Introduction

Over the past couple of decades, entrepreneurship research has undergone tremendous growth as well as maturation and institutionalization (Aldrich, 2012; Davidsson, 2016b; Meyer et al., 2012). Overall, this has no doubt been for the better, and when we sometimes feel that we have not come far enough, fast enough, it is probably because so much knowledge about the phenomenon is now taken for granted. One may have to revisit some of the early literature or engage in an extended conversation with a complete newcomer to the field in order to realize how much we have actually learned.

In this chapter I will offer some observations and speculations about current developments as well as likely and desirable scenarios for the future. The particular topics I will discuss are (a) the delineation of the “entrepreneurship research” field and community; (b) data and data sources; (c) the quest for increased theoretical precision; and (d) demands for practical relevance and real-world impact. My observations come from the perspective of an experienced “business school” researcher (with escapes into applied psychology and applied economics) who was an early (i.e., 1980s) entrant into the emerging field of entrepreneurship research, who has predominantly worked with large-scale survey and archival data, and who views outlets like the *Journal of Business Venturing* and *Entrepreneurship Theory & Practice* as main communication channels.

Narrowing and Broadening the Field

Over time, the field of entrepreneurship research has drifted away from having a considerable overlap with issues of small business management to having—at least in “business school research”—an increased overlap with research on innovation and strategy (Baker...
There seems to be increasing consensus around creation of new economic activities as the core of what entrepreneurship is and what entrepreneurship research should study (Carlsson et al., 2013; McMullen & Dimov, 2013; Wiklund, Davidsson, Audretsch, & Karlsson, 2011). Yet, characterizations of the field as being a “hodgepodge” (Shane & Venkataraman, 2000) or a “potpourri” (Low, 2001) remain relevant to an extent. The journal Small Business Economics has added “An Entrepreneurship Journal” to its name, but has not dropped the stronger connotation to organizational size from its title. Entrepreneurship Theory & Practice (ETP) features special issues on family business on a regular basis, and Family Business Review (FBR) is commonly included in the set of “entrepreneurship journals” as are the Journal of Small Business Management and International Small Business Journal (e.g., Teixeira, 2011). The Entrepreneurship Division of the Academy of Management (ENT) has retained “the characteristics, actions, and challenges of owner-managers and their businesses” as one of two alternative understandings of its stated domain (Mitchell, 2011).

It is not difficult to understand why this is so. Why would Small Business Economics’ publisher, Springer, take the risk of dropping a well-established brand name? Why would ENT choose to alienate a large proportion of its membership, or ETP willingly forgo the citation volumes generated by their special issues on family business? However, there is an undeniable problem. Critics justifiably argue that self-employment and small business activity do not equate to entrepreneurship (Henrekson & Sanandaji, 2014) and there is objective, quantitative evidence that FBR is an outlier compared to other journals associated with the field of entrepreneurship (Teixeira, 2011, p. 17). Self-employment, small business, and family business are organizational and governance contexts that are interesting and important objects of study. However, only occasionally does an interest in these phenomena coincide with a focus on creation of new economic activities. If the latter is our arrived-upon and agreed-upon understanding of “entrepreneurship” then a separation of entrepreneurship from the self-employment, small business, and family business contexts is the better long-term solution for all parties. I think it is time to complete the separation.

I would argue that criteria like innovativeness and growth-orientation should not be the basis for separation. That would not only be impractical but also an example of methodologically unsound sampling on the dependent variable. Instead, I think the divider should be entry versus state. The state of being self-employed or an established small organization has no definitional or otherwise obvious connection to “creation of new economic activities.” Entry does. True, we know that representative empirical populations of entry attempts are dominated by a “modest majority” and that this is also problematic for some purposes (Crawford, Aguinis, Lichtenstein, Davidsson, & McKelvey, 2015; Davidsson & Gordon, 2012). Nonetheless, apart from utter failures, the members of the modest majority perform, at least to some small extent, what I have
argued elsewhere (Davidsson, 2016c) are the essential functions of entrepreneurship in driving the economy forward:

1) They provide customers with new choice alternatives, potentially giving some of those customers more value for their money.
2) They stimulate incumbent actors to improve their market offerings in their turn, which increases efficiency and/or effectiveness of those actors.
3) If perceived to be successful, they attract other new entrants to the market, thus further increasing competitive pressures toward improved efficiency and effectiveness.

To this list—which essentially reflects wealth creation—we should perhaps add wealth redistribution and note that the alignment of the two is of utmost societal importance (Baumol, 1990). We may to varying degrees have qualms about uber-rich and uber-powerful plutocratic dynasties, but spectacular cases of what Baumol calls “productive entrepreneurship” are in my book a far preferable means of creating and destroying these dynasties compared to the alternative mechanisms of radical wealth redistribution (i.e., war, revolution, robbery, and confiscation) that mankind has witnessed through history.

