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What makes entrepreneurship research interesting? Reflections on strategies to overcome the rigour–relevance gap

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ABSTRACT
As entrepreneurship researchers compete to have their work published and universities strive to attract the best entrepreneurship scholars, it is appropriate to examine what makes entrepreneurship research interesting. Interesting studies are usually defined as well-crafted and well-written studies that challenge established knowledge, and produce new theories and findings. This paper examines entrepreneurship scholars' views on the characteristics of interesting entrepreneurship research by means of a qualitative approach. Eight focus group interviews comprising junior and senior entrepreneurship scholars were conducted. A core finding is that interesting studies must be relevant to practice. However, the institutionalization of entrepreneurship as an academic field has favoured rigour at the cost of relevance, leading to scholars' frustration with the rigour–relevance gap. In this paper, we analyse various dimensions of interestingness and reflect on strategies for overcoming the rigour–relevance gap, with particular focus on the creation of applicative knowledge.

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Entrepreneurship research; institutionalization; interesting research; rigour; relevance; applicative knowledge; practice–theory

1. Introduction

Some entrepreneurship studies capture your attention, are thought provoking and make you want to learn more. Such studies merit the adjective 'interesting' and are increasingly important for the reputation and expansion of the field of entrepreneurship research, as well as for individual entrepreneurship researchers. In addition to the interest, individual papers spark in a particular research field; the field as a whole benefits when it is perceived as interesting because it will attract researchers from other fields who will contribute to the renewal of the research field.

The scientific study of entrepreneurship has grown significantly since the 1980s (Frank and Landström 1997) as evidenced by the increasing number of entrepreneurship researchers, conferences and journals as well as by the publication of many scholarly articles. It can

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be argued that in comparison with many other research fields, entrepreneurship has been successful in building an international community of scholars and advancing our knowledge on entrepreneurship (Aldrich 2012; Landström, Harirchi, and Åström 2012). The widely accepted explanation for the popularity and success of entrepreneurship research is that entrepreneurship has been regarded as an interesting field of research as well as important for societal dynamics, growth and thus for policy-makers.

While researchers have addressed the subject of interesting research in neighbouring fields such as management (e.g. Alvesson and Sandberg 2013b; Bartunek, Rynes, and Ireland 2006; Das and Long 2010), no such studies exist in entrepreneurship and in general there is little research on strategies for creating interesting research. However, it can be assumed that each research field develops its own rules for what is considered 'interesting' (e.g. for family business, see Salvato and Aldrich 2012). Although entrepreneurship research can be regarded as a successful and popular field of research, our results reveal a certain degree of dissatisfaction with recent developments. Thus, there is an urgent need to address the issue of what is regarded as interesting in entrepreneurship research and how to create an interesting field in the future. In line with this need, entrepreneurship scholars have recently begun to challenge prevailing assumptions about theory and methodological practices (see the special issue of *Entrepreneurship and Regional Development* 2013; Zahra and Wright 2011) or have tried to provide results that can guide entrepreneurial action (van Burg and Romme 2014). Our paper problematizes the development of the field in terms of its interestingness and aims to trigger reflections within the community of entrepreneurship scholars as the institutionalization of the field may have changed the basis for its legitimacy.

The evolution of research fields – their rise, institutionalization and possible demise – forms a central part of sociology of science studies. An example is Fleck (1979), who spoke about 'styles of thoughts' in the institutionalization of research fields, which is also a theme in Kuhn's famous paradigm theory (1970). Both Fleck and Kuhn stressed the collective nature of research – how scholars create institutional frameworks in order to gain legitimacy as a research field. As pointed out by Hambrick and Chen (2008), based on Merton (1973), emerging research fields seem to follow an institutionalization process, including three overlapping phases: (1) differentiation of the field from existing fields, (2) resource mobilization and (3) legitimacy building in the eyes of the academic establishment. However, the trigger (i.e. the differentiation process) for the establishment of entrepreneurship as a (promising) field of research mainly emerged from external stakeholders, whereas the impetus for the creation of a new discipline in medicine and natural sciences normally arises from within the scientific community (Röbken 2004).

In line with such reasoning, since the emergence of entrepreneurship research in the 1980s a powerful and influential idea has taken root – promoted especially by the media and by policy-makers – that a better understanding of entrepreneurship can help solve various societal problems. It has been suggested, for example, that the study of entrepreneurship can give politicians ideas on job creation, lead to the founding of new companies and industries, as well as advance regional development. In accordance, a measure of the success of entrepreneurship as an academic field was the number of startups, jobs and new industries (Fayolle 2007; Landström 2005). This adds a normative and advisory dimension to the research field. Furthermore, entrepreneurship research had to be practice-oriented, helping entrepreneurs to solve their problems, thus ensuring the success of their new ventures. For example, in his editorial note in the inaugural issue of the *Journal of Business*

Venturing in 1985, Ian MacMillan stressed the need for the articles published in the journal to be of practical relevance and authors still have to preface their article with an executive summary that directly spells out implications for practitioners (MacMillan, Zemann, and Amoroso 1985, 5). This was the clarion call for relevance in entrepreneurship research. Even in the early periods of entrepreneurship research the demand for external legitimacy (i.e. practical relevance) resulted in a focus on certain research characteristics of more interest to external stakeholders (e.g. practitioners and policy-makers) than to researchers. These characteristics have created sustained imprinting elements in entrepreneurship research (Marquis and Tilcsik 2013). Stinchcombe (1965) demonstrated how imprinting elements persist well beyond the introductory phase because the influence of early experience remains for a long time despite significant environmental changes.

However, entrepreneurship research is becoming more and more institutionalized in terms of entrepreneurship chairs, highly ranked journals, a reasonably coherent set of research questions and methodologies (Finkle and Deeds 2001; Welter and Lasch 2008). In addition to the institutionalization and the internationalization of entrepreneurship as a field of research, the prevailing context in business schools (e.g. a stronger focus on accreditations and rankings based on top journal publications) enforces the consideration of academic legitimacy. Institutionalization favours rigour at the cost of relevance, while at the same time rigour promotes the institutionalization of research fields. Business schools often 'try to meet contradicting expectations for practical relevance on the one hand and academic rigour on the other hand by partially decoupling the teaching activities, particularly on the MBA and executive level, from research activities' (Röbken 2004, 174). Yet, it is important to distinguish the field level and the single study level. Increasing rigour at the single study level may not necessarily decrease the relevance of research at the field level although it poses a new challenge to integrate fragmented small pieces of specialized research into practically relevant conclusions (e.g. van Burg and Romme 2014).

The increased institutionalization of entrepreneurship research seems to have created a new sensitive period in entrepreneurship research where, as legitimacy anchored in academia enhanced, the external stakeholders' interests are increasingly marginalised. Even when some imprinted characteristics persist, Higgins (2005) found evidence of 'windows of imprintability' in which new sensitive periods occur. As stated by Marquis and Tilcsik (2013), 'multiple sensitive periods' over a timeframe may modify former imprinting elements or create new ones. This has resulted in increased scientific rigour in many management fields (Flickinger et al. 2014), triggering intense discussions about the gap between scientific rigour and practical relevance in research (e.g. Aram and Salipante 2003; Daft and Lewin 2008; Hirschheim and Klein 2003; Hodgkinson and Starkey 2011; Kieser and Leiner 2009, 2011; Lehmann, McAlister, and Staelin 2011).

