

Bridges to the future

Challenging the nature of entrepreneurship scholarship

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From outlier to mainstream

Examining the compilation of chapters in this companion leads to a sweeping conclusion that we believe entrepreneurship scholars should celebrate: the good news is that entrepreneurship research does not have a distinctive domain. We mean this both theoretically and descriptively. Not only do the contributors represent the juxtaposition of an extraordinary array of perspectives and compelling theoretical questions, their work privileges an equally extraordinary array of empirical settings. Entrepreneurship itself in all of its messy (Gartner 2004), contradictory fecundity has begotten a field of research that still clamours to reflect it adequately.

On the other hand entrepreneurship research has developed what we might call a 'mainstream', characterized by a reasonably coherent set of questions and buttressed by a set of relatively consistent (ontological, epistemological, methodological and theoretical) assumptions. This is good news in the sense that such *almost-paradigmatic* characteristics have probably helped elevate entrepreneurship research from its roots in the applied field of 'small business research' (Blackburn and Kovalainen 2009; Blackburn and Smallbone 2008; Schmude, Welter and Heumann 2008) to something more closely resembling a legitimate academic field in which career-oriented academics can hope to achieve tenure, promotion and other rewards that are contingent on recognition of their work by colleagues outside of entrepreneurship. Before we expand on our celebration of the continued messiness demonstrated by the scholarly range of this volume, we briefly examine some of the reasons that a mainstream has emerged.

First, at risk of too greatly oversimplifying, we would argue that this mainstream traces a path carved by economists who in turn influenced the management/strategy scholars who have been the primary teachers of entrepreneurship researchers (Baker and Pollock 2007). Buttressed by strongly held (though contestable) beliefs surrounding entrepreneurship as a primary driver of job growth, innovation and economic development (e.g., Birch 1979) much of this work focuses on questions of relative financial and economic performance much like those favoured by strategy scholars. Indeed, a group of leading scholars straddling the boundaries between entrepreneurship and strategy have promoted the field of 'strategic entrepreneurship' largely in terms of concern with 'wealth creation' as an outcome (Hitt et al. 2001) and have created a journal around this.

Second, in recent years, the mainstream has been coloured by what many entrepreneurship researchers refer to simply as ‘Shane and Venkat’, also known as the *Academy of Management Review* note, ‘The Promise of Entrepreneurship as a Field of Research’ (Shane and Venkataraman 2000), which elaborated on insights from Venkataraman’s (1997) earlier chapter, ‘The distinctive domain of entrepreneurship research’. By late 2013 these two publications had garnered over 7,500 citations in Google Scholar. The current generation of young faculty doing entrepreneurship research has been strongly influenced by Shane and Venkataraman’s framework, and the framework has also created tributaries into the entrepreneurship mainstream for scholars from a wide variety of backgrounds migrating to – or at least visiting – entrepreneurship research. Thus, there has been an astounding proliferation of papers that include some variant on the phrase, ‘we define the field of entrepreneurship as the scholarly examination of how, by whom, and with what effects opportunities to create future goods and services are discovered, evaluated and exploited’ (Shane and Venkataraman 2000: 218).

Importantly, Shane and Venkataraman’s work has had the effect of focusing attention on the notion of opportunity. Their follow-on claim that opportunities are objective phenomena that are simply ‘not known to all parties at all times’, because the recognition process is subjective has generated an extended debate about the nature of opportunities, initially hinging primarily on the distinction between ‘discovery’ versus ‘creation’ or ‘construction’ of opportunities (see Chapter 26 in this volume; also: Alvarez and Barney 2007, Edelman and Yli-Renko 2010, McMullen et al. 2007). Shane and Venkataraman were explicitly attempting to distinguish entrepreneurship from other scholarly fields, especially strategic management, stating that ‘although a conceptual framework to explain and predict relative performance between firms is useful to strategic management, it is not sufficient for entrepreneurship’ (Shane and Venkataraman 2000: 217). Interestingly, again perhaps because of the dominance of economics-influenced strategy programmes in training entrepreneurship scholars, much of the research coloured by their framework has nonetheless been focused on the traditional economic performance outcomes favoured by strategy scholars.

Third, some entrepreneurship research has become much more methodologically sophisticated (see Chapters 27, 28, 29 and 30 in this volume). There are several reasons why entrepreneurship research may be hard to do in a rigorous way. One challenge is the Shane and Venkataraman framework: arguably, the demonstration that an opportunity ever existed requires ex-post evidence that some entrepreneur discovered and successfully exploited it. Scholars adopting this framework therefore face a knotty methodological challenge in distinguishing between the effects of lack of opportunity, lack of discovery of opportunity and lack of skilled exploitation of discovered opportunities. Another well-known problem is that compared to the large public firms favoured in research in most other business disciplines, the nascent and young firms that are the focus of much entrepreneurship research leave few public or archival traces (see Chapters 27, 28 and 29 in this volume for approaches to dealing with this). Moreover, entrepreneurship is about human behaviour and human beings, their ways of acting and thinking. Whilst this itself does not distinguish entrepreneurship from management research more generally, it is the ordinariness and everydayness of entrepreneurship (see Chapters 11 and 15 in this volume) that makes the field distinctive as well as fascinating but also difficult to research: more difficult in part because of our tendency to focus on the ‘exceptions to the rule’, namely technology-driven, innovative and high-growth ventures.