The above places entrepreneurship in the economic but not necessarily in the commercial domain. As long as there is a market-like situation with equivalents of customers and incumbents present, we can welcome the broadening of the field exemplified by the recent surge in research on “social” and “green” entrepreneurship (Shepherd & Patzelt, 2011; Zahra, Gedajlovic, Neubaum, & Shulman, 2009). In fact, this is a refreshing move away from a narrow focus on “the art of enriching oneself by starting and growing one’s own business” which at one point threatened to come to dominate entrepreneurship research, at least in business schools (Davidsson & Wiklund, 2001).

Refreshing also is the disciplinary broadening of the field. After some debate about “entrepreneurship is a distinctive domain” versus “entrepreneurship belongs in the disciplines” (Sorenson & Stuart, 2008; Venkataraman, 1997) most have probably come to agree with Low (2001) that these perspectives are not contradictory but complementary. We need strong disciplinary theory and methods insights applied to the phenomenon of “creation of new economic activity” (Wiklund et al., 2011).

There used to be only a few individuals with strong disciplinary identities in economics, psychology, or sociology (e.g., Acs, Aldrich, Audretsch, Baron, Frese, Parker, Shaver) who participated regularly in the interdisciplinary community or entrepreneurship researchers, plus a few, significant “transient” visitors to the field (Landström & Persson, 2010). We now see the development of subcommunities of researchers identifying firstly as, for example, psychologists, sociologists, economists, and so on, who devote most of their research interest and effort to studying entrepreneurial phenomena through their particular, disciplinary lens. This is accompanied by infrastructure development that facilitates intradisciplinary dissemination and debate. For example, *Applied Psychology: An International Review* recently published a Special Issue on entrepreneurship. This is definitely a positive development because coopetition within the discipline may be a prerequisite for really good disciplinary research on entrepreneurship to result. There are one or two catches, though. We would probably not like to see these disciplinary subcommunities become completely isolated from each other,
and we would probably like them all to embrace the same basic notion of entrepreneurship as the “creation of new economic activity.” If they do not, they are not working in the same distinct domain and may therefore be little helped by each other’s efforts.

Richer, Better, and More Varied Data

One of the most exciting and unambiguously positive current developments in entrepreneurship research is the evolution of the data scene. Increasingly, studies use large-scale, longitudinal, and often multilevel data sets of a scope and quality early entrepreneurship researchers could only dream of (e.g., Amaral, Baptista, & Lima, 2011; Bakker & Shepherd, 2015; Campbell, 2005; Coad, Frankish, Roberts, & Storey, 2013; Sørensen, 2007). Moreover, entrepreneurship-specific data sets like the Global Entrepreneurship Monitor (GEM) and Panel Study of Entrepreneurial Dynamics (PSED) (including its successors and counterparts around the globe) are ever growing, public-domain resources that have provided the empirical basis for hundreds of published studies (Bergmann, Mueller, & Schrettle, 2014; Davidsson & Gordon, 2012; Davidsson, Gordon, & Bergmann, 2011). While these data sets are subject to the modest majority “problem” their sheer—and increasing—size makes it a gradually more feasible prospect to gain insights from them into the generative mechanisms behind the emergence of the select minority of “gazelles” or “unicorns” that populate the right tail of empirical distributions (Crawford et al., 2015). GEM provides only scant and cross-sectional information on the microlevel, but due to the increasing number of countries and years it lends itself to increasingly interesting aggregate-level analyses, especially when combined with country-level data from other sources (Acs, Autio, & Szerb, 2014; Levie, Autio, Acs, & Hart, 2014). PSED-type studies are being conducted in an increasing number of countries and recently a five-cohort data set with longitudinal, venture-level data was made publicly available (Reynolds, Hechavarria, Tian, Samuelsson, & Davidsson, 2016).

As regards the special case of data on “entrepreneurial culture”—prevailing mindset, attitudes, and beliefs—I can recall back in the 1990s having to collect primary data from representative samples of individuals in order to obtain any data of this kind (Davidsson, 1995; Davidsson & Wiklund, 1997). Today, researchers can combine country-level entrepreneurship data with culture data from the World Values Survey (Hechavarria & Reynolds, 2009) and even on the regional level amazingly rich “mentality” data are available in some countries (Obschonka et al., 2015; Stuetzer, Obschonka, Brixy, Sternberg, & Cantner, 2014).

Digitization, the Internet, and social media mean that all kinds of electronic traces are left which can serve as unobtrusive data sources for creative researchers (e.g., Aggarwal & Singh, 2013; Fischer & Reuber, 2011). Further, phenomena like incubators, accelerators, start-up weekends, and crowdfunding platforms provide new sources from which to sample and collect data on start-ups (Amezcua, Grimes, Bradley, & Wiklund, 2013; Mollick, 2014). Some of these contexts may provide excellent opportunities to apply the “catch early and follow-over-time” methodology pioneered by the PSED to more homogeneous and higher-potential start-up cohorts (Davidsson & Gordon, 2012). For creative investigators willing to put in some work there seem to be endless opportunities for finding new types of data that can help address novel questions and generate new insights.
More established approaches like experimentation and simulation are also being increasingly applied to entrepreneurial phenomena (e.g., Grégoire & Shepherd, 2012; Keyhani, Lévesque, & Madhok, 2015). This too contributes to getting a more complete illumination of entrepreneurial phenomena from a variety of angles on multiple levels of analysis.