Based on the above considerations, we focus on the following key research question: What do entrepreneurship scholars regard as 'interesting' entrepreneur research? and Which key dimensions characterize interesting entrepreneurship research? In order to elaborate on these question and based on the fact that our results revealed a high degree of frustration among the participating entrepreneurship scholars regarding the practical relevance of contemporary entrepreneurship research, we will investigate and reflect on some important options how entrepreneurship researchers can deal with the rigour–relevance gap diagnosed and problematized in our empirical study.

Table 1. Aspects that make management studies 'interesting'.

Study sample	Bartunek, Rynes, and Ireland (2006) 67 members of the *Academy of Management Journal* (AMJ) editorial board	Das and Long (2010) 131 members of the Administrative Science Association of Canada (ASAC)
Features that make a study interesting	1. Counterintuitive, i.e. challenging established knowledge, and going against conventional wisdom	1. Innovative method design, e.g. employing a novel research methodology or an exemplary application of an existing methodology
	2. Quality – well-crafted theory, methods, and good fit of data and theory	2. Generalizability and data analysis – research methods that employ objective and sophisticated data analysis to interpret data collected from valid generalizable samples
	3. Good writing, e.g. well-crafted, clear and engaging with rich descriptions	3. Novelty, e.g. focus on unveiling embedded assumptions, creating counter-intuitive ideas
	4. Creating new theories/findings: synthesizes previous theories, integrates multiple perspectives, etc.	4. Relevance – the findings should make sense and have practical relevance
	5. Usable practical implications that generate usable knowledge that is relevant in the real world	5. Communication, i.e. good communication to readers (logical arguments and readers involvement)
Additional findings	The study by Bartunek, Rynes, and Ireland (2006) indicates that interesting features differ in various parts of the world, i.e. they are context-dependent	Das and Long (2010) argue that the scholarly background influences which features are considered interesting

The contribution of this paper is that it is among the few research studies that address the rigour–relevance gap in entrepreneurship research using empirical evidence, and one of the first to focus on interestingness and the related rigour–relevance gap. Furthermore, based on our empirical results, we offer several suggestions for how researchers can deal with the rigour–relevance gap in order to reduce the gap. The need to reflect on such strategies is a reasonable consequence of the empirical evidence revealed by our study. Finally, as a strategy to reduce the rigour–relevance gap, we discuss the possibility of creating 'applicative knowledge' in entrepreneurship research.

The structure of the paper is as follows. Section 2 is a literature review of the characteristics of interesting research in management studies, a field closely linked to entrepreneurship research that can be considered an adequate role model. Section 3 describes the research method applied in the empirical part of our study. Section 4 presents the results from our focus groups, while Section 5 discusses our findings in the context of the research questions. In Section 6, we propose several strategies for how to conduct interesting entrepreneurship research.

2. Literature review

2.1. *That's interesting in scientific studies*

Interestingness could be regarded as a matter of taste, something personal and subjective. But interestingness is not only a matter of idiosyncratic opinions. There are collectively held assessments regarding, for example, the popularity of research topics and methodological approaches (Alvesson and Sandberg 2013a). Naturally what is regarded as interesting depends on the audience targeted by a paper. In entrepreneurship research, there are many potential audiences, from a set of fellow researchers to educators, students and textbook writers, as well as external audiences of entrepreneurs, consultants, investors, media, policy-makers and politicians. In order to be interesting, entrepreneurship research needs to attract at least one of these audiences – the wider the audience that finds the research interesting, the more impact it will have (Shugan 2003).

When management scholars are asked to define high-quality research (e.g. Astley 1985; Bartunek, Rynes, and Ireland 2006; Craig 2010; Das and Long 2010), their response is that the research question must be important and the study should be 'well-crafted' and well-written, with valid and relevant conclusions. However, Astley (1985), Bartunek, Rynes, and Ireland (2006) and Davis (1971) have argued that interesting research must be something more. In his 1971 seminal article entitled 'That is interesting!' 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 they are 'interesting' in the sense that they challenge some of an audience's assumptions. 'Non-interesting' theories, on the other hand, only affirm the taken-for-granted assumptions of their audiences.

Referring to management articles, especially those published in the *Academy of Management Journal*, Bartunek, Rynes, and Ireland (2006) support Davis's (1971) claim that management research that challenges some current assumptions is interesting. In addition, they identified a number of features that make management studies interesting. For example, interesting studies are of high quality (well-crafted and well-written), present new theories and findings, and contribute practical knowledge (see Table 1). Das and Long (2010) support these findings by underscoring the importance of novelty in methodological approaches and applications, as well as in the creation of counter-intuitive ideas that challenge assumptions. They particularly emphasize the importance of validity and rigour (generalizability and data analysis), clear communication and practical relevance in interesting research, adding some features of interesting research that Bartunek, Rynes, and Ireland (2006) neglected, such as the need for findings to make intuitive sense and the necessity of reviewing past theories. Das and Long's study also provides support for the argument that depending on their scholarly backgrounds, researchers (and journal editors/reviewers) are likely to prioritise different features that make research interesting.

In the management research area of family businesses, Salvato and Aldrich (2012) conclude that the features that make family business studies interesting are markedly different from those that make other management research interesting. A family business article is interesting when it describes family-specific issues such as the processes and structures of the family business and their effect on the family and vice versa (e.g. Frank et al. 2010; Zachary 2011). As noted in Section 1, this observation supports the claim that interesting research depends on the research area and thus highlights the need to address this problem in entrepreneurship research.

2.2. The rigour–relevance debate

In the past decade, many areas of social science research, including several subfields of management research, have witnessed an intense debate on research rigour and/or research relevance (cf. Aram and Salipante 2003; Baldridge, Floyd, and Markóczy 2004; Daft and Lewin 2008; Hodgkinson and Rousseau 2009; Hodgkinson and Starkey 2011; Kieser and Leiner 2009, 2011; Lehmann, McAllister, and Staelin 2011; Rynes, Bartunek, and Daft 2001; Starkey, Hatchuel, and Tempest 2009; Starkey and Madan 2001; Vicari 2013). It is claimed that management as a scholarly field failed to achieve academic legitimacy at an early stage because of its proximity to practice (Kieser and Leiner 2009). In response to this criticism, greater scientific rigour stemming from more theory-driven research, larger sample sizes, more sophisticated data collection methods and increased use of statistical analysis were considered necessary (Flickinger et al. 2014). Although this debate about scientific rigour and/or practical relevance in management research raises many questions, two of the most important are the following: Is it possible to bridge the rigour–relevance gap? And if so, how?

Some researchers are pessimistic about the possibility of bridging the rigour–relevance gap. For example, when referring to Luhmann's (1998) system theory approach, Kieser and Leiner (2009) claim the gap is unbridgeable. They are of the opinion that practitioners and management researchers belong to different systems that are largely self-referential and focused on their own logic. These self-referential systems are viewed as important conditions for high levels of effectiveness (Wolf and Rosenberg 2012). The difficulty of bridging the two systems is increased by different goal criteria, and the fact that the systems are stabilized by peer reviews and reputational mechanisms, and that this form of self-organization leads to dissociation from other societal structures and the formation of a community that adheres to its own logic (Flickinger et al. 2014). Other researchers (e.g. Alvesson 2012) even argue that scholars have nothing to say to practitioners – much has already been said and as the vast majority of papers are incremental, narrowly defined and based on the same assumptions, most ideas are already taken-for-granted and reproduced. One could also argue that practitioners create new knowledge that helps to solve problems in their firms but have no interest in spreading it because so doing could destroy their competitive advantage (Vicari 2013). Furthermore, the time frame of researchers and practitioners may be different. Whereas practitioners often need quick and efficient solutions for problems, researchers act on different time horizons creating an asynchrony between theory and practice (e.g. O'Driscoll and Murray 1998).

