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# The Routledge Companion to Entrepreneurship

Edited by Ted Baker and Friederike Welter

# The Routledge Companion to Entrepreneurship

Research in entrepreneurship has been booming, with perspectives from a range of disciplines and numerous developing schools of thought. It can be difficult for young scholars and even long-time researchers to find their way through the lush garden of ideas we see before us.

The purpose of this book is to map the research terrain of entrepreneurship, providing the perfect starting point for new and existing researchers looking to explore. Topics covered range from emerging perspectives, through issues at the core of the field to innovative methodologies. Starting off with a preface by William B. Gartner, each section of the book brings together a world class set of established leading researchers and rising stars.

This considered, comprehensive and conclusive companion integrates the recent debates in entrepreneurship research under one cover, to provide a resource that will be useful across disciplinary boundaries and for a whole range of students and researchers.

**Ted Baker** is Professor of Management, Innovation and Entrepreneurship at North Carolina State University, US and Senior Fellow at Bertha Centre for Social Innovation and Entrepreneurship, University of Cape Town Graduate School of Business, South Africa.

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**The Routledge Companion to Entrepreneurship**

*Edited by Ted Baker and Friederike Welter*

This is a timely and well-crafted book. Baker and Welter, two of the field's most productive scholars, do a masterful job in establishing new boundaries and foundations for more imaginative, creative, informative, and rigorous research in entrepreneurship. With an incredibly impressive assembly of topics and authors with diverse intellectual outlooks, the book pushes the frontiers of our knowledge and builds many bridges to the future. Organized and well written, the book makes an important and rich addition to the field. I congratulate the editors on a great addition to the literature.

Shaker A. Zahra, *Professor, University of Minnesota, USA*

This new volume by Baker and Welter is a must read for all entrepreneurship scholars. The book offers a unique variety of provocative ideas and views by top scholars, taking into account new contexts, theories and methods for conducting entrepreneurship research.

Candida G. Brush, *Professor, Babson College, USA*

Ted Baker and Friederike Welter have brought together a splendid range of ideas and approaches that truly celebrate the diversity, the heterogeneity and the uniqueness of entrepreneurship. This book offers inspiration for those who are brave enough to stray from the well trodden research paths; for those whose spirit of enquiry and intellectual curiosity is not subdued by a need for uniformity and path dependency.

Alistair R. Anderson, *Professor, Robert Gordon University, UK*

Scholars have responded to the recent explosion of interest in entrepreneurship with such a vigorous and dynamic body of literature that it has left considerable bewilderment and confusion about what the field is really about. With this important new book, Ted Baker and Friederike Welter assemble leading scholars that separate the wheat from the chaff and provide a compelling blueprint about what the field of entrepreneurship actually is, and why it is so valuable and important.

David B. Audretsch, *Professor, Indiana University, USA*

This is a joyful, creative, wide-ranging compendium, that celebrates and shares the burgeoning diversity in entrepreneurship research, and brings together many of the field's thought leaders. Chapters on passion, philosophy, process, and politics introduce the fundamentals of radical entrepreneurship with great verve and scholarship. Contributions exploring families and homes, bricolage, and ordinary entrepreneurs remind us of the grounded, everyday nature of most entrepreneurship.

Sarah Dodd, *Professor, The Hunter Centre for Entrepreneurship,  
Strathclyde University, UK*

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# The Routledge Companion to Entrepreneurship

*Edited by  
Ted Baker and Friederike Welter*

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# Foreword

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This book reflects a shift in how entrepreneurship scholars think and undertake research on the phenomena (as in many different phenomenons) of entrepreneurship. What the editors of the book so aptly point out, and what the chapters in the book reflect, is that the kinds of topics that entrepreneurship scholars address, and the methods that entrepreneurship scholars use to explore these topics, are both rapidly changing and expanding. While there are some commonalities in entrepreneurship scholarship, overall, the trend is towards an ever-growing diversity of ideas, perspectives, theories, methods and insights in what entrepreneurship entails.

It is difficult to grasp both the breadth and depth of entrepreneurship scholarship without efforts such as this *Companion*. I find the title of the book, *Companion to Entrepreneurship*, a very appropriate description of what this effort attempts to offer the reader: a guide to the major issues in the entrepreneurship field by experts that have been tasked with offering insights to bring you into their current journeys in entrepreneurship scholarship. You are in very able hands in this endeavour. They are insightful and enthusiastic companions in their guided tours of their specific fields of exploration. It takes a community of scholars to grapple with the phenomena of entrepreneurship, and the book provides you with colleagues who are in the forefront of theory, methods and practice. This book will serve as a useful guide to entrepreneurship scholarship for both emerging scholars in the entrepreneurship field, for practitioners who want insights into the issues confronting scholars who study entrepreneurship, and for well-established researchers who want a comprehensive and in-depth exploration of current critical issues in the entrepreneurship field.