Nonetheless, since Aldrich and Baker (1997) found themselves unable to detect much evidence of methodological progress in entrepreneurship the tide seems to have turned. As shown in this volume, researchers have increasingly brought sophisticated and clever approaches to studying entrepreneurship. The method chapters in Part G of this handbook illustrate that a variety of

cutting edge methods are now in use and help to make them broadly accessible to entrepreneurship researchers. Overall, this is very good news but with a caveat. On the one hand, methodological advances improve both the quality and the legitimacy of entrepreneurship research. On the other hand, in any social science, including entrepreneurship, methodological sophistication can become an end in itself (Elden and Chisolm 1993). Moreover, as areas of studies become characterized not only by particular theories but by particular methodologies, this can reinforce the conservatism toward keeping within the mainstream and toward making incremental contributions. Unfortunately, this is something we see too often in our refereeing and editorial roles as both authors and reviewers sometimes too easily take on the mantle of gatekeepers of received wisdom.

Fourth and finally, we need to follow the money. In a time of declining government support universities and schools of business have assiduously courted successful entrepreneurs and their businesses as donors. Such efforts are aided by and encourage the easy acceptance of 'heroic' images of entrepreneurs as portrayed in popular media (Achtenhagen and Welter 2011; Drakopoulou Dodd and Anderson 2007; Ogbor 2000). The stylized narrative, which seems to shape many of the research questions that mainstream entrepreneurship research poses, goes something like this: 'Idiosyncratic and brilliant individual discovers or creates highly lucrative opportunity and through brains and guts keeps it going, attracts equity investment, revolutionizes some industry, takes it public and delivers value, jobs, growth and capital accumulation'. While anyone doing empirical research – or even spending time casually with entrepreneurs – knows just how unusual this storyline is, it remains the aspirational background context of much mainstream entrepreneurship research, seducing researchers to a greater extent than it attracts entrepreneurs, whose motivations are far more heterogeneous and interesting than the narrow economic functions that our scholarship too often assumes and assigns practicing entrepreneurs (see Chapters 11, 16 and 17 in this volume; also: Powell and Baker 2014; Smallbone and Welter 2001).

The editors of this volume had the good fortune to enter the field during a time of transition. In the US, the early warriors who had carved out a niche for entrepreneurship research, taking on the burden of – or even glorying in – its illegitimacy were in the midst of transitioning, or of giving over leadership to scholars who sought at least enough legitimacy for the field to be able to squelch the practice of telling promising doctoral students that they 'should not study entrepreneurship because you cannot get tenure in that field'. Entrepreneurship was still characterized by the strutting sense of being on the frontier, with its 'anything goes' energy just starting to be put loosely into harness. In Europe, entrepreneurship research took off in the late 1990s, and several entrepreneurship scholars (initially) came from small business economics and management (Welter and Lasch 2008); these scholars pay or at least used to pay close(r) attention to contexts and their impact on entrepreneurship and some European scholars are still 'more likely to utilize ideas and methods from philosophy and humanities' (Gartner 2013: 8; also see Chapters 2, 3, 16, 22, 24 and 25 in this volume) – probably an advantage with regard to the relevance of entrepreneurship research if we consider relevance broadly. We will revisit this point later.

This transition of the entrepreneurship field was in many ways symbolized by the closing paragraph of Aldrich and Fiol's classic paper (1994: 666), titled 'Fools rush in? The institutional context of industry creation':

Finally, investigating these ideas will require expanding our disciplinary reach to take in anthropologists, political scientists, social psychologists, and others interested in understanding the genesis of contexts that give meaning to new behaviors. The social

construction of organizational reality involved in building a new industry requires meaning making on a grand scale, and we suspect that those entrepreneurs who do it well are obsessed with the process. As such, they make fascinating subjects of study.