However, other authors argue that management is a strongly 'reality-oriented' academic field that aims to support business practice (Wolf and Rosenberg 2012). These researchers consider it possible to bridge the rigour–relevance gap. However, bridging the gap involves three problem areas (cf. Straub and Ang 2008; Van de Ven and Johnson 2006; Wolf and Rosenberg 2012): The first is a 'problem formulation gap', indicating that researchers and practitioners experience different kinds of problem and formulate problems in different ways. The second is a 'research process gap' and concerns the problem of collaboration between researchers and practitioners in the knowledge production process as well as the fact that particular kinds of knowledge are required to make a study relevant to practice. Finally, there is a 'dissemination gap', i.e. the translation of research findings into practice and their dissemination – practitioners fail to read academic publications, which are written by researchers for researchers.

Many different solutions have been presented on how to bridge the rigour–relevance gap, including: promoting collaboration between researchers and practitioners (Hodgkinson and Rousseau 2009; Nicolai, Schulz, and Göbel 2011; Pettigrew 2001; Van de Ven and Johnson 2006); changing the academic recruitment and promotion evaluation criteria (Gomez-Mejia and Balkin 1992); modifying how research is conducted (Wolf and Rosenberg 2012); changing the style and content of research so that it is more accessible to practitioners (Rynes, Bartunek, and Daft 2001; Starkey and Madan 2001); developing evidence-based knowledge (Frese, Rosseau, and Wiklund 2014); putting more emphasis on research that builds management theory *and* contributes to management practice (George 2014); and accepting the rigour–relevance gap as a paradox that can be beneficial in generating new research and practices (Bartunek and Rynes 2014).

The rigour–relevance gap has provoked a dynamic discussion in management research, but not in the field of entrepreneurship. Nevertheless, some tension exists at present in entrepreneurship research because many researchers consider that relevance has been suppressed in favour of rigour. One can argue that this is part of the maturation of entrepreneurship as an academic field and thus entrepreneurship reproduces a pattern observable in more mature fields such as management with its vivid discussion on the interestingness and practical relevancy of its research. It is well known that this debate has already started 'behind closed doors' among entrepreneurship researchers too. But as Bartunek and Rynes (2014) observed, there is a clear need for empirical research on this topic, because even in the management field the vast majority of papers dealing with the rigour–relevance issue are non-empirical. Consequently, we believe that empirical evidence can stimulate the discussion in the field of entrepreneurship research, increasing awareness of the rigour–relevance tension and offering a more nuanced view. Furthermore, the typical development of maturing fields that favours rigour is not a law of nature but can be actively influenced and shaped through reflection.

3. Method

3.1. Focus group interviews

Based on the insights from the literature review, which revealed sparse empirical knowledge and the need to generate a broad rather than a narrow view of interestingness in entrepreneurship research in order to obtain an informative overview, we decided that qualitative data fit best (Edmondson and Mcmanus 2007). We used eight focus group interviews to collect our data. In this research method, 5–8 participants participate in a moderator-led discussion (Krueger and Casey 2009). The 42 participants in our focus groups were entrepreneurship researchers, all of whom were employed or studying at European universities. We do not assume that the results represent a coherent European perspective, but consider that the diversity in terms of career level, research experience and function enhances the grasping of a fairly comprehensive picture of the chosen topic embedded in the heterogeneous European university contexts including researchers from North, South, West and Central Europe. We conducted these focus group discussions in connection with a number of international conferences during the autumn of 2012. We divided the participants into Junior Scholars (JunS) and Senior Scholars (SenS). The JunS were in four focus groups: three with a total of 14 doctoral candidates and one comprising seven post-doctoral researchers. The SenS were in four groups (21 researchers in total). The focused group discussions lasted 1.5–2.0 h each and resulted in 128 pages of transcribed text.

Although doctoral candidates are likely to be influenced by the SenS, due to their different disciplinary backgrounds they can also introduce new ideas, theoretical frameworks and concepts. Post-docs have already gone through this process and should be familiar with the scientific standards within their focused perspective of the field, but still lack comprehensive experience of publishing articles in peer reviewed journals. SenS are expected to have a broader overview of the field and a more carefully reflected approach to research due to their experience of journal publications as reviewers or even editors.

There can also be imprinting effects at individual level as some SenS were educated during the sensitive imprinting period of entrepreneurship research (1980s) with its strong focus on relevance.

3.2. Interview guideline

The questions raised during the focus group discussions were based on an interview guideline (see Appendix 1) and first addressed topics such as reliability and validity in order to provide a context for all participants. The discussion then continued with questions about the degree to which the participants considered the aspect of interesting research in their own work, what they considered interesting in recent entrepreneurship research and why, as well as which articles they deemed especially interesting. Having obtained a differentiated picture of what the participants considered interesting entrepreneurship research, the question of what can be done to make entrepreneurship research more interesting was raised. Finally, the participants were invited to summarize and reflect on the discussion.

In qualitative research the interview questions can be modified from one discussion to the next (Krueger 1997). However, we did not do so because we wanted to have the possibility of making comparisons across groups (Krueger and Casey 2009). Moreover, no amendment was necessary as the interview guide worked satisfactorily for both the JunS and SenS groups.

3.3. Analysis

The authors audiotaped and later transcribed the eight focus group discussions. First, the authors (individually) reviewed the transcripts. Next, the authors (jointly) reviewed all transcripts. The purpose of these reviews was to obtain an initial impression of the discussions. Then the authors roughly categorized each focus group's content (Krueger 1997), followed by a detailed analysis of the logic and context of their argumentation. The authors developed the categories independently and then compared them, inviting comprehensive feedback. When differences of interpretation arose, the authors reached agreed-upon evaluations. The intent was to reconstruct the 'rules' respectively 'dimensions' considered typical of interesting entrepreneurship research but not to conduct a quantitative content analysis because such an approach would neglect the richness of the qualitative data generated in the dynamic focus group context. By moving back and forth between the parts of the text, the authors arrived at a clear understanding that provided deep insights into the logic of the argumentation that emerged in the focus group discussions. To provide an accurate impression of the focus group discussions, quotations from various focus group participants in Section 4 are presented (e.g. JunS1: 48–50 = Group 1 of JunS, lines 48–50 of the transcribed interview). These quotes (italicized in the text) are representative of what the participants consider interesting entrepreneurship research.

4. Results of the focus group interviews

4.1. Junior Scholars

The JunS groups generally agreed on most issues. However, the post-doctoral researchers disagreed with the doctoral candidates on a few topics. In particular, they revealed a stronger focus on the career implications of research and publications. Our findings about the characteristics of interesting entrepreneurship research can be described in four dimensions that only partly correspond with the categories identified by Bartunek, Rynes, and Ireland (2006) and Das and Long (2010). These dimensions emphasize the differences between entrepreneurship research and management research:

- Interesting research is subjective.
- Interesting research is novel.
- Interesting research is relevant.
- Interesting research evokes emotional responses.

