innovation, value, persistence, scarcity preservation, appropriability protection, and flexibility), within which are further nested a 15-element array of criteria that can provide much more fine-grained distinctions among new ventures (Mitchell et al. 1998). All the venture capital assessors were very familiar with assessing developing ventures. The primary idea was to ask enough questions (e.g., the NVT 15-question array), such that a distinct descriptive pattern can emerge (a 15-element vector of ratings). This distinctive pattern could then be simultaneously compared with various venture prototypes represented by standard 15-element vectors or "templates." Using a comparison algorithm that produces a first-moment correlation statistically standardized between 0 and 1, the assessors obtained a like-kind-basis coefficient for evaluation and for suggesting action that is needed for the venture to be more likely to "live with meaning." An example of results from this comparison is shown in Figure 2.

Results of the study show that ventures that went through team A had an approximate 17 percent hit rate (which is the average of one really bright success, one medium, one break-even, and 7 progressively "lousies": a net of 17 percent). For team B, which used the NVT, the hit rate when last checked, was in the 54 percent range: approximately triple the opportunity for a venture to "live with meaning."

A follow-on analysis in greater depth (used more for research than for practice presently) involves the examination of in-depth assessments on a vector-by-vector basis (element-by-element) across ventures within an industry group. In

Figure 2
The Two-Dimensional New Venture Template
Visual Comparison across Venture Types