That the paper was published in *The Academy of Management Review*, joining a very small handful of entrepreneurship papers to have ever been published in elite management journals at that time, was a reason for celebration in itself. But even in such a serious and sometimes almost solemn outlet, the authors' final phrase remained an unsuppressed exclamation of the joy that entrepreneurship scholars found in their work and in the subjects of their study. Rather than trying to delimit the field, the paper was a call, an invitation to a broad range of social scientists to help understand something viewed as very broadly important and cross-disciplinary. Retrospectively, the paper would appear to be saying that Shane and Venkataraman's attempt to delimit the field was wrong both as description and as prescription. The paper exhibited no fear that lack of a tightly bounded disciplinary domain was a source of harm or concern, thus paving the way forward to a handbook like this one, which aptly portrays the messiness of entrepreneurship, by illustrating the heterogeneity and variety of approaches to study something that is deeply fascinating and at the same time an ordinary and everyday phenomena. Indeed, a primary theme of Aldrich's work for over 30 years (Aldrich 1979; Aldrich and Ruef 2006) has been directed at trying to get scholars to pay more attention to 'ordinary entrepreneurs'. Somewhat to our surprise and to our great relief, it appears to us that many in our field are increasingly discovering why this message matters (see Chapter 15 in this volume, also Steyaert and Katz 2004).

Initially, we undertook our work on this volume with a sense of trepidation that the magic was gone, that much as Weber (2009) described 'the disenchantment of the world' through rationalization and its handmaiden bureaucratization driving the displacement of craft with technical professionalism and career structures, that perhaps institutional pressures and the promise of legitimacy, predictable career progress and money had raised the riverbanks around the mainstream high enough to make it appear the only reasonable path. As we now look over the work of our contributors, we can only chuckle at our earlier concern. The chapters in this volume show that the levee cannot hold. There is a mainstream in entrepreneurship research, but it is meandering and subject to an ever increasing set of forks; it often joins with the flow of research from other fields borrowing ideas and now and again contributing ideas back to other streams of work. In short, the range and quality of work and the range and incisiveness of the perspectives gathered in this book represent an exclamation of passion and (sometimes highly critical) joy that makes the early days of entrepreneurship research look downright sombre and constrained.

Creating a strictly defined domain for a new field is a legitimacy ploy (also see Chapters 2 and 30 in this volume). It should be, at best, a temporary heuristic to get a foot in the door, to be admitted to the club. But there is another ploy possible: growth. The ecologists' identification of legitimacy with density rings true in this case. Demands from students, donors and other university stakeholders have resulted in a rapid increase in the number of jobs for entrepreneurship researchers worldwide. This in itself has brought legitimacy. In addition, gradual improvements in theory and methods – both within the mainstream and outside its confines – have resulted in an increasing number of publications in elite journals. A proliferation of chaired positions in entrepreneurship initially resulted in many being filled by scholars whose primary identity had not been associated with entrepreneurship research. This is changing as more senior figures in entrepreneurship emerge but also as some universities fill chairs with more junior scholars than is typical in other fields.

Even more extreme, some US universities have reconsidered their criteria for tenure and promotion for entrepreneurship faculty compared to those in deeply established fields such as organization behaviour or human resource management, sometimes requiring a lower number of 'hits' in traditional elite journals from people in entrepreneurship. Moreover, we know of others, at least in the US, that are debating doing the same thing, based in part on the correct reasoning that there are simply not enough entrepreneurship researchers to go around, if a school sticks to traditional requirements along the lines of the 'one hit per year in a traditional elite journal' performance benchmark long common among elite schools in the US. Another outcome of these pressures is increasing mindfulness to the rapid improvement in the quality of leading entrepreneurship journals during the last decade and broader acceptance of these journals as elite outlets. Interestingly enough, several European countries only seem to have started on their journey towards a rigorous hiring regime based on publications, often pressured by government initiatives that couple research funding to impact measured by publications as is the case in, for example, Sweden, whilst others around the world (Australia amongst them) are already revisiting their prescriptive (and restrictive) ranking exercises.

It is easy to debate why there are not more entrepreneurship scholars hitting such 'one traditional "A" a-year' standards, with explanations ranging from those that celebrate the inadequately harnessed exploratory passion of young entrepreneurship scholars to probably-more-sensible attributions to the youth and breadth of the field and the resultant challenges of setting up and 'selling' theoretical contributions. The fact is that entrepreneurship scholars and entrepreneurship scholarship have 'arrived'. 'Real' entrepreneurship scholars whose work and whose scholarly identities are with the entrepreneurship division of the Academy of Management in the US and similar professional organisations around the world have continued to take on leadership positions in the most elite journals. Overall, whatever one's preferred attributions, it is hard to argue that entrepreneurship is not already 'adequately legitimate' or that its legitimacy is likely to be derailed. As more researchers gain tenure and promotion and more entrepreneurship papers get into top journals, as more and more schools in non-US and non-European parts of the world discover entrepreneurship as a core discipline (and the publish-or-perish process as their main route to promotion), more home-grown theoretical contributions and methodological innovations will occur and scholarly legitimacy will continue to build. We also find particularly encouraging what we see as the *Journal of Business Venturing* editor-in-chief's – Dean Shepherd's – increasingly successful mission to publish 'interesting' research, with 'interesting' defined very broadly (cf. Chapter 2). It is hard now to imagine suggesting to a promising graduate student that she or he avoid entrepreneurship as a dead end.