4.1.1. Interesting research is subjective

Writing a PhD thesis or conducting research as a young post-doc is primarily based on intrinsic motivation. Especially at the beginning of a PhD process there is a lack of overview of the entrepreneurship field and PhD students often have different disciplinary backgrounds. This absence of a clear orientation in terms of recent developments (e.g. 'hot topics') and expectations, i.e. the rules of the game (e.g. the quality standards of journals) as well as the time involved in a doctoral dissertation contribute to the decision to choose a topic and method that are considered interesting from a personal point of view and linked to the individuals' disciplinary background. Such a decision is of major relevance for the motivation to face the challenges of a PhD project, which is mainly a personal undertaking in order to develop scientific skills and competencies. PhD students might be driven by such reasons: *I think you start out looking at what you think is interesting (JunS1: 26). What is interesting to you is not necessarily interesting to other people (JunS2: 6–7). I think I have thought of interesting as a motivating factor, interesting for me (JunS3: 101–102).*

However, JunS are embedded in a specific institutional environment, i.e. research groups and departments. Such a context incorporates expectations that typically go beyond subjective interests. Consequently, these statements were relativized: *After developing your idea you check whether other people find it interesting. That also raises the issue of whether you can be a strategic researcher. Can you research something that you don't think is interesting but that other people might find interesting? (JunS2: 26–29)*. In addition, the pressure on the JunS to publish may create tension between these intrinsic and extrinsic (strategic) motivations. They may wish to research topics of personal interest, but they recognize they must write articles that journals will publish. G*etting published is not the same as interesting (JunS2: 171).*

Turning to interesting publications, JunS focus to a high extent on works that help them to develop their own research. The argumentation is fairly pragmatic, because publications are used for generating and constructing a story line in their own study with its own logic that leads to a publishable paper or a PhD thesis. This is not surprising, but on the other hand the idealistic notion that research has to be considered important and meaningful from a personal point of view is clearly modified in the direction of being helpful in order to keep the research process running. *To be interesting, it should help me in my own research (JunS1: 6–7). I'm looking for two things, first a paper that helps me to write another paper, second something that involves a theory or that can give me a theoretical base to justify my ideas or helps me to construct the story of my own paper (JunS1: 15–18). And (…) interesting is something that is very relevant to what I'm doing (JunS3: 53–55).*

4.1.2. Interesting research is novel

The JunS find studies interesting that introduce them to new ideas and new research areas. However, novelty is relative. Some research shapes or frames a research field by presenting a new perspective or focus, while other research produces relatively little that is new. The JunS identified articles by Gartner (1988), Shane and Venkataraman (2000), and Sarasvathy (2001) as highly influential. These articles offer new perspectives on entrepreneurship research, for example, define the field's essential core, use 'opportunities' as a key concept, and introduce an effectuation logic. The JunS consider that conceptual articles such as these shape the field because of their broad applicability. *Such papers are (…) about the concept, about the definition, about the boundaries of the field (JunS2: 90). I don't even know if you can plan it. But this is what I find most interesting when I read a conceptual paper. It introduces a completely new perspective, although it happens very rarely (JunS4: 166–168).*

Moreover, research that generates new scientific findings, despite being based on a narrow research question that may have a lower overall impact, is nevertheless considered interesting. Such papers usually address specific topics. *The other thing is that interesting is when it introduces something new and at the same time challenges and helps me to develop a paper in a novel way (JunS1: 18–19). For me it's very much novelty, the 'wow factor' that adds something unexpected and contributes to my knowledge in some ways (JunS3: 87–88). It's essentially about telling me something that I not only don't know but that I never really thought about (…) so it contributes something new, either in the way we think about things or how we analyse them (JunS4: 48–50).* These statements clearly highlight a major goal of most research to reveal something new.

Another aspect, albeit one that does not change the roadmap of the field in the same way as conceptual or empirical papers that contain surprising findings, concerns structuring a certain research area by synthesizing existing research: *Following 50 papers that have made fairly small contributions, there is one that sums them all up and can really nail and pinpoint the one or two simple truths that emerged. But they don't work without the other 50, so you need the other 50 before you can sum them up (JunS4: 182–185).* Such literature reviews should not only offer a structured overview and draw conclusions about what we know of a certain area but reveal research gaps or problematize existing research*: (…) papers that sum up what has been done already and raise new issues for the future, changes for the future (…) (JunS1: 26–28).* Such papers provide valuable orientation by enhancing the learning and research process of young scholars and are therefore considered interesting research.

Furthermore, the JunS think entrepreneurship research benefits from theories, concepts and methodologies developed and tested in other disciplines. Generally speaking, they favour the cross-disciplinary fertilization of different research areas. This acceptance is the result of their previous education and of the general receptivity of entrepreneurship research to new ideas. *(…) some of these interesting papers are from elsewhere but are embraced by the entrepreneurship field (JunS2: 420–421). The very nature of entrepreneurship seems to (…) facilitate cross-disciplinary research that (…) may spark something new and interesting (JunS3: 253–254).* They also think cross-disciplinary fertilization creates possibilities to give something back to other disciplines.

4.1.3. Interesting research is relevant

Most JunS clearly favour relevance over rigour in research, despite the advances rigour has made in entrepreneurship research as an academic discipline. *As entrepreneurship scholars we* *want to make positive changes in the world (…). We have to research something practical, which will help improve society in some way (JunS4: 7–9)*. The JunS, in particular the post-doctoral researchers, want their research to reach audiences outside academia such as policy-makers and the entrepreneurs themselves. The JunS are aware, however, of the potential contradiction between rigour and relevance. *Because (…) you want to advance your career, the easiest way is to publish mainstream research (…) (JunS4: 643–645).*

The JunS recognize the duality of audience interests in their research. This duality influences the rigour–relevance debate: *(…) there are a number of groups that have different interests and some might find what you do interesting and others might not. It's always like that (JunS2: 82–83).* The JunS are aware that *(…) two different orientations exist, two different outlets for publications (…) so you can publish the same research in an academic style with more emphasis on rigour (…) and then you publish it in business magazine in a style for practitioners (JunS4: 90–94)*. These comments pinpoint the theory–practice division that is at the heart of the rigour–relevance debate. However, the JunS believe that rigorous research can be translated into a more practitioner-oriented form. They admit that several researchers (e.g. Sarasvathy in her research on effectuation) have overcome the theory–practice division.

4.1.4. Interesting research evokes emotional responses

Presentation and writing style are important in making entrepreneurship research interesting. Published research must be thought-provoking, sell its argument clearly and evoke a response of some kind in the reader. *Interesting research (…) evokes an emotional response. It makes me angry, makes me smile, or makes my head spin (JunS3: 22–24)*. Another way to arouse emotions is by provocation: *If you are provocative you engage the reader emotionally. So you have to create a relationship with the reader (JunS3: 276–277)*. Writing style is influential in provoking responses. If an author's argument or claim is easy to follow, the reader is more willing to engage with it. *I also think it's important to simplify so that the reader goes away with just one message. That's the key (JunS2: 407–408)*. These statements point to an article-driven research culture that stresses the need to create a unique selling point in order to make the research interesting.

