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## **Editors' Introduction: Low and MacMillan Ten Years On: Achievements and Future Directions for Entrepreneurship Research**

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In 1988, Murray Low and Ian MacMillan published a review of developments in entrepreneurship research and identified challenges for the future (Low and MacMillan, 1988). Since the appearance of their paper, there has been an explosion of entrepreneurship research. This Special Issue had its genesis a decade after the appearance of the Low and MacMillan article, when nineteen scholars from nine countries were invited to two workshops held at the Jönköping International Business School (JIBS) in Sweden in the fall of 1998. The purpose of the workshops was, and the purpose of this Special Issue is, to take stock of the developments of the past decade and to identify directions for future research.

The timing of our bringing together this collection of papers is also motivated by an atmosphere of continuing uncertainty and unease relating to just what entrepreneurship research is about. This unease is reflected in a recent paper by Shane and Venkataraman (2000) which refers to current entrepreneurship research as a 'hodgepodge'. They observe that to many people, it is not clear that the field is explaining phenomena beyond what is known from work in other fields. Yet at the same time, entrepreneurship is attracting increasing attention in mainstream journals. The *Academy of Management Review*, *Strategic Management Journal* and the *Academy of Management Executive* are each at the time of writing devoting Special Issues to aspects of entrepreneurship.

Low and MacMillan divided their review of entrepreneurship research according to six design specifications: the purpose of the research, the choice of theoretical perspective, the focus of the

phenomena to be investigated, the level or levels of analysis, the time frame of analysis and the methodologies used. This same organizing framework was used for this Special Issue.

The first two papers, by Low and Gartner, present commentaries on the general state of entrepreneurship research. In “The Adolescence of Entrepreneurship Research: Specification of Purpose,” Low argues that the field has not come far enough, fast enough. He discusses four alternative strategies for entrepreneurship research. Low searches for the balance that will maintain the integrative, inclusiveness and practical characteristics of the field while making substantive and lasting intellectual contributions.

Gartner’s “Is there an elephant in entrepreneurship? Blind assumptions in theory development,” tackles the issue of “collegiality without community.” Gartner reviews the recent Shane and Venkataraman (2000) article in the context of the Low and MacMillan specifications. He applauds Shane and Venkataraman for their attempt to create a community of scholars around a set of clearly defined issues. For Gartner, entrepreneurship is about organizing, which he acknowledges is but one of many topics currently studied under the rubric of entrepreneurship. He suggests that entrepreneurship research might be better served by the creation of several scholarly communities focussed on more tightly defined subject areas.

The remaining four papers provide reviews of developments in entrepreneurship research over the past decade. In “Many are Called, but Few are Chosen: An Evolutionary Perspective for the Study of Entrepreneurship” Aldrich and Martinez review the significant progress made during the past decade in applying evolutionary theory to the study of entrepreneurship. By detailing advances made through the application of a single theoretical perspective, Aldrich and Martinez demonstrate the power and potential of a community of scholars examining a clearly defined phenomenon with a common set of conceptual tools. Most exciting is the recognition that many of the advances in evolutionary theory can be attributed to the willingness to learn from related fields. They demonstrate that good scholarship does not have to mean academic silos.

Ucbasaran, Westhead and Wright examine the issue of focus in “The Focus of Entrepreneurship Research: Contextual and Process Issues.” In a broad literature review organized according to six

“themes,” they demonstrate the wide range of subjects that entrepreneurship researchers have chosen as the foci of their research. Ironically, they demonstrate that while individual research projects may have focus, the field as a whole does not. Consideration of additional contexts such as management buy-outs and buy-ins, franchising, corporate venturing and family business, in combination with additional types of entrepreneurs such as nascent, novice, serial and portfolio, pose questions of context and process that broaden, rather than narrow, the field. Importantly, while arguing for inclusiveness, they conclude that narrowly defined studies provide the most useful insights.

Davidsson and Wiklund tackle the next issue in “Levels of Analysis in Entrepreneurship Research: Current Research Practice and Suggestions for the Future.” This paper reviews the research of the past decade and finds few have heeded Low & MacMillan’s call for multi-level approaches. Entrepreneurship studies increasingly focus solely on the individual and firm levels of analysis. There are some encouraging examples of studies at the industry, regional, and national levels of analysis. However, almost completely absent are studies that combine levels, examining for example the societal impact of micro-level enterprise. Davidsson and Wiklund provide an insightful framework for thinking about these issues and demonstrate how level of analysis issues are fundamentally linked to our definition of the field.