Based upon our supposition that entrepreneurship is adequately and increasingly legitimate and that it has a mainstream but still travels relatively unfettered, flooding and making fertile the alluvial plain through which it meanders, we will now go on to examine a series of important questions about the future of entrepreneurship, in each case suggesting that the appropriate answers depend in large part on what one thinks about the current state of the legitimacy of the field and therefore in each case arguing in favour of the same spirit of excited welcoming and openness that characterized the time when we came to this field.

Who should entrepreneurship research serve?

Like most social science research coming from schools of business, entrepreneurship research faces questions of its practical 'relevance': for whom is it useful other than for the increasingly global community of entrepreneurship scholars who read and evaluate one another's work in what some critics see as an increasingly 'ivory tower' theory-building exercise (Greenwood and

Levin 2005)? The Academy of Management and other organizations of business school scholars periodically experience paroxysms of existential dread during which themes of relevance come to the fore. To many of the warriors who had one foot firmly in the world of practice as they fought to create the field of entrepreneurship research, such developments can seem particularly egregious. We agree that if entrepreneurship research becomes of no use to anyone but career-minded entrepreneurship scholars, something important has been lost. But we want to question what we see as the often seemingly taken-for-granted and implicit assumption that entrepreneurship research should primarily serve narrowly defined economic interests. While we are all in favour of helping to find tools and answers that help entrepreneurs to be more efficient and effective in their decisions and behaviours, we think that contemporary discussions of 'relevance' are limited in several ways, a theme explored in the next sections.

Entrepreneurship as panacea

Mainstream entrepreneurship research is – still – driven by the assumptions that entrepreneurship is something good and valuable and that its value is derived narrowly from its economic contributions, although this latter idea is seldom made explicit. We suggest that it is worth questioning and reconsidering these assumptions, for two reasons.

First, if we accept them, we are sure to miss out on many interesting themes (also see Chapter 2 in this volume). Worse, we may also start losing our curiosity – and maybe our identity? For both of us, being a researcher is about being curious and, in a way, being and remaining non-mainstream, in part because we have had the chance to observe how this orientation has allowed the scholars we most admire to identify and develop novel and interesting ideas. For us, one distinctive feature of the entrepreneurship field is its 'relative newness, the openness of its boundaries and research vibrancy' (Blackburn and Kovalainen 2009: 141). But with the entrepreneurship field gaining legitimacy there has been a tendency to narrow down research questions and methods – many PhD students and their supervisors are risk averse, too closely following fashion and trends and too narrowly focusing on doing whatever will get them published (see Davidsson 2013 for a similar assessment, also Chapter 30 in this volume). Interestingly, we saw some of this 'narrowness' when assembling and reviewing the first chapter versions for this handbook: although we encouraged a bold stance towards established knowledge, wishing to see a focus on what is cutting edge and themes for the future, many of those trained within the past decade needed time and our repeated explicit 'permission' to get into the mood to be more bold and daring, questioning what we have taken for granted and illustrating possible future research avenues. We think the results of this book reflect the observation that our gentle nudges typically unleashed a robust underlying desire to challenge what is taken for granted in various areas of study and to imagine new directions.

Second, our research agendas and results also influence the public agenda (and they are influenced by public agendas where researchers have to go for the money), in particular when researchers communicate results to those outside the ivory tower and advise governments. Governments and support organisations, always in need of means to grow and develop their own economies, have eagerly picked up the mantra of 'entrepreneurship is something good', contributing to employment, innovation, general wealth and wellbeing. Consequently, for decades now entrepreneurship has been perceived as something of a panacea for all kinds of economic problems (also see Chapters 17, 22, 24 and 25). Name it, and entrepreneurship helps to solve it! Losing large businesses as happened during the 1970s? No problem, new business opportunities will arise for new ventures. Growing unemployment around the world during economic crises? Go and foster entrepreneurship programmes that push those with limited knowledge and resources

into opening their own businesses. Your economy has become less competitive and innovative? Again, look at entrepreneurship – that is the solution! New businesses bring in new ideas, and supporting high-growth businesses is the best way to leverage economic development. Women, immigrants, youth and disabled persons are excluded from the labour market? Get them to open their own business. A need to develop regions? Why not try entrepreneurship policies? There is something in it for everybody . . .