4.1.5. Summary: the Junior Scholars

The focus on interesting research that extends beyond personal interest is fairly low at the beginning of the PhD process. The prevalance of intrinsic motivation of the PhD students does not facilitate a strengthening of the dimension of interesting research. Consequently, the selling of research results and specific measures such as writing style are stressed later on in the PhD process. In addition to the communication aspect, novelty and relevance are intensely discussed. Surprisingly, aspects such as generalizability, robustness and rigour were almost completely neglected.

The JunS in all of the focus groups discussed interesting research, but very few works were generally considered interesting. However, JunS described two kinds of interesting research: conceptual research that influences the entire field and specific research that relates to their sub-fields. They recognize that the field is dominated by empirical research and therefore conceptual papers tend to receive greater attention and will be regarded as interesting.

4.2. Senior Scholars

Compared to the JunS, the SenS focus groups took a more critical view of contemporary entrepreneurship research, focusing to a large extent on the evolution of the field, with less emphasis on individual research achievements. In this respect, the SenS focus groups elaborated on the following three dimensions:

- Entrepreneurship research requires new routings.
- Interesting research is relevant.
- Interesting research is novel, challenging and important.

4.2.1. Entrepreneurship research requires new routings

To achieve legitimacy as an academic field, the SenS considered that entrepreneurship research has begun to conform to the norms and standards of other academic fields by adopting a 'normal science' approach. In this institutionalization process, there is always a risk that entrepreneurship research will focus increasingly on narrow research questions. Such research, driven by the academic rewards system, may add little to what we already know. *Many conference papers are nicely done, are rigorous and accumulate knowledge (…) but very few are interesting in the sense of contributing something new (SenS1: 560–565). We are seduced by journals' influence and try to replicate what appears in these journals (SenS4: 425–426).*

Over time there has been a stronger focus on robustness in entrepreneurship studies, reflected in the use of larger samples, pre-tested variables and sophisticated statistical analyses. Robust studies are of course important. Research cannot always be novel because that would result in increasing fragmentation and lack of knowledge accumulation. Robust research creates generalizable and cumulative knowledge that leads to stronger theories. The institutionalization process favours gap-driven research, an inward looking development and has a tendency to lose contact with real world issues due to the focus on increasingly smaller problems, thus fails to take external audiences (e.g. entrepreneurs, policy-makers) into consideration. *We dig deeper and deeper (…) tiny little things (…) rather than really grasping what is going on (…) in the world (SenS3: 117–120). We are to a certain extent in an ivory tower. I mean, we are very focused on our own small concerns, ignoring the rest of the world (…) sometimes I think we are lost in our research, in our academic world (SenS3: 172–175).*

During the institutionalization process the incentive systems changed and today there is strong pressure to publish in top-ranked journals. Thus, the evolution of the field will to a large extent be driven by top-ranked journals and the norms they impose. In this respect, research becomes focused on earlier studies and researchers only publish for the sake of publishing. Some of these concerns are reflected in the following statements: *We tend to pick on research topics and issues by looking at journals because it's imperative to publish and the way to publish is to look at what other people have published. Therefore, the whole field takes off in a certain direction (SenS4: 16–20). I believe that policy-makers and governments hope to make universities better. There is a real risk that this could be counterproductive because articles are written for the sake of publication, not for any other reason (SenS3: 262–265). I just wondered: Is something wrong with entrepreneurship research or with the top journals? They are two different things. I think there is an obligation for us, as reviewers, as well as for journal editors, to recognize the difference in those two things.*

4.2.2. Interesting research is relevant

As a research field, entrepreneurship is strongly rooted in practice and policy-making. If the field is to be interesting, the SenS think it should address real world problems and engage in a dialogue with various audiences. *We need to think about the research agenda, which must be closer to the real world. I think as the field developed, we somehow became a bit distanced from the actual object of study (SenS4: 29–32). We are not 2–3 years behind reality, but probably almost 10 years behind (…). Mainstream entrepreneurship research is the result of the past (…) but it provides a better chance of being published (SenS4: 312–315). Sometimes when you listen to paper presentations you wonder, how will this piece of research actually help that poor entrepreneur who is trying to figure out how to run his business, it has no relevance (…). It's even totally uninteresting (SenS3: 160–164). When you read articles in top-ranked journals and come to the last pages where the implications are presented, you find some implications are really stupid. If you know anything about the real world, they are really stupid (SenS3: 317–320).*

Some of the SenS, however, are concerned about the focus on relevancy in research. As researchers, they see their goal as being to contribute to a more general understanding of phenomena and not to solve the everyday problems of the entrepreneur or politician. *I talk to entrepreneurs every day and they ask me how to manage their business, how to get money from the banks (…) so many practical problems. I have two answers: I connect them with people who can help them, which is the practical answer. And I have another answer (…) I want to generalize from behavioural patterns so I can give then the institutional answer (SenS3: 330–346).*

For the SenS, the key issue for relevant research is the formulation of research questions that can solve real problems. *I have just one difficulty: How to formulate the research question? This is the key point. (…) problem-setting is, in my opinion, more important than problem-solving. Most people see the practical problem but not the conceptualization that frames the practical problem in a larger perspective. I think the key point is to overcome the gap between practice and theory (SenS3: 330–346). I feel that if we were closer to entrepreneurs we would understand relevancy better. Then we would apply the appropriate theoretical lenses that would contribute relevant knowledge (SenS4: 129–131).*

In the theory and practice discussion, the SenS stated that more theories should be developed in entrepreneurship research. In many cases, the discussions ended with mention of Saras Sarasvathy's effectuation approach, which was highlighted as a good example of integrating theory and practice but also as useful in teaching settings. *Effectuation is anchored in reality and appeals to many different audiences. When I'm talking about effectuation to executives, it speaks to them. They can easily understand the usefulness of effectuation as a theoretical framework. These theories are more interesting (SenS3: 139–145). One paper that always sticks in my mind is the one on effectuation. I remember that year the students were really frustrated with what I was presenting. They were older students who wanted something really practical and what entrepreneurs actually do. They just didn't want what I was sharing with them (…) and I was aware that this isn't what entrepreneurs really do (SenS2: 48–54).*

4.2.3. Interesting research is novel, challenging and important

The SenS stated that entrepreneurship research should be novel and provide new insights that change the views of readers. *What makes entrepreneurship research interesting? I believe it's something unexpected (SenS1: 532–536). It's about new insights that will really change perspectives (SenS2: 61).* The SenS raised the following issues (that the JunS had neglected):

- Strong connections with context: In entrepreneurship research, the context is often taken as given, but researchers should be aware that failure to take the context into account may result in contradictory findings. *What I find interesting is the context (…). Differences between countries, within families and between generations. Many contexts have to be taken into account in entrepreneurship studies (SenS4: 323–325)*.
- Connection to mainstream disciplines: Mainstream disciplines have frameworks that can facilitate problem-solving as well as inspire research. *I often look outside the field for new inspiration because I think there are many insights that we can use in an attempt to expand how we think about entrepreneurship. (SenS2: 108–110). I'm a great believer in the permeability of the field. I don't want to live on an entrepreneurship island (SenS4: 26–28). It's the right field for a multi-disciplinary approach. We can draw from various core disciplines (SenS4: 194–195)*.
- Development of methodologies for entrepreneurship research: in order to achieve progress and make the field interesting in the future, methodology must be improved. According to this line of argumentation, it involves developing methodologies that offer the possibility of capturing the essence of entrepreneurship. This requires more qualitative research, more longitudinal studies and more multilevel analyses.