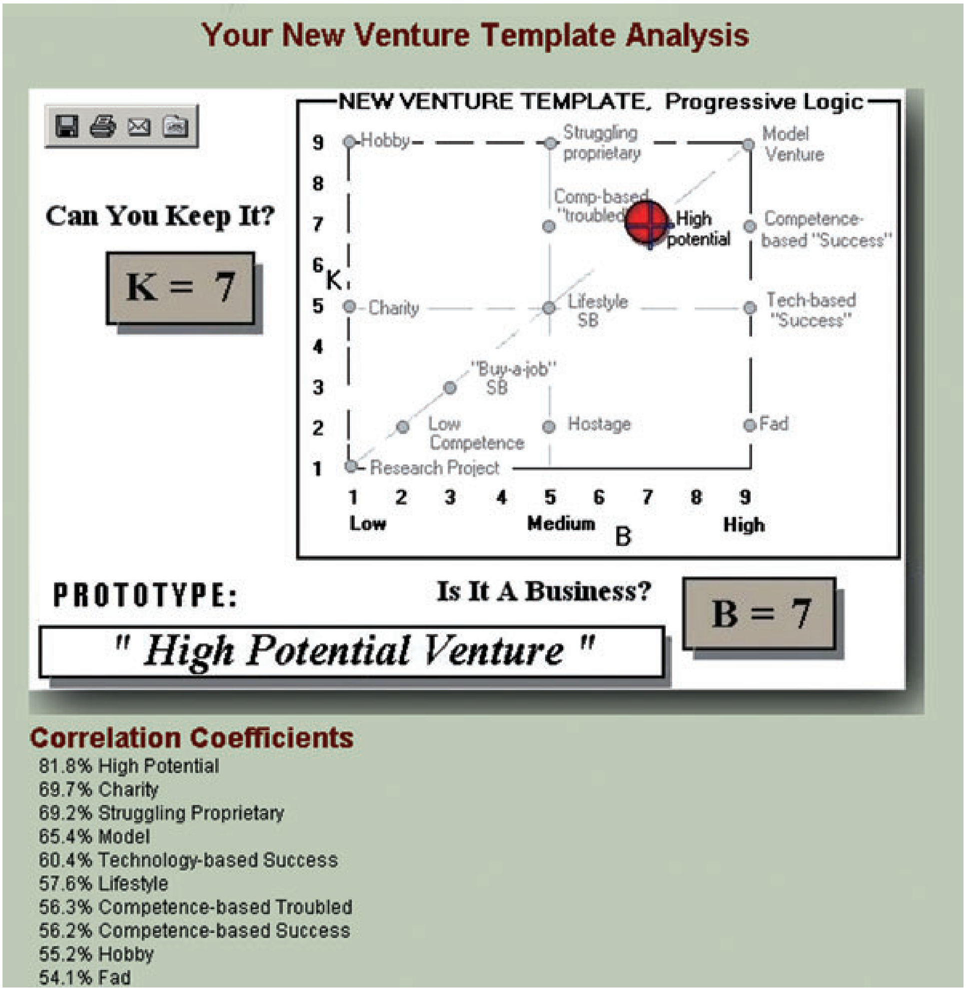
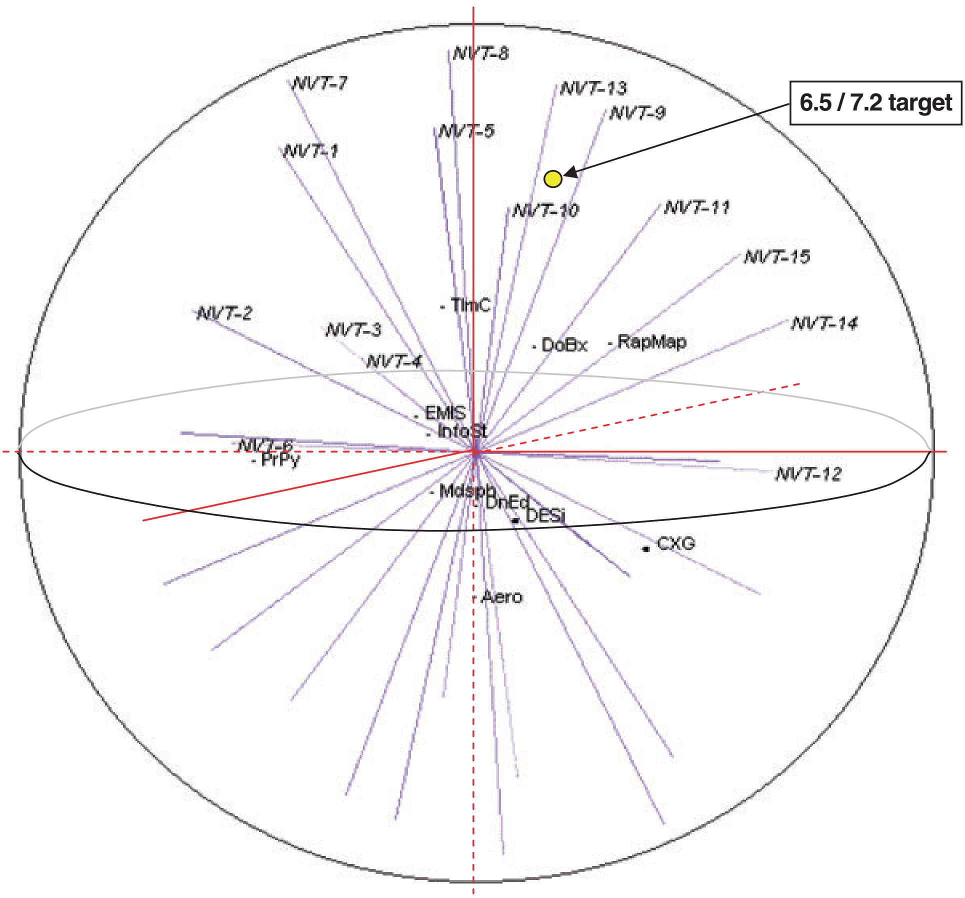


Figure 3, represented in n-dimensional space is a visualization of an analysis of the computer-services industry new ventures in the database mapped in three dimensions.

The extent to which the lines are near each other means closer correlation. Those vectors at right angles are the orthogonal variables; and those at 180° are inversely correlated (e.g., variables 6 and 12 are negatively related). In the three-dimensional simulation that we have constructed, the researcher can rotate the sphere and get a look at the relationships from every possible perspective; and in this manner it is easier to

Figure 3
The Three-Dimensional New Venture Template
Visual Comparison across Venture Attributes



conceptualize what might be needed to make a difference in new ventures such that they can "live with meaning."