As this *Foreword* is not an introduction, but more of an *amuse bouche* before the main course, I want to suggest three observations on ways that this book can be read as a guide during your journey with these able companions as they tour you through the entrepreneurship field. I offer these observations less as dogma and more as whimsy.

What is both implicit and explicit in nearly all of the chapters in this book is a fundamental quality of entrepreneurship: variation. One of the challenges of studying entrepreneurship is that the phenomenon invariably is about how and why ‘difference’ is created as part of the entrepreneurial process. We might assume that anything that is created is, by definition, new, and, therefore, newness is inherently different from what existed before. But, entrepreneurship is more than the quality of ‘newness’. It really is about ‘difference’, and the creation of ‘difference’ makes the phenomenon so very difficult to both conceptualize and study.

I suggest that one aspect (not all aspects) of the scientific method involves a way of thinking that tends to look for similarities among people, places and things. This way of thinking, I believe, tends to distract us from making sense of entrepreneurship. We look for commonalities that are generalizable across the various phenomena we encounter. I would simplify this, for illustrative purposes, by offering this idea that many of the methods we use when studying groups

(i.e., entrepreneurs, new ventures, environments, processes) is to focus on the means (the averages) in various characteristics of the group. That is, the research question often becomes: On what basis are the members of this group similar? (And, therefore, how or why are entrepreneurs, or new ventures, or entrepreneurial environments different from . . . ?) The average of the group is used as a representation of this convergence to find a similarity within the group and a comparative difference to other groups. Yet, what entrepreneurship 'is', is inherently about the deviations, the differences from each other. There is, in some respects, no average for the group, as the point of the phenomenon is 'difference'. What does the average of differences among a phenomenon that is inherently different, then, really mean? (Yet, as I write this, I am working on a number of papers that use analysis of variance as the primary analytical tool. So, even my own research practice tends to ignore this insight, for better and worse. Or recognize the dilemma at the margins of the discussion.) Oftentimes the differences in the entrepreneurship we study get scrubbed away in looking for the averages in the group. It is even possible to see this tendency when looking at individual cases. While a single case is inherently unique, we as scholars tend to work towards providing generalizable insights from that case study. What was inherently unique, truly unique, in that specific entrepreneurship is then ignored, in order to offer generalizable insights and advice. Yet, these 'outlier', ungeneralizable or even bizarre characteristics, I believe, might be the insights that are truly worth paying attention to. So, we may miss the story completely.

The generalizability of uniqueness is one of the challenges underlying entrepreneurship scholarship. How do we both recognize variation in a phenomenon that is about variation, and then offer insights into how that variation might be generalizable? I see that many of the chapters in this book work diligently to address this conundrum by identifying new 'types' of entrepreneurs and entrepreneurial situations as well as new 'types' of processes and mechanisms by which entrepreneurship occurs. There is no average in entrepreneurship.

The second issue in entrepreneurship scholarship that I think is bubbling to the surface involves accounting for aspects of process and time in entrepreneurial phenomena in different ways. It is not that scholars have not recognized that entrepreneurial events occur over time or that scholars have not recognized entrepreneurship is a process that occurs over time, it is more that the 'feedback' loops in our scientific methods are often treated in a linear manner, rather than co-causal. We tend to think about events occurring over time (for example: and then this happened, and then this happened, and then this happened) rather than within time, itself (for example: these things happened). That is, it is not just reversing the arrows in the feedback loop:  $X \rightarrow Y$  to  $Y \rightarrow X$ , it is more  $X \leftarrow \rightarrow Y$ . The phenomenon of entrepreneurship is not just action or interaction, it is something else altogether. I do not know whether this might be labelled as simultaneous causation. I do see in reading the entrepreneurship literature a difficulty in parsing out where to begin the story when exploring entrepreneurial phenomena. Do we start with environmental factors that then drive individual behaviours, or, do we start with individual factors that drive environmental behaviours? Or do we try our hand at stories that include both? We create the worlds that create us. The world creates us creating the world. That is the rub. I do not think, in the end, either view has primacy over the other. Yet, these two views tend to struggle, in the background, for our attention.