The SenS were of the opinion that existing assumptions in contemporary entrepreneurship research should be challenged. They agreed that entrepreneurship research is anchored in certain assumptions about how society works. For example, the research focuses on (a) growth, high-tech companies and innovation, (b) Western society, (c) the individual and opportunity nexus and (d) assumptions about actors' rational behaviour. However, entrepreneurship researchers should challenge this focus by studying a wide range of industries, by exploring the dark side of growth, by focusing on countries that do not perform well in terms of innovation and economic growth and by accepting that entrepreneurship research also involves the investigation of irrational behaviour. *We focus strongly on radical innovations, innovative business models, high-tech industries. (…) But I wonder why we don't pay more attention to existing companies that are stable and have a huge potential to modernize and renew their activities (SenS1: 183–189). Growth has largely been about two things – mortgaging the future and exploiting resources (…) (SenS1: 229–230).*

Finally, to be interesting, entrepreneurship scholars need to study the important issues in society. Academic entrepreneurship research has neglected the necessity of having a 'bird's eye view' of changes in society and has instead focused on discussing small research 'gaps' that make very limited contributions to society.

Specific works and topics that could be regarded as interesting in entrepreneurship research were discussed in the SenS focus group interviews. In the same way as in the JunS interviews, the results reveal a fragmented picture. The only works that received more than a single 'vote' were Sarasvathy (2001), Baumol (1990) and Shane and Venkataraman (2000). The other suggestions seemed rooted in the interests of the individual scholar and included old theoretical discussions, e.g. Kirzner (1973) as well as more contemporary contributions such as Grandori and Gaillard (2011).

4.2.4. Summary: the Senior Scholars

In general, the SenS were experienced scholars who had been involved in entrepreneurship research for a long time. It was therefore to be expected that they discussed the evolution of entrepreneurship as a research field. However, what was somewhat surprising was the fact that many SenS were essentially critical of contemporary research and frustrated with regard to its progress, despite the fact that they themselves have been co-creators of the field. Lack of relevance was mentioned as a main concern, i.e. the failure to research relevant questions or adequately disseminate our knowledge to practitioners, policy-makers, etc. The lack of relevance was also emphasized by the JunS. However, there is a paradox in that many of the SenS who participated in the focus group interviews are influential within the field as reviewers, editors of journals, supervisors of PhD students and directors of research centres, and thus could contribute to finding a balance between rigour and relevance.

5. Discussion

5.1. Characteristics of interesting entrepreneurship research

Analysis of JunS and SenS revealed a fairly similar but in some respects a different picture of what is considered interesting. JunS deem research interesting when it has a personal flavour, is well crafted in the sense that it has a story to tell that evokes the reader's emotions. The SenS are more focused on the field as such and consider research interesting when it broadly reflects issues that challenge existing knowledge and contributes something important to society.

As shown in Table 2 both groups of scholars agree that novelty and relevancy are key issues. Novelty is an obvious characteristic of interesting entrepreneurship research. To be interesting, the research should have a high degree of dynamic renewal characterized by the choice of new research topics, methodological approaches and theoretical frameworks, as well as unexpected research findings (e.g. Gartner 2013). Novelty is reflected in the following: research that creates new directions, i.e. provides new perspectives (e.g. Sarasvathy 2001), defines the core of the field and proposes new opportunities for future research (e.g. Gartner 1988; Shane and Venkataraman 2000), uses theories and methodologies from other fields, challenges existing assumptions and tackles important societal problems. Novelty fits to the findings of Bartunek, Rynes, and Ireland (2006) and Das and Long (2010). However, due to the methodology employed that stimulated reflection and creativity, we present a more elaborate picture of the aspect of challenging current practices in entrepreneurship research, which corresponds with Davis (1971). The entrepreneurship researchers who participated in our study especially challenge the prevailing focus on rigour and demand that entrepreneurship research becomes more relevant. Furthermore, the studies by Bartunek, Rynes, and Ireland (2006) and Das and Long (2010) only addressed researchers as 'stakeholders'. In our study, although we only interviewed researchers, external stakeholders, i.e. practitioners and policy-makers, were addressed extensively during the interviews, thus making the path for the rigour–relevance issue which, consequently, is an important part of our reflections on potential implications for entrepreneurship research.

Whereas novelty is an undisputed aspect of interestingness and an obvious part of research, the rigour–relevance topic is more challenging. Since its emergence as a research field in the 1980s, entrepreneurship research has been largely practice-oriented. This practical orientation, which had a strong, externally driven legitimacy, became a major imprinting element. However, as the field became increasingly institutionalized, its basis for legitimacy changed. Entrepreneurship scholars now stress that research has become more and more

Table 2. Interestingness dimensions of entrepreneurship scholars.

Junior Scholars	Senior Scholars
Focus on individual interestingness	Focus on the interestingness of the field
Interesting entrepreneurship research is subjective • Creating a balance between intrinsic interests and institutional expectations	Interesting entrepreneurship research requires new routings • The institutionalization of entrepreneurship research, reinforced by the academic incentive system, makes the research gap-driven and less related to real world issues
Interesting entrepreneurship research is novel • Works that (1) define the core of the field, (2) generate new perspectives and surprising findings, (3) synthesize a research area and (4) use theories and methodologies from other fields	Interesting entrepreneurship research is relevant • The field is rooted in practice and policy, thus there is a need for a stronger focus on relevance and the integration of theory and practice
Interesting entrepreneurship research is relevant • Different audiences and a need to overcome the theory-practice division	Interesting entrepreneurship research is novel, challenging, and important • Entrepreneurship research needs to be (1) connected to context, (2) connected to mainstream disciplines, and (3) based on methodologies that capture the essence of entrepreneurship • Contemporary entrepreneurship research needs to be challenged and focused on important issues in society
Interesting entrepreneurship research evokes emotional responses • Presentation and writing style that provoke the audience	

devoted to solving an academic puzzle and has relinquished its quest to address problems at societal, firm and individual levels. Entrepreneurship research today bases its status as an academic area on the number of academic publications in highly ranked journals (Pearce II 2012). Thus, academic legitimacy has replaced external legitimacy. It is surprising to note that when entrepreneurship scholars search for interestingness in the future they look to the past – the restoration of entrepreneurship as a practically oriented research field. It was obvious in the focus group interviews that entrepreneurship scholars experienced a strong rigour–relevance gap frustration. Because both groups strongly emphasized the relevance dimension of research, we will discuss the issues in more detail.

5.2. Rigour–relevance gap frustration

As the main indicator of academic quality rigour can be more easily evaluated because researchers are trained in scientific methods and the identification of research gaps but are often less familiar with evaluating the relevance of research (Flickinger et al. 2014). Furthermore, relevance is often not evaluated as part of the research performance either at individual or university level. In addition, the gap-driven approach to research strongly contributes to the self-referential closure of academic research. A gap-driven approach leads to new research that is thematically linked to the gap, which in turn generates another gap that leads to research targeting this new gap and so forth (see also Hirschheim and Klein 2003, 260). The risk inherent in this approach is that ultimately the gaps addressed become irrelevant and even practitioners with an academic background consider such gap-driven research results more and more meaningless. The change in the basis of legitimacy from external to academic stakeholders has resulted in a rigour–relevance divide in entrepreneurship research. Indeed, as demonstrated by our empirical results, interesting entrepreneurship research is strongly influenced by strengthening the relevance, but the rigour demanded by top-ranked journals that set the main professional norm has led to less focus on relevance. This perception seemed much stronger among the group of entrepreneurship researchers who participated in our study compared to the findings of Bartunek, Rynes, and Ireland (2006) and Das and Long (2010).