To further provide the context, we note that the WBI has actually started the cooperative venturing network with an initiative called Venture Analysis Standards 2000 at its core.[2] Living with meaning, then—one version of the new venture APGAR—has been in use now for approximately 15 years. As researchers we have been tracking results, and are now in a position to assert that if actors within the small business arena follow and apply this standard consistently, we might aspire to ISO-type criteria for new ventures—quality standards that can make a difference in the sur-

[2]Information is available from http://www.venturecapital.org/fundamentals.htm

vival of new ventures: compliance with expected standards. Put in terms of the quality movement—essentially Deming's founding premise (e.g., Deming 1986) to simply focus on variability (the upper and lower control limit), such an approach has a simple and highly applicable logic when applied to a new small business *living with meaning*. This is because in small business formation, we have a quality control problem the creation of new ventures that may (unfortunately) live in vain because we don't assess early and often. With quality control of the type suggested, we could then build a track record database. Presently, as we understand it, the database includes about 400 companies, and the data are sufficient such that we now have a credible conceptual foundation from which to teach new venture "living with meaning" to students in our entrepreneurship programs, and within SBDC networks, etc.

And now, turning attention toward an example of an initiative that might be suggested in response to the challenge to help small businesses die/terminate with meaning we recall to the mind of the reader the idea of a venture forensics initiative, as previously introduced. Such an approach would be essentially metacognitive: thinking about thinking (Mitchell et al. 2005). That is, we might investigate how people think about their new venture experience. Kruger and Dunning (1999) suggest that unskilled persons inflate self-assessments, and skilled persons inflate others' assessments. Through a venture forensics initiative that examines failed new ventures, these thinking errors can be removed through better calibration of attributions for all concerned.

In a typical situation—before the venture starts—a first-time entrepreneur would be expected to be unskilled and unaware. People often just plunge in—sometimes called the "entrepreneurial seizure" (Gerber 1986) moment. Then, on the other end of the typical situation (to complete the cycle) after an entrepreneur has failed, instead of having an inflated self-assessment, we would expect to see self-assessments to be very low, with entrepreneurs actually miscalibrating in the other direction—actually inflating other's assessments. And based on this repetitive behavior a failure rate in an economy can be computed. Venture forensics could lower this, by helping recalibrate both unskilled and unawareness elements (toward a more realistic self-assessment). What would a venture forensics initiative do? Essentially in practice, (as noted) the bankruptcy process could be enlisted to support this recalibration process through venture forensics; and thereby assist small business in its social responsibility to die with much more meaning —not to die in vain.

The small business live-with-meaning and die-with-meaning initiatives would have the following implications for research. As regards living with meaning (i.e., keeping new small business life in mind), we could and should create normative theory building with assessment, we could increase audits with methods, and we could do instrumental empirical research. Using such tools like the key-factors-analysis-based NVT approach, we could do focused descriptive research with the standards. And from a practitioner-focused standpoint, we could focus on several crucial "enactment"-type questions, such as: How, as a field, do we actually enact the needed changes? How can a grassroots broad-scope appreciation for the importance of small business life be brought about? From a practical "how might we get this done?" standpoint, we wonder if the answer lies within approaches that are being suggested within the newly emerging institutional entrepreneurship research stream, where institutional entrepreneurs enact changes the underlying meanings within society. According to some of the recent literature, institutional entrepreneurs act upon underlying beliefs and values to create and transform institutions (Greenwood and Suddaby 2006; Maguire, Hardy, and Lawrence 2004; Garud, Jain, and Kumaraswamy 2002). In fact, Hoffman (1999) suggests that institutional entrepreneurship occurs as organizational-field configurations (such as underlying beliefs and values) are changed, thereby resulting in the alteration of the corresponding institutions (p. 353). This is the practical process we see unfolding as the result of the new research we are suggesting.