My own odyssey in applying narrative methods to entrepreneurship has made me much more sensitive to 'who' is telling the story in entrepreneurship. I believe that perspectives that take a more social constructionist view of entrepreneurship might be more sensitive to simultaneous agency in entrepreneurship: there are multiple voices that are a part of the phenomenon and that an aspect of our role as researchers it is to appreciate this harmony or cacophony.

Finally, it is worth putting some diligence into recognizing the words that entrepreneurship scholars use to characterize aspects of the phenomenon of entrepreneurship. I hold close this aphorism from Wittgenstein – ‘To imagine a language is to imagine a form of life’. I am growing in my appreciation of the linguistic capabilities of entrepreneurs to invent new ways of talking about their experiences. I am not as confident that scholars in entrepreneurship have been as creative. So, I hope you will celebrate those occasions when scholars provide new words and new meanings to the study of a phenomenon that is so much about new forms of life.

William B. Gartner  
Copenhagen Business School, Denmark and  
California Lutheran University, USA

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## **Part A**

# Setting the scene for the Companion to Entrepreneurship

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# Bridges to the future

## Challenging the nature of entrepreneurship scholarship

*Ted Baker and Friederike Welter*

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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 ordinarieness 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 satiate.

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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## Part B

# The discipline of entrepreneurship research

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The three chapters in this section strongly undergird our observation both that the field of entrepreneurship is legitimate and that it has achieved legitimacy without giving up its rich heterogeneity of perspectives and topics. Hans Landström (Chapter 2) illustrates how history can matter. By tracing the history of academic interest in entrepreneurship he provides a compelling explanation of how legitimation has occurred while acknowledging that for some scholars, our very diversity continues to be a challenge. He points out two promising recent developments that support continued diversity (which we know to be important to adaptive evolution in the most general sense): resistance from entrepreneurship scholars to the perception of narrowing norms around what constitutes good entrepreneurship research, and a throwing off of the shackles of economistic narrowness to view entrepreneurship from a broader social lens.

To our thinking, Daniel Hjorth (Chapter 3) demonstrates the rich combination of legitimacy and openness of our field by the very fact that he calls for and then sketches one version of a ‘philosophy of entrepreneurship’. In scholarly terms, any field that is ready for a ‘philosophy of’ has reached a state of ‘being’, a condition of legitimacy and a promise of ‘becoming’ that puts another nail in the coffin of questions about whether the field should exist or grow. Hjorth addresses both of these issues in what we read as an intellectual dance that moves from attempting to explore deep ontological and epistemological underpinnings to some of the important questions and work in which entrepreneurship scholars engage and seem likely to engage in the near future, while at the same time providing resources for critical reflection. His focus on exploring what it means to study and build theories and models and philosophies of process in entrepreneurship is particularly striking. For a world in which the term ‘philosophy’ is too often invoked to glorify narrow personal ideology, Hjorth’s chapter is broad, open and we think usefully ambiguous at times.

Phil Kim (Chapter 4) celebrates the continued diversity of meanings of the term entrepreneurship to different stakeholders, specifying both what it means to him as a scholar, teacher and someone who engages with entrepreneurs and also by demonstrating respect and understanding for the astoundingly broad spectrum of meanings he identifies. Without attempting a reductionist strategy he proposes that, across this spectrum, our understanding of entrepreneurship would be enhanced by greater attention to the nature and role of values in

entrepreneurship, including the values that elicit entrepreneurial passion, the different values that characterize the varied contexts in which entrepreneurship takes place and a much broader sense of ‘value creation’ than is captured by a narrow focus on financial and short-term economic outcomes. Moreover, if we want to be relevant as entrepreneurship researchers we need to pay attention to how others define and talk about entrepreneurship, others being the media, practitioners and policy makers, or, in other words, stakeholders of our entrepreneurship research.

Ted Baker and Friederike Welter

# Entrepreneurship research and its historical background

*Hans Landström*

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## Introduction

Scientific knowledge has grown significantly in recent decades and many research fields have witnessed a huge increase in the number of scholars, conferences, published articles and journals. Entrepreneurship is no exception and it could even be argued that, compared to many other research fields, it has been tremendously successful. The main explanation for the popularity and success of entrepreneurship research is that the topic is important for societal dynamics and growth. It is a relevant issue for politicians and policy makers in many countries around the world and a great deal of resources have been made available for research and education. In this chapter I will argue that not only is entrepreneurship research important, but also that scholars in the field have been very successful in formulating interesting research questions and conducting interesting research. These scholars have always been very open-minded when it comes to recognizing changes in society as well as experimental in their use of methods and not afraid to challenge the assumptions of both their own field and mainstream disciplines, in theory as well as in practice. This has attracted new researchers from other fields (Landström and Benner 2010), making entrepreneurship a fast-growing research area in recent decades (Landström et al. 2012).