The key argument in the reasoning about the rigour–relevance divide is that the knowledge accumulated in top-ranked entrepreneurship journals cannot be transferred to practice because of the differing logic of science and practice (Kieser and Leiner 2009). Therefore, the connectivity between scholarly entrepreneurship research and entrepreneurship practice is low, something that was clearly evident in the discussions. However, the evaluation of relevancy may require more than a look at individual studies but a broad overview of a larger number of studies that can reveal a coherent and relevant pattern of results. Thus, as argued by Hodgkinson and Rosseau (2009), high scientific standards and practical relevance are not necessarily conflicting goals, i.e. it is possible to bridge the gap between theory and practice. This may also be a strong argument for entrepreneurship research, which has become a well-established research field within the canon of academic disciplines and can now put more emphasis on relevance and bridging the theory–practice gap. However, evaluating the relevance of research results is not an easy task and it is a moot point whether researchers or practitioners are best suited to evaluating the relevance of academic research. Kieser and Leiner (2011) argued that only practitioners 'can ultimately assess the relevance of research by applying solutions derived from research results' (891). In addition, Silvia (2006) claims that an individual's appraisal of what is regarded as interesting depends on skills, knowledge and resources to deal with a problem ('coping-potential'), and that entrepreneurship scholars need to have an understanding of the abilities of their audiences to deal with an issue or problem. Consequently, the dialogue between entrepreneurship researchers and practitioners must be significantly intensified and forums created to enhance beneficial exchange (see also Van de Ven and Johnson 2006).

We can conclude that there is significant rigour–relevance gap frustration among entrepreneurship scholars on the evidence of those who participated in our study. A closer connection with the 'real world' would be valuable and will make entrepreneurship research more interesting. But one has to be cautious not to be considered naive, because as academic entrepreneurship research and entrepreneurship practice operate in different societal contexts, establishing connectivity is a challenge for both sides. In the following section, we reflect on some aspects that may contribute to overcoming the rigour–relevance gap in entrepreneurship research and thus make entrepreneurship research more interesting.

6. Implications for entrepreneurship research

We structure this section into three strategic issues for entrepreneurship research; (1) types of knowledge creation, (2) institutional strategies and (3) process strategies.

6.1. Types of knowledge creation

Bridging the rigour–relevance gap is not a trivial issue as demonstrated by Kieser and Leiner (2009), who argued that practitioners and researchers belong to different systems with varying sets of logic. Most discussions have been focused on the creation of a closer collaboration between researchers and practitioners/policy-makers, which is in line with Mode 2-research

(Gibbons et al. 1994). However, we will emphasize another key feature of the rigour–relevance gap that is often neglected: namely, the many types of knowledge.

When talking about knowledge that is relevant for practitioners, we have to be aware that there are different types of knowledge (Hirschheim and Klein 2003): (a) technical (rules of skill) knowledge, (b) normative (ethical) knowledge, (c) theoretical (explanatory-predictive nature) knowledge and (d) applicative knowledge (i.e. problem-solving and action-oriented). Applicative knowledge, which was implicitly targeted in the focus groups, does not neglect theoretical knowledge, but reaches beyond the knowledge generated by means of hypothetico-deductive methodology, which is the dominant approach in entrepreneurship research today and closely linked to the prevalent understanding of rigour. Applicative knowledge cannot be acquired by only generating data with standardized online questionnaires and analysing it with statistics programmes, or reading journal articles and books. It requires a close connection and exchange with practice, leading to researchers with 'seasoned experience' (Hirschheim and Klein 2003, 265), thus incorporating a more holistic and hermeneutical approach that reaches beyond pure cognitive knowledge. It is based on a co-created exchange between entrepreneurship researchers and practitioners, i.e. developing competences that relate theory to practice and vice versa. In the best case, people with experience of both research and practice can facilitate the development of applicative knowledge.

Applicative knowledge is not always explicit or explicable but tacit. Tacit knowledge, which is the result of personal experience and learning processes from successes and failures, often creates a special know-how. Individuals may not even be aware that they have such know-how. Consequently, there are limits to the verbalization of knowledge because 'we know more than we know how to say' (Polanyi 1958, 12). The knowledge that practitioners have may be such tacit knowledge. Surprisingly, practitioners do not need to know what they know and do not need to explain how they do what they do (Willke 2011, 43). While tacit knowledge is an important part of many entrepreneurship activities, entrepreneurship researchers have rarely dealt with it. This oversight points to the need for cooperation between entrepreneurship researchers and practitioners as well as a limitation to overcoming the rigour–relevance gap.

Furthermore, applicative knowledge is closely related to personal interests (and emotions) and, consequently, leads to a new and different identity as a researcher (Hirschheim and Klein 2003). In order to bridge the rigour–relevance gap we need different kinds of researcher producing different kinds of knowledge. We use this explanation of applicative knowledge in the following suggestions for bridging the rigour–relevance gap

6.2. Creating an institutional context favouring the creation of applicative knowledge

The incentive systems at universities and business schools require reappraisal. Current systems favour rigorous research published in top journals and are based on easily quantifiable measures that often lack a pluralistic conceptualization of incentive systems (Aguinis et al. 2014). In order to be promoted, researchers should be required not only to publish rigorous studies in high-ranked journals, but to demonstrate their capacity to publish practically relevant studies and interact with practitioners and policy-makers. However, every single researcher may not need to strive for excellence in terms of rigour and relevance. Exploring the relevancy potential of rigorously generated research results in collaboration with practitioners could be a first step in a dialogue that ultimately leads to institutionalization. However, entering into dialogue with practitioners requires communication strategies that enhance the exchange between academia and practice (Steffens et al. 2014). Because such exchanges are both resource and time consuming, and require a reorientation of performance indicators, such systems can only be implemented at organizational level. Obviously, the success of such an approach also depends on practitioner interests and capabilities as well as the quality of the researcher–practitioner relationship (Lambrechts et al. 2011).

The often chosen quantitative hypthetico-deductive methodology requires re-examination. This perspective often leads to the creation of a closed academic arena that is more or less sealed off from the concerns of practitioners (Binks, Starkey, and Mahon 2007), although several editorial notes from journal editors ask for rigour *and* relevance (e.g. George 2014). Entrepreneurship, which at its core is about change, involves complex, diffuse and sometimes messy research investigations. A quantitative methodology creates a reality that differs from objectively reproducing it, which is its core claim. The attempt to generate clear descriptions and explanations of phenomena that are not very coherent and even in a constant state of flux simply increases the mess (Law 2004). However, the prevailing institutional context may favour the quantitative approach and thus contribute to undervaluing the more complex, self-referential phenomena that also form a part of the entrepreneurial process. Consequently, more methodological open-mindedness is required to overcome the rigour–relevance gap in entrepreneurship research.