To add clarity to our point, we might (with yet another simile) compare institutional entrepreneurship to the process of managing luggage in an airport: When somebody gets a new venture rolling, it's like putting your belongings in roller board carry-on luggage, so you can get it through the airport faster. Institutional entrepreneurship is like getting you and your roller board suitcase on one of those moving sidewalks, where you actually move everybody with the roller board forward. Institutional entrepreneurship moves everyone on the walkway. Applied consistently with large sample empirics, we might then envision the track-record database, and instead of the FAA, we would have the FVA, which would stand for (hypothetically) the Federal Venture Administration. Or if we are anti-regulation, we might instead suggest something like a "Google-Venture" so that people can enter tradeoff information into an analytical website and actually begin to assess themselves. Our point: plenty of provocative research opportunities exist for exploring how new small businesses might better *live with meaning*.

Now as respects research initiatives that can arise from framing new questions in terms of *dying with meaning*, we could undertake normative theory building so that we can better understand expertise, which would really give us a new vision, for example, for public education. Presently, we observe that we have people starting businesses who could have learned to avoid all the standard pitfalls, had we enabled this type of education within the elementary schools. We could enable the failure recognition/opportunity creation process introduced earlier, by changing the way people think about their having to go bankrupt—to actually preserve what was learned (e.g., Mitchell, Mitchell, and Smith 2008). Instrumental research could develop the entrepreneurial mindset as a national asset, using the bankruptcy courts; and then new descriptive research could use venture forensics for economic planning because of the possibilities that arise from a much higher-veracity database. We could, with explicit attributions and incredible data, accelerate the opportunity options learning cycle (ibid.).

So, as a suggestion for a "futuristic" vision of small business research, might we therefore renew our beginning assertion that the social responsibility of small business is to neither live nor die in vain? Of course we need to learn a great deal more to enable us to fully shoulder this responsibility. Small business social responsibility is one notion that suggests an important future for a small business research, especially as small business confronts the ever-changing technological landscape, which dramatically impacts the living and dying of new ventures. We therefore further refine our focus to look specifically at the future of research in technology entrepreneurship.

Wither Research in Technological Entrepreneurship?

Schumpeter's (1934) notion of economic creative destruction comes closest to a description of the relationship between technology and entrepreneurship. It posits the emergence of novel combinations wherein macroeconomic or technological forces trigger "reforms . . . [in] the pattern of production . . .

and reorganize an industry" by entrepreneurs. Since that time, research has focused on the *mechanisms* by which businesses respond to such events. Today, accepted theories view businesses as configuration of resources that are seamlessly manipulated to create and claim emerging value propositions. However, these theories, notably dynamic capabilities, have not properly delineated the relative importance of information, technology, human ability, human motivation, organizational design, or the *processes* by which these elements are (re)combined to form the sorts of emerging businesses contemplated by Schumpeterian theorists. More fundamentally, theories of the relationship between technology and entrepreneurship have not made a distinction between technological entrepreneurship in small and large businesses. Is the technology driven value creation in small businesses the same as in large businesses but writ small? Our theories have not yet considered this question in part because the boundaries between the two are not theoretically defined.

Serious research in technological entrepreneurship became prominent only when scholars in the management of technology and engineering management began to consider the centrality of the entrepreneur. Theories of production subsumed the individual actor. In the old world, managing technology was largely about technological and organizational choices. *How* innovation arose in such a world was unclear and it was only when the technological entrepreneur, a hybrid of scientist/engineer and businessperson, was included in the equation that a proper understanding could be attempted.

Technological entrepreneurship research is foremost about understanding the conditions and drivers that lead to the exploitation of scientific discoveries for value creation, typically in small firm settings (although attention on corporate entrepreneurship in large firms has gain traction). The process of opportunity search is heavily influenced by the entrepreneur's background as well as the task environment in which the entrepreneur finds himself. Therefore, a large part of the extant research is focused on opportunity identification. Theories in opportunity identification recognize the importance of bounded rationality and differences in risk preferences among entrepreneurs. Bounded rationality not only determines the limits to individual information processing but also the environmental conditions that create uncertainty. Risk preferences are therefore a function of both individual differences *and* the environmental conditions confronting the decision-maker. Therefore what is important is to understand when technology exploitation does *not* occur as much as when it occurs.