The one exception from the positive data trend would be the collection of primary data from representative samples through surveys. When I conducted my above-mentioned studies on culture and entrepreneurship in the 1990s in Sweden, I attained a 70% response rate in mail surveys directed to the general population on a topic that would not necessarily interest them. Barring mandatory government surveys, this level of cooperation is impossible to achieve today from any sample, anywhere, on any topic, by any mode of survey data collection. Similarly, when we started data collection for the Comprehensive Australian Study of Entrepreneurial Emergence (CAUSEE) study in 2007 (see Gruenhagen et al., 2016) it was still possible to use landline phone numbers for sampling without severe bias. Not so today (Steffens, Tonelli, & Davids-son, 2011). Much more fragmented and diverse use of communication technologies increases the challenges of building a sampling frame in the first place, and the proliferation of telemarketing and spamming has made audiences much less cooperative.

Being more concerned with the theoretical suitability of the sample than whether it exactly matches the empirical population in a particular place at a particular time (see Davidsson, 2016c, Chapter 5) I do not think it is a major problem that we no longer have a single mode of contact that works “for all” as long as researchers invest enough effort into constructing a theoretically sound sampling frame. However, the sharply reduced rate of voluntary participation is worrying.

The “improvements” in survey methodology that have occurred—how easily and cheaply surveys can be created and distributed online, and the presence of panels of individuals who are willing to fill out surveys “professionally” for peanuts—only serve to increase those worries. What all my rather comprehensive experience in survey research tells me is that, in order to get business founders to (repeatedly) provide quality data, you need to talk to them and build rapport with them, which means that the interviewer must have some business knowledge. Further, you need to show respect for them and their business, which means being willing to call back when it suits them, and having an advanced, computer-aided questionnaire design which remembers previous answers through skip patterns and adapted question wording or response alternatives (Gruenhagen et al., 2016). It is a lot of work but that is also why serious survey data are—literally—worth infinitely more than some pay-per-click survey responses received from a largely self-selected sample of unknown composition invited via email or other nonprobabilistic, electronic means.

The Quest for Increased Theoretical Precision

Early entrepreneurship research was to a large extent characterized by relatively atheoretical “mapping of the territory,” that is, finding out what the empirical realities of entrepreneurial phenomena look like. This is not synonymous with bad or primitive research but

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3 Other than for GEM, where correct statistical representation is essential for country comparisons (Steffens et al., 2011).
rather a necessary step in sound knowledge development. One cannot effectively build or test theory about a poorly understood phenomenon (Hambrick, 2007; Locke, 2007) and still today we may sometimes jump too quickly to large-scale theory testing without sufficient, close-up familiarity with the phenomena (Dimov, 2011). Hence, we should not dismiss contemporary efforts that aim primarily at empirical fact-finding, either.

This said, a field of research cannot forever remain in a state where empirical fact-finding is the main game. For example, one of the findings from the 1990s Business Dynamics in Sweden study which really hit the media was that “seven out of ten new jobs are created by small firms” (Davidsson, Lindmark, & Olofsson, 1994). An empirical fact like that is not particularly exciting for people outside that time–space context. At the end of the day it is the sense-making of theory that makes empirical facts travel through space and stand the test of time (and today we know it is “new firms” rather than established “small firms” that create the job surplus; see Haltiwanger, Jarmin, & Miranda, 2013). Further, even within the context, a sound theoretical understanding is needed in order to take some action on the revealed information.

There is widespread perception that emphasis on theory in published entrepreneurship research accelerated around the year 2000. To substantiate this point, I checked the main body text occurrences of “theory” and its derivatives in the first five, regular-issue manuscripts published in Journal of Business Venturing in 1995, 2000, 2005, 2010, and 2015. The results are displayed in Figure 1.1.