Doctoral programmes in entrepreneurship research should place greater focus on the issue of interesting research and be more methodologically open-minded. Often such programmes focus on rigour but devote little attention to how doctoral candidates can produce interesting research. Supervisors can stimulate new thinking on thesis topics, offer doctoral courses on researcher–practitioner collaboration and point out that literature-based research often suppresses practical relevance. Professionally qualified doctoral students (i.e. students with practical experience in their dissertation area) can be supported as role models who can help bridge the rigour–relevance gap (Klein and Rowe 2008). Involvement of this type of researcher may enhance the discovery of empirical irregularities or puzzles that challenge existing theories, leading to new concepts and theories (Clark and Wright 2009).

6.3. Process strategies

Entrepreneurship researchers can reduce the gap between scientific rigour and practical relevance by modifying how they identify research issues, construct research questions, conduct their research, use theories and disseminate results (Wolf and Rosenberg 2012). First of all, we have to acknowledge that practitioners are a very heterogeneous mixture (e.g. policy-makers, founders of new businesses, venture capitalists, etc.) and may have different ideas than researchers about what makes research results useful and applicative. When researchers do not differentiate among practitioners, their strive to generate applicative knowledge can be too vague and limited. We need a better understanding of the different groups of practitioners. These knowledge deficits are an expression of the underdeveloped relationship between researchers and practitioners (Bartunek and Rynes 2014; see also Hirschheim and Klein 2003). Exploring the expectations that practitioners and entrepreneurship researchers have of each other would be a fruitful starting point for bridging the rigour–relevance gap.

Creating applicative knowledge based on a closer relationship with different types of practitioner provides opportunities for new methodological approaches. Practice-oriented research approaches in entrepreneurship research have been rare. However, a few attempts have been made, for example, in narrative studies (cf. Gartner 2007; Hjorth and Steyeart 2004), interactive approaches (cf. Aagaard-Nielsen and Svensson 2006) and in enactive research projects (cf. Johannisson 2011). Enactive research as an interactive way of conducting field research would enable entrepreneurship researchers to create practice-driven knowledge that benefits practitioners who have a partner with theoretical reflection competence and at the same time the researcher connects with someone who has action competence. The merging of these processes may enhance the creation of applicative knowledge, and especially the development of qualitative and longitudinal research case studies. These kind of studies are needed in order to address the contextual complexity and unique characteristics of entrepreneurship (Zahra 2007). Qualitative research emphasizes the richness of the phenomenon and typically better connects with how practitioners analyse problems and design actions to solve these problems.

Obviously, no methodological approach is a priory superior to another, and we need and can learn from different methodological approaches. However, when employing a quantitative methodology we suggest that entrepreneurs, practitioners and policy-makers need to be more involved in the problematization and first conceptualization phases of research projects (Wolf and Rosenberg 2012), as it will provide a stronger focus on actionable variables, i.e. variables that practitioners and policy-makers can influence and act upon (Kenworthy and McMullan 2013). A dialogue between practitioners and/or policy-makers throughout the research process may help to ensure a better understanding of results and provide 'practical' interpretations (feedback) of the research findings (Silvia 2006). In order to increase the external validity of studies, literature reviews and replication studies are potentially useful tools in establishing the practical relevance of previous research. This includes an examination of the recommendations in studies that have successfully bridged the rigour–relevance gap. More high-quality replication studies that examine and test the findings of previous research studies are needed. Too many findings in entrepreneurship research are only developed and/or tested in one study. Thus, we need a stock of robust entrepreneurship knowledge that practitioners and policy-makers can use.

6.4. Concluding remarks

We believe that these measures can contribute to creating knowledge with a higher degree of applicability, thus increasing the relevance of entrepreneurship research. The challenge for researchers is to follow these strategies. Without practical experience, researchers may be reluctant to work closely with practitioners. They may also question whether following these strategies will advance their professional careers and reputations. Entrepreneurship researchers who are frustrated by the rigour–relevance gap have reason to work towards a change in how entrepreneurship research is conducted and evaluated. Where once it was a practical academic field that obtained legitimacy from external stakeholders, today entrepreneurship research is an increasingly institutionalized field legitimized by highly ranked journals and the ivory towerism of academia. If entrepreneurship research is to be more interesting, and therefore read and used, its applicative knowledge should be developed. The strategies we describe in this paper can help entrepreneurship research to regain relevance and thus increase the variety of types of knowledge in our field. If followed, such strategies may reduce the dominance of rigour with its inherent focus on adding single research constructs. That approach has produced the paradoxical development whereby entrepreneurship research has become so homogenized that it targets a very small audience of researchers, despite generating a dazzling variety of findings that are, unfortunately, barely connected to reality (Schultz 2010).

Future research could address a set of different research questions. First, more elaboration of the concept of applicative knowledge is needed. It might be useful for both sides of the rigour–relevance gap to explore different aspects of applicative knowledge in the context of various external stakeholders. In addition, entrepreneurship research should strive to provide more interesting concepts. Conceptual thinking can be stimulating for practitioners when framing problems. Ambiguous concepts may be very valuable for practitioners because they can be flexibly employed in different problem settings (Astley and Zammuto 1992). Second, the differing expectations of external stakeholders (e.g. policy-makers and entrepreneurs) must be explored. A starting point could be to establish groups of homogeneous stakeholders and analyse their expectations beyond a purely technical level. Third, entrepreneurship research has generated a great deal of highly influential research (in terms of citations, i.e., 'citation classics'; Landström, Harirchi, and Åström 2012) within the field. Practitioners confronted with the problems dealt with in these papers could evaluate the research with regard to its practical relevance and develop ideas about how to improve relevance.

We are aware that this study has several limitations and therefore call on other entrepreneurship researchers to examine our findings and ideas. Owing to our choice of methodology and the exploratory nature of our research, we had a fairly small group of participants although the composition of the groups may adequately represent the characteristics of entrepreneurship scholars (doctoral candidates, post-doctoral researchers, professors, institute directors, editorial board members and journal editors). Based on the focus group dynamics, an over-critical view may have emerged in the interviews because it is easier to be critical than to elaborate on the strengths and benefits of research.

We hope our research stimulates reflection and lively discussion about the future of entrepreneurship research that addresses the diverse interests of practitioners and researchers, thus helping to make entrepreneurship research more interesting in terms of relevance and novelty. However, we emphasize that not all entrepreneurship studies need to place the main focus on relevance.

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Appendix 1. Interview guidelines

Participants should personally know each other to a certain degree, i.e. there should be the possibility for an open and real discussion, and not the mere stating of different opinions.

Questions should be asked in a conversational manner in order to create an informal atmosphere. A basic element is to use the conversational dynamics of focus groups, i.e. besides explicit questions the moderator should be aware that 'immanent questions' are very important. Immanent questions use interesting statements made by the participants and re-phrase them in order to obtain a deeper understanding of what was said. These questions are equally or even more important than those prepared before the discussion.

Introductory question:
Entrepreneurship research should address important topics, be valid AND interesting. What do you think about this?

Transition question:
To what degree do you consider the aspect of 'interesting research' in your own research?

Key questions:
What do you consider really interesting in recent entrepreneurship research and why?
Which strategies or principles were applied that make this research interesting from your point of view?
Nominate up to three entrepreneurship research articles over the past 30 years that you regard as particularly 'interesting'. Why do you consider each of these articles particularly interesting?
What can we do to make entrepreneurship research more interesting in terms of topics, theories, and methods?

Concluding questions:
How would each of you summarize the discussion? What were the main points?