There are three levels of analyses in technological entrepreneurship research. At the individual level the focus is on the scientist-entrepreneur, venture capitalist, and other individuals that incite and drive innovation. At the organizational level the research is on entrepreneurial teams, structures, processes, and interorganizational relationships that enable value creation and appropriation. At the systems level it is about exchanges across value networks; constrained or enabled by governing conditions such as technology and competition policy, industry standards, and the economics of geographic location. Properly executed, technological entrepreneurship research is therefore interdisciplinary, multilevel, and incorporate dynamic systems with negative *and* positive feedback loops. To date, few theories of technological entrepreneurship posit models that include negative feedback loops.

In sum, we should think about how the impact of a change in one variable cascades through an entrepreneurial system and leads to changes in the relationships between the other variables.

For example, a scientist may become alert to commercialization opportunities in his research when he encounters the need to create more resources for future research. As the market potential of these opportunities improves, the scientist-entrepreneur may become more willing to consider riskier opportunities. His risk propensity may, however, be tempered by life-stage considerations such as the need to accumulate an inheritance for an offspring. As he gains experience at venturing, the scientist-entrepreneur's network grows more dense and complex, leading to a wider range of opportunities. Networks also bring resources that attenuate risk and thus enhance opportunity exploitation. Hence, the types of ventures an entrepreneur finds attractive shifts with changes in his individual differences, which impacts the way in which these opportunities can be pursued.

One area that deserves more attention in the literature is the *emergence* of opportunities, individuals and businesses that arise from technological change. Emergence is the spontaneous appearance of ordered social and organizational structures. Past researchers have identified, though not always consciously, the phenomenon of emergence in the discovery and exploitation of economic opportunities, the coming-into-being of new firms, the unexpected growth spurt of small businesses, and in the creation of new industries. In the abstract, a small business can be defined as the coming together of formerly dispersed knowledge about opportunity and technology at a specific location in a point in time. However, what is yet to be explicated in this research is the *process* by which emergence occurs. More importantly, we do not yet know the conditions under which small businesses will *not* emerge. We suspect these are not simply the absence of the enabling conditions that foster emergence. For example, government policies that favor technology transfer and foreign direct investment by multinationals may in fact militate *against* the emergence of a domestic technology-based small business sector.

The concept of emergence helps us articulate the suddenness of the organizing processes that entrepreneurs encounter prior to the commencement of production. Therefore, a theory of how technology drives or is driven by entrepreneurship should be capable of modeling the prefirm formation stage, which has till now been poorly articulated in the literature. Note that organizational emergence can also describe sudden changes to technological trajectories that represent something novel and unanticipated for the growing business.

Another area that deserves more attention in future research is that of the *interactions* among actors in a value network. We know from the research in national innovation systems that actors in the ecology of value creation can be risk arbitragers, resource providers, and knowledge generators. The impact of technology change and shifts in legal institutions and social norms can impede, enhance or alter the roles of these actors. When these roles are altered (e.g., knowledge generators becoming risk arbitragers, as is the case when scientists become entrepreneurs) the cycle of value creation is altered, sometimes with unintended consequences.

The role of public policy is another important research area that deserves more attention. For example, competition policy can reduce the incentives for entrepreneurial activity by reducing the gains from risk arbitrage or innovation. On the other hand, economic policy targeting specific technologies for government support can create economies of scale and scope in research and development and give rise to entire populations of new businesses. We believe that the impact of government intervention depends on the stage of development of an industry and might prove beneficial during the early stages of organization when markets fail to form. Initial conditions matter and high levels of general human capital engendered by education, reliable and low cost infrastructure and fairly enforced legislation can reduce transactions costs and startup costs.

In doing research on technological entrepreneurship, some of the techniques that deserve more attention include critical events analysis with its focus on phase changing events, dynamic multi-agent games employing objective based optimization routines, evolutionary modeling with complex systems theory, process mapping, multi-case clinical studies, panel data analysis using probabilistic models, and computer simulation. These types of techniques can account for population and idiosyncratic emergence processes, which means that they can account for the simultaneous existence of established *and* small businesses that face the same economic and sociological conditions in a population.