We do not mean to suggest that entrepreneurship does not contribute to economic development. Of course it does! But we also suggest, and are supported in this by several of the authors contributing to this handbook, that entrepreneurship is not something we can fit into one box. Nor does today's 'mainstream' model of how opportunities emerge and are exploited, preferably through an innovative, profit- and growth-oriented business, and how entrepreneurs behave, fit the global reality. Homo economicus is a rather thin and flimsy theoretical construct, while more and more research demonstrates the seemingly 'economically' irrational behaviour of individuals who set up ventures despite a pronounced lack of skills, knowledge, resources (see Chapters 5, 6, 7, 8 and 15 in this volume) and sometimes do it again and again (see Chapter 9). This puts a huge question mark against the dichotomies entrepreneurship researchers like to apply (not least because it makes their life easier as the messiness of entrepreneurial behaviour – or reality – is difficult to capture); opportunity versus necessity entrepreneurship, lifestyle or hobby versus 'entrepreneurial' businesses, small business versus entrepreneurial businesses, social versus regular entrepreneurship, and so on . . .

Take the former Soviet and post-Soviet countries as one example where entrepreneurs in initial periods of transformation towards a market economy did not play by the rules because those rules favoured large state-owned business, but where this, their rule-avoiding, rule-bending, sometimes simply illegal behaviour helped to create more economic value (private entrepreneurship, jobs, income) than would have happened had they played by the rules. There is an ethical/moral dilemma in this that is rarely discussed by entrepreneurship scholars (for a notable exception see Anderson and Smith 2007): doing good for the enterprise and, implicitly, for society, versus doing good for the economy. And how does such a longstanding 'legacy of non-compliance' (Feige 1997) impact on the longer term development of societies and economies? We suggest that there is much scope to further incorporate and study the heterogeneity of entrepreneurial motivations and behaviour and the outcomes of entrepreneurship with regard to individual, social and economic wellbeing considered in the broadest possible light (see Part E of this volume). While we share the optimistic bias of many entrepreneurship scholars, we wonder, for example, why there does not seem to be a robust research conversation among entrepreneurship scholars about topics such as the sorts of entrepreneurship – as part of global supply chains, for example – that leads to events such as the recent Bangladesh textile factory collapse? Surely, our research domain should be broad enough to encompass a range of questions regarding such conditions, events, behaviours and outcomes?

Ideologies and myths

Why is it that we still tend to focus on but one contribution of entrepreneurship? Blackburn and Kovalainen (2009: 141) draw our attention to the 'ideologies lurking behind the knowledge produced by research', which they consider as a crucial question for how to develop the field. Recently, scholars have started to revisit and challenge the myths and ideologies that persist in our field (Rehn et al. 2013; also see Chapters 16, 22, 24 and 25). One prominent example of such a persistent myth includes our identification of 'high growth/high potential' ventures as

the essential embodiments of true entrepreneurship (Achtenhagen et al. 2010, Kiviluoto 2013). If we are honest, of course, neither venture capitalists nor the ventures they fund need any help from us. The standard heroic imagery of 'big idea, generate investment, grow fast, exit' is both intellectually and theoretically boring, because these processes work quite well and are largely understandable without any contribution from us! Perhaps more disturbingly, seeking to be 'relevant' to the already rich and privileged who do not actually seek us out for new ideas risks leaving us positioned primarily as cheerleaders and apologists.

On the other hand, why has there been so little work on the struggles of 'normal' entrepreneurs? Those who struggle to compete at all, those without access to efficient markets for resources or to sell their goods, those facing numerous institutional 'voids' (Mair and Marti 2009), those millions who operate 'informally', those, in other words, that we might help? We are irrelevant to these entrepreneurs almost by definition. There are tens of millions of entrepreneurs struggling to make it, across an amazing variety of contexts, and we have basically nothing to say to them, other than labelling them disparagingly as 'lifestyle' businesses, small business owners or 'necessity' entrepreneurs.

We have chosen to align ourselves with studying and celebrating the tiny fraction of entrepreneurs who have little or nothing they need from us. And let's face it, highly paid, highly skilled, highly resourced consultants consistently do a better job of discovering 'what works' for high-profile entrepreneurs than do academic researchers – which is probably how it should be. In contrast, we find the recent development of research – and theories – that have some promise of being relevant to entrepreneurs and communities that can benefit from our work to be extraordinarily exciting. This includes, but of course is not limited to (and we admit that this is a menu coloured by our own experiences and assessment of what is relevant and exciting): work on community entrepreneurship (Chapter 17), on resourceful behaviour of entrepreneurs and informal entrepreneurship (Chapters 8, 10, 12, 13, 16, 23 and 25) and on entrepreneurship in 'other' regional and cultural contexts (Chapters 18, 19, 20 and 21).