The question of what makes entrepreneurship research interesting is rarely raised – despite the fact that it is critical for a dynamic research field. It is essential in all research fields that scholars are able to formulate interesting questions and conduct interesting research that will ‘open up’ new research areas and resolve long-standing controversies, thereby facilitating the integration of different approaches and allowing conventional beliefs to be challenged (Sandberg and Alvesson 2011). If scholars fail to present interesting research, the field will become less attractive over time and few will pay attention to it. Therefore, interesting research is necessary for the survival and progress of entrepreneurship as a research field.

When scholars are asked what they regard as interesting (Astley 1985, Bartunek et al. 2006; Das and Long 2010) they often mention the importance of being novel and creating counter-intuitive ideas, well-crafted studies in line with accepted scientific practice, valid and relevant conclusions and that the work is well written. However, authors such as Astley (1985), Bartunek

et al. (2006) and not least Davis (1971) argued that valid and well-crafted works are usually not enough for a study to be considered 'interesting'. In his 1971 seminal article Murray Davis asked: How do theories that are generally considered 'interesting' differ from those that are considered 'non-interesting'? His answer was that scholars are regarded as 'great' not because their theories are true but because their theories are 'interesting' in the sense that they challenge certain assumptions, while non-interesting theories affirm their assumptions. When theories challenge certain assumptions they stand out and capture the attention of their audience. In a similar vein, Garfinkel (1967) characterized the state of low attention as the routinized taken-for-granted world of everyday life. This implies that a theory will be regarded as interesting if it challenges accepted truths and represents an attack on the taken-for-granted world of the audience. If it does not challenge but merely confirms taken-for-granted beliefs, the audience will reject its value and the response will be: 'Of course!', 'That's obvious!' or 'Everybody knows that!'

My main argument is that over the years, entrepreneurship scholars have been successful in producing interesting research, but also that the field needs to remain interesting in the future. In this chapter I will elaborate on the issue by employing a historical approach. The aims of the chapter are to (1) provide an overview of the evolution of entrepreneurship as a research field, (2) present works and scholars that have been regarded as interesting in entrepreneurship research at different points in time and in different contexts, and (3) propose some recommendations for the future in order to keep the field vibrant and 'interesting'.

The chapter has four sections. After this introduction I will outline my interpretation of the evolution of entrepreneurship as a research field. Then I will present entrepreneurship works and scholars regarded as interesting (influential and highly cited) over time based on bibliometric analysis and elaborate on historical and contextual differences in our view of what is deemed interesting. Finally, I will discuss how the field can remain 'interesting' and continue to challenge our views of entrepreneurship.

## **Evolution of entrepreneurship as a research field**

### *A framework for understanding the evolution of entrepreneurship as a research field*

Sociology of science scholars have addressed a wide range of issues related to the establishment of new academic fields (Pfeffer 1993). Hambrick and Chen (2008: 33) presented a model based on Merton (1973) to explain the emergence and growth of new academic fields, which they termed a model of an 'admittance-seeking social movement' comprising three overlapping phases: (1) differentiation; (2) resource mobilization and (3) legitimacy building. The phases are not assumed to follow in fixed sequences that typify the successful evolution of new academic fields but can occur multiple times and in multiple directions.

For a new field to emerge, it needs to differentiate itself from existing fields, i.e. early proponents must demonstrate that some important phenomena cannot be adequately addressed by or fall outside the scope of existing fields. New fields need to highlight their distinctiveness and worthiness by framing an agenda indicating that their existence will promote knowledge and even the advancement of society. This is a phase during which 'academic entrepreneurs' play an influential role by making the worthiness of the field and the differences in relation to other fields visible inside as well as outside academia (Landström et al. 2012). In this respect, Hagström (1965) talks about these claims of worthiness as 'utopias' in which a new field can present a vision of making the world 'better'. However, it should be emphasized that a new

field needs to minimize the impression of being a threat to established fields that could otherwise block its development.

In addition, it is necessary to mobilize resources in order to ensure a critical mass of scholars who can derive energy from each other and secure control of the resources needed. According to Hambrick and Chen, it is an issue of a 'social movement' in which three factors determine the effectiveness of the mobilization: political opportunity, shared interests, and social infrastructure. A new research field has greater possibilities of success when conducive environmental conditions are present such as a public agenda and political opportunities. A common interest will help to define the scholarly identity and solidify its membership and efforts. The progress of the field also depends on the existence of social ties among its advocates as well as forums for meetings and communication. Social movement theory highlights a strong association between the strength of relationships among key actors and the possibilities of success (Marwell et al. 1988).