With respect to data requirements, dynamic models require temporal data and statistical procedures capable of handling such data. Industries evolve at different rates, and experience technological shocks at different time periods. Additionally, dynamic models are necessarily recursive, which demand sophisticated use of non-linear estimation techniques that are capable of operating at multiple levels of analyses. However, the real challenge in entrepreneurial research is to obtain such data as they are likely to be proprietary, noisy, and difficult to compare across organizations, time periods, and industrial contexts.

In sum, because technological entrepreneurship is a multilevel phenomenon, theory building and testing has to pay attention to the interactions between individual, group, formal organization, and industry levels of analyses, even though testable models tend to isolate these levels. Each level of analysis can be represented as a system of interdependent components. At higher levels these components combine to form a system, itself a component in the next higher level of analysis. Hence, one cannot fully understand opportunity recognition and exploitation as a co-evolutionary emergent phenomenon without being sensitive to its higher contexts—culture, institutional arrangements, and political-economic exigencies. This, we believe, is the direction in which the research should proceed.

Conclusion

One of the hallmarks of provocative research is that it generates more questions with continuing research, than it does answers. Yet as a field, the system within which new scholarship emerges has not yet been perfected—and in some ways is following a direction that is less than productive, and is certainly not provocative. A brief anecdote from one of the authors—although somewhat in jest—illustrates this point.

> I was invited to a highly respected university to talk to a group of doctoral students who are learning to do empirical research: what we might call mainstream management research. In their program they had all learned how to conduct a literature review, write a paper, develop hypotheses, gather and test data, get results and then produce a discussion and implications section for that paper. I asked them: "So how does it work for your project?" They answered: "We collected data, we tested the model, we got the results, and we find that our hypotheses were not supported." Then I asked: "What do you do then?" They said: "We go back and revise the model." I then asked: "What if it still doesn't work?"

> They answered: "We go back and revise the hypotheses." But I persisted, and asked: "What if it still does not work? Are you going to revise the data?"

Yes, it is an anecdote, but it still leads us to ask: To what extent does this anecdote (or some portion of it) actually exist in research practice? And if, at least a partial hint of reality can be expected, what does this mean for the creation of provocative research? Certainly such practices are at variance with the traditional expectations, which go something like:

- One first reads the literature to understand what is known with respect to theory.
- Then you find unique concept to test the theory according to what is not yet known and propose it as a hypothesis.
- From theory design appropriate research that will test the theory or hypothesis.
- If you truly develop a set of hypotheses grounded in good theory, and your research design is rigorous and solid; but the result do not support the theory, well you actually have a significant finding, because either the previous theory is wrong, or your research design is wrong; and if you can successfully assert that there is nothing wrong with your design and execution, you've got a significant finding, at least according to logical-positivist falsification theory (e.g., Stinchcombe 1968).

Presently, however, it is the preference of the research community to expect scholars to try to support something that somebody else has already done, or to propose yet another alternative hypotheses; so as a result, we have long rigorously designed and crafted empirical papers that basically make trivial contributions, do not offer provocative ideas, and do not inspire others to think. Accordingly, the research becomes more standard, more standardized, and more fragmented.

What does this mean for the future of small business research, especially for the maturing of the dynamic view of small business research, where small business is the main character at the "center of the action" in technology and innovation? In the foregoing sections we have presented challenges, perspectives, and observations that eschew "business as usual." In fact, they dimensionalize a set of wide and varied opportunities for the rising generation of entrepreneurship, small business, and technology and innovation strategy scholars to challenge existing paradigms and do something that's truly innovative. It is toward this future that we look; and we invite interested colleagues to conduct an investigation that lives up to the potential of the newly-emerging stream small business management research: to truly extend our understanding of the attributes and strategies that enable small businesses to grow, to contribute and to flourish at the center of the innovation and technology-based action.

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