The value of entrepreneurship – and of entrepreneurship research

There is a tendency to see the (entrepreneurship) world in black and white – understandable because it is easier to create measures and models from such dichotomies, but those on which much entrepreneurship research has settled do not reflect the 'real world' of the entrepreneur nor their real contribution to our lives, their own lives and wellbeing writ large. We suggest that scholars should embrace and – as evidenced in this companion – have embraced a much wider understanding of what constitutes wealth and wellbeing and the role of entrepreneurship. This goes hand in hand with our responsibility to also discuss the ethical and moral dimension of entrepreneurship and the many types of value it creates or destroys (Anderson and Smith 2007). From a macroeconomic point of view, new ventures and SMEs contribute to economic development through innovating and creating employment, whilst at the business level value creation is commonly reflected in the expansion and growth of an enterprise. At the same time, for the individual entrepreneur a variety of sources of satisfaction needs to be considered, including non-pecuniary ones, since her/his assessment of 'value' is likely to include 'personal' dimensions (Welter and Smallbone 2004). As Powell and Baker (2014) have demonstrated, the structure of founders' identities (also see Chapter 7 in this volume) may include a variety of chronically salient social and role identities and economically oriented 'keeper of the bottom line' identities may play but a minor role in overall motivations and satisfactions. While the large, generally

public firms may share a boring similarity of purpose, constrained by fiduciary duties towards equity and debt holders, the smaller and generally private firms that are the focus of much entrepreneurship research are varied and heterogeneous and, in many or most cases, not well understood when treated simply as smaller, earlier stage or immature versions of the corporate entities that are the traditional focus of much management research. The heterogeneity of goals and values expressed in the creation and nurturing of founder-run ventures requires models and theories that the study of large public firms has simply not demanded. Indeed, an important research frontier may be to explore how and why some founders may resist the pressures of institutional homogenization that render so many 'successful' firms so similar as founders' diverse goals and values are derailed.

Moving beyond the individual level, value creation depends on the perspectives we take and the contexts we analyze and from which we come. Can we claim economic value creation without considering what happens to our societies as follows from a narrow focus on entrepreneurship as job generation? Are not economic and societal value creation unavoidably intertwined: two sides of the same coin? While Baumol (1990) provided the field with its notion of 'negative' entrepreneurial value creation from an economic perspective, we believe that the creation of value and assessment of positive and negative implications of different sorts of entrepreneurship is far more complex and in desperate need of research. Neither positive nor negative naiveté serves either our scientific or our practical concerns.

The special privilege and responsibility of entrepreneurship research

For a long time, relevance seems to have been narrowly equated with publishing in major academic outlets: relevance has been seen as a contribution to the academic debate, and not just any contribution but those in mainstream journals. As argued in the preceding section, we believe that this narrows down our field and creates the risk that we become or remain irrelevant to the many varied stakeholders entrepreneurship research should and could serve. Isn't it interesting that recent calls for research with impact (from governments that want value for their research funding) and for evidence-based entrepreneurship (from scholars who always had kept a foot in the practice and are well versed in translating research results) comes at a time when the entrepreneurship field has achieved legitimacy in academia (Frese et al. 2012)? Early on, Davidsson (2003) insisted that entrepreneurship research had both a scientific and a practical social dimension. And, of course, many established entrepreneurship scholars with a more applied or practical background have continued communicating their results to practitioners and policy-makers outside the ivory tower (Blackburn and Smallbone 2008), thus maintaining their foot in both the academic and practice-oriented camps.

How can we best achieve relevance for those businesses and entrepreneurs our research could serve? For the most part, large firms do not turn directly to universities for help in determining their business strategies. Certainly, a handful of highly accomplished – perhaps even 'famous' – strategy scholars earn a great deal of money helping to shape the decisions of large corporations, but most management faculty play at best indirect roles in affecting corporate strategy making, mostly through their teaching or if – as occurs somewhat rarely – their research happens to be recognized as practically useful by corporate decision makers. Life is different for entrepreneurship faculty. It would be hard to find even the most junior entrepreneurship professor who is not inundated with requests for help from budding or operating entrepreneurs looking for free consultancy from the faculty member and students. Indeed, for faculty at research-intensive schools, managing and limiting responsiveness to demands for help from entrepreneurs can be

an important skill for 'safe-guarding' research time. Twenty-five years ago, many and perhaps most entrepreneurship faculty came from the ranks of practicing entrepreneurs, and perhaps still had one foot in practice and felt comfortable in the role of consultant or coach. Current junior faculty appear to have less practical experience on average, in all likelihood as a result of the shift towards academic hiring practices as the field continued to achieve legitimacy.

Partly due to a lack of qualified entrepreneurship researchers (and partly as an element of the overall decline in the percentage of university faculty holding tenure track appointments, at least in the US), many universities have relied on non-research-trained practicing or retired entrepreneurs to teach entrepreneurship classes. In addition, many universities, especially those with a public mission, have set up small business or entrepreneurship 'support' centres, mostly staffed by non-researchers. As a result, it is easy to observe a split on many large university campuses between people applying 'practical' lenses and those applying 'theoretical' lenses to entrepreneurship. Unfortunately, it is also commonplace to observe a mutual lack of respect among people on opposite sides of this divide, even when they share teaching responsibilities in the same programmes.