Finally, an emerging research field needs to build legitimacy in the eyes of the academic establishment and its leaders to demonstrate that they are qualified to spearhead this development. Hambrick and Chen argued that legitimacy takes two forms; intellectual persuasion, i.e. a field gains status by making unique contributions to knowledge and/or society, and emulation, i.e. by conforming to the methodological or paradigmatic conventions of more well-established fields. Thus, it is important that the research in new fields operates in accordance with the norms of more established neighbouring fields – it is often a question of adopting the conventions of 'normal science' in which research is assumed to be cumulative and new research is built on and cites earlier high quality works – a process of 'institutionalization' (DiMaggio and Powell 1983).

In this section I will use Hambrick and Chen's framework to discuss the evolution of entrepreneurship as a research field in its own right. However, I will start by presenting a brief overview of early contributions to entrepreneurship research. It is important to emphasize that history can be written from many different perspectives and focus on various aspects, and this review is my subjective way of describing the evolution of the field.

### *Early contributions to entrepreneurship research*

The function of entrepreneurship is probably as old as exchange and trade between individuals, but it was not until the emergence of economic markets during the Middle Ages that the concept gained importance and authors started to take an interest in the phenomenon, for example, through the writings of Cantillon (1755/1999) in which he outlined the principles of the early market economy with focus on individual property rights and economic interdependency. In the mid-eighteenth century, classical economic theory was developed based on Adam Smith's seminal work *Inquiry into the Nature and Causes of the Wealth of Nations* (1776/1976), which laid the foundation for analysis of the way the market economy functions as well as influencing the view of the entrepreneur in the economy, who then more or less disappeared from economic theory for a considerable period.

Although interest in entrepreneurship among economists seemed to lessen, there were a few exceptions. Joseph Schumpeter is probably the best known of the economists with an interest in entrepreneurship in the early part of the twentieth century (Schumpeter 1912, 1934). His idea was to build a new economic theory based on change and novelty, and his basic assumption was that economic growth resulted from innovations or 'new combinations' that create a disequilibrium on the market. Another view of the entrepreneur in economic theory was to

be found in the Austrian School of Economic Thought, represented by Carl Menger in the nineteenth century, further developed by Ludwig von Mises and Friedrich von Hayek. Today this view is represented by Kirzner (1973), who regards the entrepreneur as a person who is alert to imperfections in the market and able to coordinate resources in a more effective way thanks to information about the needs and resources of different actors. Finally, I should mention Knight (1916/1921) who made an important distinction between insurable risk and non-insurable uncertainty, arguing that entrepreneurial returns result from activities that cannot be predicted and that entrepreneurial competence is the individual's ability to deal with uncertainty.

In the 1940s, a number of scholars anchored in economic history began to take an interest in entrepreneurship. The studies typically employed a Schumpeterian approach and focused on the modernization process of societies around the world. After a couple of decades this stream of research lost momentum and scholars from psychology and sociology entered the field. The latter were interested in the entrepreneur as an individual and started to study her/his key traits and personality (e.g. work by McClelland 1961). As a result, the personal qualities of the entrepreneur occupied a prominent position in entrepreneurship research during the 1960s and 1970s.

Thus, for a long period, entrepreneurship and small business did not attract a large number of researchers nor did it become institutionalized within mainstream disciplines (Landström 2005b, Landström and Benner 2010). This marginalization may partly be explained by a limited interest in entrepreneurship and small business in society. Economic development and dynamics were assumed to be based on mass production; large companies were seen as superior in terms of efficiency and as the driving force behind technological development. The marginalization may also be partly explained by changes within some mainstream disciplines. For example, economics became increasingly formalized and mathematically oriented, which made it difficult to include the entrepreneur in such models.

## Differentiation

From having been a fairly marginal topic in some mainstream disciplines, such as economics, economic history, psychology and sociology, the origins of becoming a separate field can be traced back to the 1970s and 1980s. The claim of distinctiveness mainly came from outside academia. These decades were characterized by a 'creative destruction' in Western societies, not least in the US (Carlsson et al. 2009), where a number of institutional reforms were introduced such as changes in the tax law, deregulation of financial institutions and the introduction of the Bayh-Dole Act within the field of intellectual property rights. Several technological breakthroughs could be identified at this point in time, for example, in DNA research and the microprocessor revolution. The world economy changed substantially due to intensified global competition. Furthermore, there was also a change in mentality. The 'twin oil crises' in the 1970s triggered uncertainty about large corporations' ability to create jobs and dynamics in society, and they were increasingly seen as inflexible and slow to adjust to market changes. 'Small is beautiful' became a catchword and entrepreneurial activities developed into a dynamic factor in society, supported by politicians such as Ronald Reagan in the US and Margaret Thatcher in the UK.