It is interesting to note that in the year 2000, it was still not unusual to have articles with no mention of theory. Further, it may be noted that the minimum number of mentions of theory in the first five articles of 2015 exceeds the maximum number in 1995 and even in 2000. In all, this admittedly small test clearly indicates that the increase in

![Figure 1.1](image_url)
The Quest for Increased Theoretical Precision

theory emphasis is substantial and still ongoing. For the reasons stated above, this is in many ways sound development. However, theorization does not automatically lead to true and correct understanding. Theoretical storytelling may seem to make sense, but so can pure fiction. To arrive at a really sound theoretical understanding of phenomena requires much more than rhetorical skill and statistically significant results. I would argue that a number of factors coalesce to create a strong need for increased theoretical precision in future research in entrepreneurship. For example:

● Increased theorization implies increased level of abstraction, which calls for greater attention to conceptual clarity (Bacharach, 1989; Suddaby, 2010).
● The same increase in abstraction also increases challenges of operationalization (Davidsson, 2016c).
● Increased sample sizes and data quality (Edwards & Berry, 2010) and growing understanding of the limitations of statistical inference (Bettis, 2012; Bettis, Ethiraj, Gambardella, Helfat, & Mitchell, 2016; Schwab, Abrahamson, Starbuck, & Fidler, 2011) make achieving mere “statistical significance” for a directional relationship trivial or relatively meaningless.
● Increased pressures for practical relevance (Frese, Rousseau, & Wiklund, 2014; Gulati, 2007; Khazragui & Hudson, 2015; McKenna, 2015) also make “statistically significant effects” rather meaningless without further information on the absolute and relative size of these effects.
● Increased realization of the importance of context calls for further exploration of how and why empirical relationships vary across countries, cultures, regions, industries, and time periods (Welter, 2011; Zahra, Wright, & Abdelgawad, 2014).

These issues are discussed below.

Abstraction, Conceptual Clarity, and Operationalization

A legitimate field of inquiry ought to achieve clarity about its most central concepts. I will here use the notion of entrepreneurial opportunity to illustrate the need for greater conceptual clarity. In the last couple of decades this concept has become one of the most central in our field; “opportunity” even has a central role in the ENT Division’s domain statement (Mitchell, 2011).

The popularity of the notion of opportunity is understandable. If our main task is to study the creation of economic activities we need a starting point that precedes the up-and-running business, which makes the “opportunity” a candidate. Further, on an abstract, aggregate level the existence of opportunity can be said to follow directly from the theoretical assumption of disequilibrium. On this aggregate level it may even be possible to meaningfully measure the available (relative) “amount” of opportunity (Anokhin, Troutt, Wincent, & Brandyberry, 2009). To this we can add that the notion of “opportunity” is intuitively appealing and therefore hard to avoid in a lay conversation about entrepreneurship.

When we move to the micro-level and let time, change, and the possibility of failure enter the picture the notion of “entrepreneurial opportunity” becomes a conceptual mess, making it unfit for research purposes. Hansen, Shrader, and Monllor (2011) found no less than six distinct meanings of “opportunity” in the literature. My own deep dive into the topic revealed that only a minority of researchers define the term and that
inconsistencies in meaning flourished not only across but also within works, even when a definition was offered (Davidsson, 2015b, 2016a, 2016c). An “opportunity” sometimes refers to the entire set of external circumstances that make (the success of) an entrepreneurial venture possible. At other times it means a single external circumstance—a new technology or a regulatory change, for example—upon which some profitable business might be built. Often it refers to a founder’s subjective idea, which may be rudimentary and vague or fleshed out in great detail. The favorability which makes the entity earn the “opportunity” label may be theoretically assumed, empirically proven by a positive outcome, a (possibly delusional) belief on the part of the entrepreneurial agent, or not justified in any manner. When we look more closely, it becomes clear that whether an entity deserves the opportunity label or not depends on who is supposed to act on it, who is doing the evaluation, where in the entrepreneurial journey we find ourselves, and what other agents do.

No wonder, then, that there has been limited progress, due in part to failure to measure these elusive and hopelessly complex entities (Shane, 2012). A field cannot rely on central concepts of this nature because we cannot develop meaningful theory based on a concept that apparently cannot be clearly defined and consistently applied even within a given paper. I have outlined elsewhere how I believe the several important phenomena associated with “opportunity” can be more effectively approached through three separate and clearly defined concepts: external enablers, new venture ideas, and opportunity confidence (Davidsson, 2015b; 2016c). This triplet makes clear distinctions between the agent and the object acted upon, between the objective and the subjective, and between the contents and the favorability of that entity. Like Suddaby (2010), I believe we need to be much more careful in defining our central concepts, explicating their essential properties, and locating them in time and space. “Entrepreneurial opportunity” is but one example of this challenge.