It strikes us that this represents a tremendous missed opportunity. Entrepreneurs tend to follow – at least to some degree – the advice given to them by entrepreneurship faculty. That is to say, unlike most business and strategy faculty studying large corporations, entrepreneurship faculty and university-based coaching and consulting staff are engaging in natural experiments – or some form of 'action research' if you prefer – whenever they engage with entrepreneurs and their firms. Importantly, much of the 'advice' that is proffered may be insightful, useful or correct but because of limitations in the state of the body of research-based knowledge in entrepreneurship, the advice that is given is frequently not 'evidence-based'. In other words, university faculty and staff may give advice to entrepreneurs that is based on personal insight but that they do not know from any theoretically generalized empirical basis to be right or wrong. For any problem or opportunity an entrepreneur may face, there are typically many 'bad' answers but also several potentially 'good' answers and the state of the art of our theory and research is not up to the task of deciding which answer is 'right' even in simple terms, never mind in terms of the multidimensional concerns across which the entrepreneur may be trying to optimize or satisfice.

We believe it is time to re-imagine universities' involvement in their entrepreneurial communities as an opportunity for research partnerships supporting theory-driven research with practically useful answers (also see Chapter 14). There are many ways this could be organized. For example, one approach is to mimic the university teaching hospital/medical school model in which basic research drives clinical research that is applied in clinical practice and provides feedback to both clinical and basic researchers. Subject matter experts (faculty and PhD students) train the clinicians (in this case, for example, MBA students), document the effects of the advice given to entrepreneurs in something like the form of 'clinical trials' and provide data that help to answer both the theory-driven questions important to basic research and the applied questions important to people outside the academy. Theory is improved through generating and harnessing field experiments/clinical trials, practical 'clinical' knowledge is improved and 'clinicians' enter practice, better trained from their prior experience applying theoretical and practice-based insights in a clinical setting.

We want to emphasize two points here, namely that on the one hand entrepreneurship faculty have the largely unexploited opportunity to conduct 'field experiments' with entrepreneurs who are eager to give informed consent to being part of a research programme; and that complementing this, entrepreneurship faculty have an ethical responsibility toward developing expertise allowing them to give better evidence-based advice than we give now.

The handbook

We structured the chapters in this handbook around six broad themes that we believe represent important undercurrents in contemporary and emerging scholarship: the discipline of entrepreneurship research (Part B, Chapters 2–4), reasons and motivations for entering entrepreneurship (Part C, Chapters 5–9), resources and resourcefulness (Part D, Chapters 10–14), entrepreneurship, wealth and wellbeing (Part E, Chapters 15–21), entrepreneurial opportunity (Part F, Chapters 22–26) and Part G with Chapters 27–30 on the methodological future of the field. Each of the six main sections begins with a brief introduction.

Where do we go from here?

This handbook brings together scholars from many countries and research traditions. But, the country of the current affiliation of our contributors masks one important point to which we would like to draw the reader's attention: the variety of countries of origin – in other words, the heterogeneity and breadth of experiences and different backgrounds that have been brought to this handbook. We believe that this variety and heterogeneity has become a distinctive characteristic of the entrepreneurship field – as someone said to us at a recent Babson Entrepreneurship Research Conference session, 'Being at these entrepreneurship meetings is like being at the United Nations'; similarly, the RENT conferences could be compared to (the positive aspects of) 'a small European parliament' – and we see this as a foundation for an exciting future for our field.

It is important to note, however, that this handbook remains dominated by scholars trained and operating in North American and European university contexts. For example, the handbook contains few African or Latin American voices, despite our efforts to involve a broader community of scholars. This is an important hindrance to the development of our field. At the recent second biennial African Academy of Management conference, a session on 'publishing from the periphery' addressed questions of whether African scholars should attempt to become part of the community publishing in 'elite' English language journals or whether it was more important to focus on local issues and publishing in local and regional outlets. Several people in attendance suggested that the rules and requirements for publishing in elite journals represented a form of neo-colonialism that should be resisted in favour of finding 'African solutions for African issues'. In the end, the strongest sympathies seemed to be around a strategy of both rather than either/or, but it strikes us that this conversation encapsulates a number of important ways in which context matters (also see Welter 2011; Zahra and Wright 2011).