These changes in the economy triggered an interest among scholars from different fields. Studies about intellectual migration to new research fields have often focused on anomalies of various kinds in existing fields, lack of opportunities (due to intellectual rigidity) and chances to pursue lines of research in a more unrestricted manner within new fields (Ben-David 1970), but also the importance of political and/or economic environments for creating an interest in and thus migration to new fields (Elzinga 1985). Irrespective of individual reasons, obviously many scholars from different fields identified the changes in the economic environment as well as deficiencies

in existing theories and started to investigate issues related to entrepreneurship and small business. Given the newness of the field, it was easy for researchers to carry out research on entrepreneurship without experiencing obvious competence deficits – it was a ‘low entry field’.

At this time many pioneering studies on entrepreneurship emerged that focused on (1) the discovery of this ‘new’ phenomenon, (2) differentiating it from mainstream disciplines and (3) making entrepreneurship and small businesses more visible, not least to policy makers and politicians. The pioneering studies included, for example, the work by Birch on job creation in the US (1979), but also those by Becattini (1989) and Brusco (1982) on ‘industrial districts’ in Italy that demonstrated the importance of small businesses for regional development as well as the studies by Acs and Audretsch (1990) in which they argued that small businesses have an innovative role in the economy. Many other studies also challenged our assumptions on new ventures and small businesses, making the field interesting and attracting new scholars from different disciplines.

An important factor in the emergence of entrepreneurship as a research field was the claim that existing fields were ill-equipped to focus on the changes in the economy, i.e. the assertion from ‘inside’ academia that existing mainstream disciplines could not properly address the questions of importance in the new economic environment. One of the clearest illustrations in this respect is the statement by William Baumol:

‘The theoretical firm is entrepreneurless – the Prince of Denmark has been expunged from the discussion of Hamlet’ (p. 66), and the neoclassical model is essentially an instrument of optimal analysis; ‘maximization and minimization have constituted the foundation of our theory, [but] as a result of this very fact the theory is deprived of the ability to provide an analysis of entrepreneurship’ (p. 68).

(Baumol 1968: 66–68)

Another example is Casson (1982), who argued that the reason behind the lack of entrepreneurship in economic theory was the extreme assumptions about access to perfect information that are implicit in orthodox (neoclassical) economic theory. At the same time Toffler wrote *The Third Wave* (1980), in which he predicted the revolutionization of businesses in a way that would affect the structure of industry and the size of work units towards smaller businesses, while in the same vein, Handy (1984) argued that the changes during the 1970s could be considered a fundamental restructuring of work. In this respect, Brock and Evans (1986) empirically demonstrated the changes occurring in the industrial structure including the increased importance of small businesses in the US, thus suggesting that small business economics was a distinct research area.

Initially, there was no distinct community of entrepreneurship scholars. Most of the researchers who rushed into the field in the 1980s did not regard themselves as entrepreneurship researchers but as scholars in mainstream disciplines. However, during the 1990s the number of entrepreneurship chairs around the world increased, thus contributing to differentiating it from other fields, although it remained fairly difficult to obtain a faculty position in ‘entrepreneurship’, and most of the researchers held positions within other fields (Finkle and Deeds 2001). Nevertheless, many scholars with an interest in entrepreneurship began to regard themselves as entrepreneurship scholars (as opposed to sociologists, economists or management scholars). As a distinct research community, the differentiation vis-à-vis other fields continued during the 2000s; for example, we can identify greater convergence in entrepreneurship research through the creation of an increased number of tenured faculty positions in entrepreneurship, a larger group of faculty members who regarded themselves as entrepreneurship scholars and not least more young scholars

anchored in knowledge on entrepreneurship (who had gained their Masters in entrepreneurship and written their thesis on entrepreneurship issues) compared to earlier generations of entrepreneurship scholars who were rooted in other fields of research (Hjorth 2008).