On a more self-critical note, I have enjoyed an amazing number of citations for my coauthorship of the article “The Role of Social and Human Capital Among Nascent Entrepreneurs” (Davidsson & Honig, 2003). One reason for its popularity is probably that it was one of the first in entrepreneurship to use the theoretical concept human capital (HC) rather than just discussing effects ascribed to empirical variables like “experience” and “education.” In some ways applying the more abstract notion of HC and using already gained theoretical insights surrounding it is no doubt an improvement. But it comes with some responsibility. When we discuss possible effects of “education” or “industry experience” and measure these variables as the number of years of involvement in these activities, there is a high degree of correspondence between what we discuss conceptually and what we measure empirically. How about “human capital?” In the early days it was an aggregate (population) level concept used by economists and sociologists (Becker, 1962; Coleman, 1988; Schultz, 1961). At that aggregate level, the population’s average number of years of formal schooling can be a pretty accurate indicator of the country’s level of human capital (although it is not the only indicator being used). At the individual level, however, even with a rather narrow HC definition like “skills and knowledge that individuals acquire through investments in schooling, on-the-job training, and other types of experience” (Unger, Rauch, Frese, & Rosenbusch, 2009) the correlation between the theoretical variable and the empirical measure “years of education” can be quite small, considering the variance in school quality, inherent talent, and effort.
What does that imply? A positive relationship between human capital—knowledge and skills—and entrepreneurial success is almost definitional. When Unger et al. (2009) find that HC explains about one percent ($r=.098$) of the variance in success, should we really believe that? Or should we rather conclude that the measurement of HC in most studies is rather weak? In the worst case scenario, Davidsson and Honig’s (2003) failure to find support for the hypothesis that HC is positively associated with establishment of a viable firm only shows that the measures were too weak, or the sample too small, to confirm an effect that is bound to exist. Unger et al. (2009) probably provide a more important contribution by finding that the relationship is stronger for more direct measures of knowledge and skills than for indicators like years of education and experience. This suggests that if we want to test the theoretical effects of HC, we need to be more careful in our measurement of it.4

Issues of conceptualization and operationalization also pertain to the dependent variable (DV). If our main task is to study the creation of new economic activities, then our most important DVs concern entry into, progress, and success in that journey. For starters, this calls for clear conceptualizations of the start and end points of the process, which is no trivial task (Davidsson, 2016c; McMullen & Dimov, 2013; Schoonhoven, Burton, & Reynolds, 2009). I have come to the conclusion that the starting point should be defined by a combination of intentionality and action (Katz & Gartner, 1988; McMullen & Dimov, 2013). Mere intention without action is clearly unsatisfactory while including actions undertaken before there was an intention to start a business risks leading to something of an infinite regress. While the latter type of actions may create resources that benefit a start-up I would argue against viewing them as part of the venture creation process (Davidsson, 2015a). A venture creation process commences when an intention to create a new economic activity is backed by action towards realizing that intention.

As regards the end point of the process there are several alternatives. A main problem in the literature is the tendency to theorize drivers of success while using continuation of the start-up effort as operationalization (Davidsson & Gordon, 2012). This major misalignment of theory and operationalization leads to incorrect conclusions. Continuation of a doomed effort only represents waste of resources, which is about as far from success as one can get. However, rather than discarding continuation as a bad operationalization I would argue that continuation and success are two theoretically distinct concepts with, in part, different antecedents and consequences, which are both worthy topics of study. In Davidsson (2016c, Chapter 7) I elaborate on a string of conceptually distinct DVs from start to finish of the entrepreneurial journey. On the individual level there is intention, engagement, persistence and success whereas on the venture level there is initiation, continuation, emergence success and business performance.5 Today I would probably wish

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4 In fairness, many of the studies included in Unger et al.’s (2009) meta-analysis probably did not have their theoretical focus on HC relationships but included HC indicators as control variables. However, this only reflects the widespread lack of appreciation of making reproducibility rather than single-study statistical significance the central truth criterion (Hubbard & Lindsay, 2013a; Open Science Collaboration, 2015). If we really wish to build solid knowledge about the role of human capital (or something else) in entrepreneurship we need multiple, serious replications of entire theoretical models based on strong operationalizations of the core concepts, not just meta-analyses of zero-order correlations built on simple indicators. The surge in meta-analyses is part of a promising start toward building more solid evidence, but not the optimal or ultimate solution (see also O’Boyle, Rutherford, & Banks, 2014).

5 See also Davidsson (2012) and van der Zwan and Thurik (Chapter 2).
to add yet another distinction. One important transition is from the preoperational state to becoming a regular participant in the market, that is, achieving \textit{operational status}. Nonetheless, many may trade for a long time before they achieve \textit{emergence success}, which I define as generating a cumulative surplus exceeding all start-up and running costs; or they may never reach the latter status. This motivates distinguishing both theoretically and empirically between the achievement of operational status and emergence success, respectively.

Entrepreneurship researchers are not alone in struggling and sometimes being a bit careless with their dependent variables (see, e.g., Miller, Washburn, & Glick, 2013). However, this is clearly an area where increased precision is called for. What could be more important than the conceptualization and operationalization of the very phenomenon that we are trying to explain?