First, while the 'neo-colonial' aspects may be lacking, this sense of academic cultural periphery does in some sense attend to the distinction between English and all other languages. Some element of relevance, in particular in communicating with local policy makers and practitioners, is lost when scholars' careers whose first and primary local language is not English need to build their careers by publishing in English language journals. And when good scholarship remains 'locked away' in local, non-English journals rather than contributing to the global conversation, we all lose. As an example, take the recent debate on (institutional) context and entrepreneurship (see Chapter 23) – issues that played a large role in the works of German-speaking historian and economist Gustav Schmoller (1838–1917), who emphasized the institutional embeddedness of human behaviour (see Schmude et al. 2008). Unlike Weber and Schumpeter, his works were never translated into English and he never gained any substantial international recognition. An opportunity lost?

We are not sure about the solution to this problem. In many countries, important scholarship published in English is routinely translated into the local language. This makes sense. But the reverse hardly seems to happen – what is the outlet for the good work of talented scholars published in other languages to make its way into the global conversation dominated by English? It is our impression that the newest generation of Chinese, French, German, Indian, Spanish, Russian, Latin American and other scholars is increasingly forsaking publication in journals with ‘local’ relevance, by which we mean situated national relevance, in favour of the global English language scholarly conversation. What was earlier a ‘both’ answer seems increasingly to be ‘either/or’. Relatedly, some of our ideologies and self-indulgences about what qualifies as ‘good’ research may be serving to exclude the development of a healthy and robust community or group of intertwined communities of entrepreneurship scholars beyond the usual suspects (Davidsson 2013; Gartner et al. 2006).

Second, because so much of mainstream empirical research still is conducted in North American and European venues (and because we may not be aware of research conducted elsewhere), we find it too easy to imagine that our theories (and methods) apply, perhaps with a little tweaking, to all places and all times. In a socially constructed and non-equilibrium world, this is incredibly unlikely to be even roughly true. For anyone who is curious about how the world works, the absence of grappling in a theoretically meaningful way with the most varied contexts possible creates a systematic, institutionalized and unnecessary poverty to our theorizing. To too great an extent, we continue offering examination of things that work fine without our help and of phenomena that are already so well understood that some of our questions seem like a variant of the navel-gazing question of ‘how many angels can dance on the head of a pin’.

Related to this, we remain concerned about likely methodological trends. Maturing scholarly fields tend to concentrate on quantitative methods and testable topics in order to gain legitimacy (Cornelius et al. 2006). We also observe a greater focus on quantitative methods because of the internationalisation of national entrepreneurship research communities and younger researchers coming into the national field who pay less attention to national context, instead focusing on what is publishable and what is not (Schmude et al. 2008). While qualitative research in management has the reputation of creating some of the most interesting and high-impact publications (Gartner and Birley 2002), it is generally considered to be a more time-consuming and risky path to publication. For early career researchers in departments that ‘can count but can’t read’ – that is to say those that care more about the number than the significance of faculty members’ publications – incremental quantitative theory testing studies are much more ‘career friendly’ than are most forms of qualitative research. We agree, however, with Hjorth’s (2008) concern that this tendency of internationalization toward convergence on the universal language of numbers may also presage the loss of scientific curiosity and openness.

Finally, and again relatedly: as we argued above, as entrepreneurship scholars, we do not seem to engage much with places and people where good social science might actually help people. We often do not listen properly, we are increasingly mono-disciplinary and do not see what has been developed elsewhere (and published outside our entrepreneurship and management journals). In some cases, we are losing our curiosity at the manifold richness of entrepreneurship, trapped within an economist’s functionalist dream of how entrepreneurship should work and no longer able to imagine how it might work. Both of us believe, for example, that a better understanding of resourceful behaviour can provide practically useful insights to the vast majority of people across time and places, who if they engage in entrepreneurship are forced to do so under severe resource constraints. The emerging body of theory on resourcefulness,

if it is grounded across highly varied contexts, may turn out to be a powerful example of entrepreneurship research that is theoretically interesting and practically useful, and where it is appealingly all but impossible to separate the 'interestingness' and the 'usefulness'. But we also believe that the rhetoric of 'self-sufficiency' and entrepreneurial resourcefulness can become an awful tool of neglectful or even vicious policy making and politics. Surfacing and explaining such ideological predation could be both theoretically revelatory and practically emancipatory. Overall, the range of intertwined theoretically interesting and practically useful themes that emerges from engagement with the questions: 'Who can our research help? And who do we want it to help?' is staggering in our opinion.

All in all, working on this volume has reawakened the excitement we felt as we entered this field and began to experience the yawning chasm between what we as a scholarly community knew and what was worth knowing. Our fear of the other kind of yawning, that which results from the somnolent reading of boring research reports emanating from scholars trapped in a well-structured division of labour reflecting a tightly defined domain specifying what they are supposed to care about, has been put well and firmly to rest. The domain of entrepreneurship research is broader, more contested, more energetic and more promising than it ever has been.

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