Having said that, in the 2000s we can also find increased divergence within entrepreneurship research itself. This divergence is both internal and external. The internal divergence emanates from (1) entrepreneurship scholars who were critical of the perceived lack of creativity and imposition of rigid norms in the current dominant entrepreneurship research approaches and called for alternative paradigmatic premises within the field (Steyart and Hjorth 2003) as well as (2) scholars who argued for a broadening of entrepreneurship, viewing it not only as an economic achievement, but as a societal phenomenon, thus the number of prefixes in entrepreneurship has increased (e.g. social entrepreneurship and societal entrepreneurship). The 'external' divergence is due to the fact that entrepreneurship has become attractive to scholars in many different disciplines. As a consequence, there is a scattered and loosely connected group of researchers anchored in many different disciplines who now and then conduct studies on entrepreneurship. They regard themselves as 'disciplinary researchers' and do not participate in the entrepreneurship research community or 'conversation' to any great extent.

New fields need to demonstrate their importance and uniqueness while at the same time collaborating with established fields in order to avoid being perceived as a threat. Entrepreneurship has always drawn heavily from other knowledge platforms and in that sense been regarded as complementary compared to more established fields. The relationship to strategic management is particularly important. For example, in the (American) Academy of Management, the Entrepreneurship Division could be considered an offshoot of the Division of Business Policy and Planning (now Business Policy and Strategy), and many entrepreneurship pioneers could also be regarded as pioneers in strategic management (e.g. Cooper, MacMillan and Hofer). Over time, strategic management has been acknowledged as a core approach within entrepreneurship research and many are in favor of closer integration between the two fields.

### *Mobilization*

Mobilization is a major factor within emerging fields in order to attract a larger group of scholars and gaining control over the resources needed for collective action (Jenkins 1983), i.e. it is a 'social movement' (Hambrick and Chen 2008). In the 1970s and 1980s, the research community was small, fairly fragmented and individualistic, i.e. entrepreneurship research was to a large degree dependent on individual initiatives and projects as well as a very enthusiastic group of scholars who argued for the importance of entrepreneurship and small business in society. Many initiatives were taken by 'academic entrepreneurs' (Landström 2005a) to stimulate communication between the fairly fragmented and individualistic research community in order to create an adequate infrastructure, for example, through the creation of local research units (Clausen et al. 2012), initiating the start of professional organizations as well as the launch of academic conferences and scientific journals. As a consequence, entrepreneurship changed from a rather marginal topic of interest and only a few researchers in some mainstream disciplines to one that attracted many scholars from different fields, not least management studies, who rushed in and started to elaborate in a more systematic way on issues related to entrepreneurship and small business.

The education system also played an important role. Students started to request courses on entrepreneurship and small business, and their early introduction by prestigious academic institutions such as Wharton, Harvard and Stanford conferred a measure of legitimacy in the academic system, after which many business schools introduced such courses. In the 1960s and

1970s, entrepreneurship and small business management courses emerged in the US, several years before such courses were offered in Europe (Cooper et al. 1997). The early growth of entrepreneurship courses was also linked to the large resources directed towards US entrepreneurship programmes – mainly from external donors but also foundations such as the Coleman and the Kauffman Foundations as well as several government support programmes across the US and Europe aimed at stimulating entrepreneurship education. However, it was mainly during the 1990s that an enormous growth in the number of courses and education programmes took place, not least at universities in Europe. At the turn of the century, the number of educational institutions in the US providing entrepreneurship courses totaled 1,600 with more than 2,200 courses on offer (Katz 2003).

In the 1990s the research field grew significantly, as entrepreneurship and small business continued to be a ‘hot topic’. Many changes took place in society, for example, in the area of consumer tastes, technological advances and transformed industry structures, leading to an increased complexity and uncertainty that constituted a hotbed for entrepreneurship and new business opportunities. This situation meant that societal interest in entrepreneurship remained high and entrepreneurship and small business became an important element of the policy agenda in many countries. In Europe entrepreneurship moved from being an inconspicuous part of economic and employment policies to a political centerpiece. This connection to policy served as a springboard for the growth of entrepreneurship in the 1980s and 1990s but also strengthened the external legitimacy of the field. However, it remained a ‘low-entry’ field that lacked strong theoretical frameworks and had rather fuzzy definitions and unclear boundaries. As a consequence, although migration into the field was extensive, there was also large-scale mobility of scholars into and out of the field.