\textbf{Sample Size, Data Quality, Statistical Significance, and Practical Relevance}

So far I have discussed the need for clear concepts. The increased precision we now shift to concerns the relationships among concepts. I have noted above the delight we should take in the improved data now available to entrepreneurship researchers. Further below I will discuss the increased pressures to demonstrate impact on practice. Somewhat paradoxically, access to large-scale, high-quality data in combination with increased demands for practical relevance also drives increased demands for theoretical precision. The current mainstream culture accepts theoretical arguments leading to hypotheses of the kind “\textit{X} has a positive (or negative) effect on \textit{Y},” where the criterion for research success is statistical significance at a defined level, usually $p < .05$, which should mean “less than five percent risk of a false positive if no such effect exists in the underlying population.” Little attention is paid to effect size, that is, the actual magnitude of the estimated effect.

In a world of small samples and/or large random measurement errors, this approach may make some sense (or represent the least bad we could reasonably hope for) because the estimate is known to be imprecise, yet it is also known that for a result to come out significant the real-world effect would have to be rather large.\textsuperscript{6} If we have population data, significance as truth criterion does not make any sense. The “estimate” \textit{is} the population parameter; it is not surrounded by any statistical uncertainty of the type statistical inference can help us assess (let alone resolve). If we have a very large probability sample, any tiny effect will come out as “statistically significant.” Unless the effect concerns a small but real effect on saving lives—and it rarely does in entrepreneurship research—it is hardly of much interest that an effect of minuscule magnitude is unlikely to be completely absent in the underlying population when in fact it only explains a negligible fraction of the variance in the DV in the sample. If we have a very large sample that we can argue is theoretically relevant but which is not the product of probability sampling or random assignment, statistical significance testing again has no meaningful role. In all these cases, our first concern should instead be \textit{effect size}, that is, the magnitude of the estimated effect. Thus our theoretical interest ought to be directed at the absolute and relative size of the effect, the statistical uncertainty of the effect sometimes being a secondary concern and in other cases a nonissue. This is also what practitioners

\textsuperscript{6} A caveat being that the result might be entirely driven by \textit{systematic} measurement error rather than real-world effect.
care about: How large is the effect compared to cost or compared to other ways of influencing the dependent variable?

Edwards and Berry (2010) note the increasing meaninglessness of merely confirming that “X has a positive effect on Y.” As remedies they discuss the following (and other) ways of increasing theoretical precision, thereby making hypotheses more interesting and meaningful:

**Expanding the null hypothesis**
This entails demanding theoretical predictions that a parameter not merely differs from zero but that it deviates from zero by some minimum threshold amount. This should be standard practice when using population data and nonrandom (but theoretically relevant) samples. In fact, attention to the magnitude of the effect should always be the primary concern.

**Stating predictions as comparisons**
This entails theorizing not just that the effect of X on Y is greater than zero, but either that “the effect of X₁ on Y is greater than the effect of X₂ on Y” or “the effect of X on Y₁ is greater than the effect of X on Y₂.” This type of prediction is occasionally seen in entrepreneurship (e.g., Naldi & Davidsson, 2013). Davidsson and Honig (2003) found reason to discuss relationships of this nature, for example that the importance of social capital relative to human capital seemed to increase the further you get into the entrepreneurial journey, and the same for specific relative to general forms of capital. Albeit not hypothesized, these exploratory observations may actually be more interesting and important contributions than some of the hypothesized (and conceptually near self-evident) results.

**Developing non-nil predictions**
This requires the theorist to specify a range within which an effect should fall in order to be consistent with the theory. This would oblige us to take note not just of “disappointingly weak” effects but also “suspiciously strong” ones. The latter may be driven by common-method bias in measurement (Podsakoff, MacKenzie, Lee, & Podsakoff, 2003), multicollinearity, or omitted variables in the model (Shugan, 2007) and hence not help increase our understanding of the phenomena under study.

**Specifying other than linear functional forms**
A linear relationship is sometimes a reasonable assumption within the range of variance represented in the data at hand, but conceptually it rarely makes sense to assume that each increment of an X-variable would have an equally strong effect on Y. For example, the first prior start-up experience or year of industry experience should be relatively more important than the fifth or the 19th. Nonlinear predictions occur with some regularity in the entrepreneurship literature (e.g., Kreiser, Marino, Kuratko, & Weaver, 2013; Senyard, Baker, Steffens, & Davidsson, 2014). However, it is probably more common that researchers induce nonlinearity through variable transformations undertaken for technical reasons without reflecting on the fact that this changes the variable relationship to a nonlinear one.

**Recognizing Context**
Several influential authors in entrepreneurship and management have recently pointed out the importance of context and the possible consequences of not paying sufficient attention to contextual variation (George, 2014; Johns, 2006; Welter, 2011;
Zahra & Wright, 2011; Zahra et al., 2014). Developing contingent predictions, which is another of Edwards and Berry’s (2010) recipes for more theoretically precise hypotheses, is one way of dealing with context. For example, in Chandler, McKelvie, and Davidsson (2009) we hypothesize and find that rationalistic hypotheses derived from Transaction Cost Economics hold up in resource-scarce environments but not in munificent ones.

Theorizing contingent relationships, often with a context-based moderator, is now the norm in entrepreneurship research. As a case in point, at the time of writing the most recent issue of the *Journal of Business Venturing* features six articles, five of which offer hypotheses. All five include some kind of contingent relationship. This indicates that entrepreneurship research has become relatively advanced in terms of considering context. There are some caveats, though. First, moderation effects are often quite small (Aguinis, Beaty, Boik, & Pierce, 2005). As discussed above, small effects are not necessarily of much theoretical or practical interest even if they are “statistically significant.” Second, contingent effects are less likely to replicate than are main effects (Open Science Collaboration, 2015). Third, whereas only a few moderating effects can be considered in any one analysis, it may be the case that the entire causal system differs by context.

The latter is at least implicitly considered in the other main approach to dealing with contextual variation that has become commonplace in entrepreneurship research, namely to restrict the empirical study to a single, narrow context. For example, Kapoor and Furr (2015) focus on the solar photovoltaic industry whereas Cliff, Jennings, and Greenwood (2006) center on law firms in the Greater Vancouver area. This has advantages in operationalization by allowing more precise, customized measures and in analysis by reducing problems of unmeasured heterogeneity (Davidsson, 2016c). The obvious downside is that our main interest is not in those particular industries and regions. Academic interest is usually directed at a broader, conceptual category of entrepreneurial phenomena whereas practitioner interest typically concerns some other particular place and time (Davidsson, 2002).

This means that both types of stakeholders should embrace replication. While theoretical concepts often remain relevant across time and space, their interrelationships are likely to show considerable contextual variation. If we want to become a more advanced field of research by developing more solid knowledge that has greater practical impact, then replicability rather than single-study statistical significance should be the main truth criterion (Davidsson, 2016c, Chapter 9; Hubbard & Lindsay, 2013a, 2013b). It is therefore encouraging to see the launch of new outlets like the *Journal of Business Venturing Insights* and *Academy of Management Discoveries* explicitly invite replication studies. For journal editors who are nervous about replication studies not getting cited it may be interesting to know that the few explicit replication studies in entrepreneurship that I know of tend to do far better than the average same-year, same-journal article in terms of citations (Dahlqvist, Davidsson, & Wiklund, 2000; Frank, Kessler, & Fink, 2010; Honig & Samuelsson, 2014; Obschonka, Andersson, Silbereisen, & Sverke, 2013; Weismeier-Sammer, 2011).

### Increased Demands for Practical Relevance

Early entrepreneurship research was arguably driven mainly by an interest in important economic phenomena that were neglected by policymakers as well as by mainstream academic discourses (e.g., Birch, 1979). With this strong focus on the phenomena, and
the need to engage in exploratory, empirical fact-finding about them, it was quite easy and natural to produce research that would appear relevant and accessible for practitioners, at least on the aggregate, policy-oriented level (e.g., Delmar & Davidsson, 2000; Reynolds, Carter, Gartner, & Greene, 2004).

Over time, a large portion of entrepreneurship got absorbed into the culture and incentive-system of North American, business school research. This arguably brought increased theoretical and methodological sophistication to the field but also a sometimes excessive, singular focus on theoretical contributions in top tier, peer-review publications. As a result, entrepreneurship research also became part of the ongoing and sometimes self-critical debates regarding “rigor versus relevance” (Frank & Landström, 2015; Frese et al., 2014; Gulati, 2007) and “the research-practice gap” (Bansal, Bertels, Ewart, MacConnachie, & O’Brien, 2012). Researchers—in particular those who study economic phenomena (see Harzing, 2005)—respond to incentives, and as long as these rather singularly focus on impressing one’s peers the rigor–relevance debate is likely to have but marginal impact at the fringes. Just as increased access to research funding is likely to have played a significant part in the quantitative growth of the field to date, the threat of funding drying up will likely have stronger effects on redirection toward practical relevance than have any self-evaluative debates among academics.

This is what is happening; in the UK, the Research Excellence Framework now explicitly requests academic institutions to provide evidence of the impact of research on business or policy practice (Khazragui & Hudson, 2015; McKenna, 2015) and other countries are following suit by adapting their research evaluation systems (Morgan, 2014). The mechanisms may be different in the US, but there is no doubt in my mind that if business school research is being perceived as detached from practice the funding will dry up, be it donation based or somehow derived from taxpayers’ pockets. As I see it, this would also be fair enough. Management and organization, including the entrepreneurship branch of these fields, are fundamentally applied areas of research. If they were to develop solely into domains for academics’ exchange of ideas there would be little reason for governments (i.e., taxpayers) and donors to fund it. This said, I think there are some points that need to be considered in any discussion of these issues:

- **The illusion of relevance.** Simple, empirical fact-finding may appear accessible and relevant, but it may also be an example of overly simplistic research that does not help anyone draw the right conclusions. I found reason some years ago to seriously discuss the harmful impact that bad entrepreneurship research can have on practice (Davidsson, 2002) and this type of research is hardly what we would like to see increased. In this sense, there is no contradiction between rigor and relevance; practice can only be served well by good research, and doing good research requires some rigor.