A main characteristic of the 1990s was the building of a strong infrastructure within the field. In this respect Aldrich (2012) emphasized the importance of ‘institutional entrepreneurship’, involving collective action by countless numbers of scholars, groups, associations and agencies. These have been instrumental in the creation of an infrastructure within the field, comprising: (1) social networking by means of professional associations such as the Entrepreneurship Division of the Academy of Management (1985), the European Council for Small Business (1988) and the European Foundation for Entrepreneurship Research (1987) and conferences (e.g. the UK Small firms’ Policy and Research Conference, 1979; the Nordic Conference on Small Business Management, 1980; Babson College Entrepreneurship Research Conference, 1981; Research in Entrepreneurship and Small Business [RENT], 1987); (2) publication opportunities (e.g. *Entrepreneurship Theory and Practice* 1975/1988; *International Small Business Journal*, 1982; *Journal of Business Venturing*, 1985; *Piccola Empresa* 1987; *Revue Internationale PME*, 1987; *Entrepreneurship and Regional Development*, 1989; *Small Business Economics*, 1989); (3) training and mentoring, for example, due to the introduction of PhD programmes and courses as well as through professional organizations and conferences, of which the European Doctoral Programme in Entrepreneurship and Small Business Management (launched by the European Council for Small Business in 1990) can be mentioned as an example of a pioneering programme; (4) foundations and funding sources; (5) recognition and awards, of which the best known is the Global Award for Entrepreneurship Research (since 1996), although there are also several awards related to professional associations and conferences; and (6) globalization of the field, which has developed into a worldwide research community since the start of the new millennium with extensive interest in entrepreneurship and small business among scholars in China and the rest of Asia. Since the 1970s the entrepreneurship field has grown from a small group of isolated scholars to an international community of researchers, teachers, institutes and departments involved in entrepreneurship and small business issues, or as Ziman (1994) expressed it, there has been

a shift from 'cosmopolitan individualism' to 'international collectivism' making the research more global.

Of equal importance is the cognitive development of the field, which includes the definition of central concepts, methodological considerations and the production of knowledge. During the strong infrastructure building within the field in the 1990s, this cognitive development became highly fragmented, mainly consisting of the field in empirical explorations of the phenomenon in which scholars tried to understand the entire phenomenon of entrepreneurship, pursuing a number of different directions. The field became a 'melting pot' of scholars from various disciplines, many novel research issues emerged, new methodological approaches and concepts/theories were used, and we gained a great deal of empirical knowledge about the phenomenon. A range of new problems and issues were identified that triggered our interest in and increased our knowledge of entrepreneurship. The multidisciplinary character of the field opened up for a variety of methodological approaches, ranging from deep qualitative studies to sophisticated quantitative analyses. Many of the new concepts and theories were borrowed from mainstream disciplines, not least strategic management, despite the fact that some of them were found to have a weak explanatory value when tested in the context of entrepreneurship.

New fields not only need to mobilize human but also financial resources. At a relatively early stage, funds were made available for entrepreneurship and small business research, for example, in the US, the National Federation of Independent Business (NFIB) and the Kauffman Foundation promoted efforts to improve the databases to which entrepreneurship researchers had access (e.g. the seminal study by Birch and Cooper in the 1980s, and later the Global Entrepreneurship Monitor and the Panel Study of Entrepreneurship Dynamics), while the OECD provided funding for the Observatory Report that described the evolution of the small business sector in OECD countries over time. In this respect the databases built within the Internationalization of Strategic Orientation of Small and Medium-sized European Enterprises (INTERSTRATOS) project should be mentioned (Landström et al. 1997), while in the UK, the Economic and Social Research Council (ESRC) played an important role in funding a major research programme on small businesses (Storey 1994). Governments in several countries and on regional levels have also been important for promoting entrepreneurship research through the financing of chairs in entrepreneurship, such as in Germany at the end of the 1990s (Klandt 2003) and the establishment of entrepreneurship research centres in, for example, the Netherlands (Aldrich 2012).

### *Legitimacy*

In order to ensure a position in the academic system, an emerging field must be considered legitimate in the eyes of other scholars, and entrepreneurship researchers worked hard to achieve their legitimacy. In the early days of entrepreneurship research, the studies were characterized by a great diversity in terms of research approaches due to the high migration of scholars from different fields. Scholars within as well as outside the field were critical of the quality of the research conducted and many reviews complained about the lack of consensus on definitions, as well as fragmentation, the absence of a theoretical foundation and methodological inconsistencies in empirical research (Landström 2005b), indicating that the academic legitimacy of entrepreneurship research was generally deemed quite low. Thus, for a long time, entrepreneurship had difficulty gaining legitimacy in the academic world. However, legitimacy was created by external forces, as entrepreneurship captured the public's attention, not least because of the efforts of policy makers and politicians, but also due to increased media interest.