- **The illusion of irrelevance.** As a field of research grows and matures, the topics of individual articles may seem increasingly narrow and esoteric from a practitioner’s point of view. However, to some extent this may be a levels fallacy. At early stages it may be possible to address broad questions in single papers. At a more advanced stage, the contribution of the single paper may be restricted to the influence of some moderator variable or some methods detail. This does not mean that the field as a whole is becoming less relevant. Entrepreneurship research undoubtedly has more relevant insights to offer practice today than it had 10 or 20 years ago, and is also likely to produce
much more new, useful knowledge per annum than it did back then. It is rather the demands on the research translation function (Shapiro, Kirkman, & Courtney, 2007) that increases as the field becomes more theoretically and methodologically sophisticated, making individual papers’ contributions more narrowly focused.

- **The illusion of separation and detachment.** Many entrepreneurship researchers do in fact engage in practice on a regular basis. They may be directly involved in business start-ups, invest in them, consult for them, sit on their boards, or provide advice in other capacities. They may, like myself, regularly interact with policymakers on a face-to-face basis and conduct contract research as an input to policy decisions. They may write practice-oriented reports, magazine articles, and blog posts. At the very least they influence practice indirectly in the classroom. The question here is whether this engagement sufficiently serves the purposes of implementing research-based insights into practice or of canvassing practice for problems to address in academic research. Sometimes one can get the impression that researchers themselves (subconsciously?) do not fully believe in the practical value of research, and then the two activities can become unduly separated.

- **It is a two-way street.** I am not under the impression that business practitioners show a high level of professionalism in terms of keeping up to date with research-based insights of potential value to their professional conduct. In this regard, business people behave differently from how we hope and believe that, for example, pilots and medical doctors behave (Romme, 2016). Then again, business schools share a lot of responsibility for this lack of professionalism by not providing students with the skills and mindset necessary for making the keeping up with developments in research a natural part of their professional identity. Notwithstanding this, as an individual academic I have reason to ask “If I provide a practitioner with good, research-based advice and they do not act upon it, why should the failure to have impact be attributed to me?”

- **Relevance for whom and impact on what?** Regardless of whether the funding comes from taxpayers or other sources, it is hardly the duty of academics and universities to unquestioningly assist particular people or organizations in their quest for profits (or other goals) or to provide proponents of particular ideologies with arguments to help their electoral success. Apart from defensible forms of contract research, the goal should rather be to benefit “society-as-a-whole,” which requires consideration of all stakeholders affected by the creation of new economic activities. If researchers agree on that principle and (a) try to stay true to it and (b) interpret its implications somewhat differently, this is probably a good thing.

Despite all these reservations, there are sound reasons for increased pressures towards practical relevance and impact. Practitioners—and educators—have dramatically changed their business start-up toolbox in the last decade or so. Foremost among the newly adopted tools are the models, language, and recipes offered under the rubrics of Lean Start-Up and Business Model Canvas (Blank, 2013; Osterwalder & Pigneur, 2013a; Ries, 2011). These are nonacademic, semi-academic or—in the case of Osterwalder and Pigneur—products of a different research tradition than the dominant track in business schools. Many of the ideas they propose on frugality, incrementalism, and adaptation have parallels in leading themes in entrepreneurship research, such as effectuation (Sarasvathy, 2001), bricolage (Baker & Nelson, 2005) and a critical view on business planning (Honig & Karlsson, 2004). These strands of research have enjoyed an above-average impact on practice and teaching in their own right, and they may have facilitated the uptake of
the even more popular ideas offered by Blank (2013), Ries (2011), and Osterwalder and Pigneur (2013a and 2013b). Yet, the latter arguably win the volumes game quite easily. This raises the question of whether future business school research on entrepreneurship ought to leave more room for “design school” research that—in an engineering-like manner—aims at developing practical “tools” and other primary types of outputs rather than journal papers (Davidsson & Klofsten, 2003; Osterwalder & Pigneur, 2013b; van Burg & Romme, 2014). If we were to reinvent business research from scratch today, we might actually find it odd not to have more of that perspective represented.

Conclusion

In this chapter I have discussed some likely and more or less desirable current and future developments in entrepreneurship research. My selections and conjectures are no doubt biased, and readers may therefore want to consult also some other gazes into the crystal ball (Carlsson et al., 2013; Choi & Majumdar, 2014; Clarke & Cornelissen, 2014; Shepherd, 2015; van Burg & Romme, 2014; Wiklund et al., 2011; Zahra & Wright, 2011). At any rate, it is safe to say that entrepreneurship remains an important societal phenomenon that is not yet fully understood, and that entrepreneurship research remains an exciting field in which to be active.

References


References