Finally, Chandler and Lyon review developments in use of statistics, analytical techniques, data sources, levels of analysis and time frame in “Methodological Issues in Entrepreneurship Research: The Past Decade.” They systematically review all of the 416 empirical entrepreneurship articles published in nine leading journals over the past decade. Their careful investigation identifies some exemplary studies of methodological sophistication, and an overall trend in the right direction. However they conclude that Low and MacMillan’s admonition for the field to move away from exploratory studies and towards causality remains relevant. Their comprehensive inventory and commentary will provide a helpful benchmark researchers to design future projects.

Collectively, these six papers provide a compelling commentary on the state of the field. They identify areas of consensus, and areas of continuing debate. In the main body of this introduction we

utilize Low and MacMillan's six themes to synthesize implications of the contributions to this Special Issue for the design of future entrepreneurship research.

## **Themes**

### ***Purpose***

There is considerable debate about the purpose of entrepreneurship research. One dividing line seems to be whether the purpose of entrepreneurship research is to generate knowledge that facilitates the creation of new *economic activity* or new *organizations*. A second issue concerns whether entrepreneurship research should confine itself to micro level issues or if it should aspire also to explain the role of new enterprise or new organizations in societal development.

Low and MacMillan suggested that entrepreneurship research should focus on new enterprise and its role in furthering economic progress. Shane and Venkataraman (2000) consider that the field of entrepreneurship is concerned with the discovery and exploitation of entrepreneurial opportunities, the individuals involved and the modes of action used to exploit the opportunities. These are similar views, focusing on the creation of new economic activity. In contrast, Gartner (this issue and elsewhere) takes the position that entrepreneurship is about organizing and that this has a greater likelihood of being understood through the study of firm creation.

Importantly, both Low and Macmillan (1988) and Shane and Venkataraman (2000) also include in the stated purpose of entrepreneurship a clear interest in societal level outcomes. Others propose a more narrowly defined purpose. One of the alternatives discussed by Low (this issue), 'Entrepreneurship Research as Teaching Support', arguably has no explicit considerations of societal level outcomes. Students' main interests tend to revolve around micro-level issues. In trying to define a manageable task for entrepreneurship, Gartner does not emphasize societal level outcomes. Aldrich and Martinez, while applying a view similar to Gartner's on entrepreneurship as 'the creation of new organizations', start from the more aggregate level interest of sociology and hence find it natural for entrepreneurship to consider societal level outcomes.

Another aspect of purpose concerns what it is that entrepreneurship researchers should ultimately strive to achieve. Gartner (this issue) and Shane and Venkataraman (2000) are probably the prime proponents for developing entrepreneurship into a distinctive domain, i.e., one that predicts a set of empirical phenomena not explained by other fields. Other researchers are clearly less concerned with the distinctiveness of the domain, pursuing various research interests on new and small business, innovation, family business, franchise, spin-offs, buy-outs, venture capital, and so on. These researchers may define entrepreneurship in a manner that fits their research, but such definitions will have little “distinctiveness” and little in common. While it is appealing to be inclusive, entrepreneurship research as potpourri runs the danger of forever keeping the field in second-class status (Low; cf. also Gartner).

There seems to be growing consensus among influential scholars in the field that entrepreneurship research must become more theory driven, taking advantage of progress made in other disciplines and fields. Whether or not this should lead to the permanent establishment of a separate ‘domain’ can be debated. Low argues that although views that entrepreneurship research belongs in the disciplines and that entrepreneurship research is a distinctive domain appear to be fundamentally opposed, they are in fact mutually dependent.

Clearly, there is no agreement about the appropriate scope of the field. The challenge is to create a community of scholars that bring insights from multiple disciplines to investigate a set of phenomena that are not too broad as to defy the notion of intellectual community, nor so narrow as to lose sight of our goal. Shane and Venkataraman may be overly ambitious, and Gartner overly restrictive. While unresolved, there is no matter of greater urgency for the advancement of our field.

### ***Theoretical Perspective***

Low & MacMillan argued in favor of theory-driven research that avoids unstated assumptions. On this point our current contributors agree. However, following from the previous discussion on purpose, it is not at all clear what that theory should be.

Gartner takes the view that with a broad delineation of the field, it is not possible to obtain a comprehensive theory of entrepreneurship. In his view there is no over-arching theory that can connect all the phenomena that are currently studied under the entrepreneurship label. Gartner suggests that scholars in the entrepreneurship field need to choose sides and actively divide into more homogeneous groups or communities of entrepreneurship research scholars. These communities would study more specific topic areas such as corporate entrepreneurship, management succession, organization founding, etc. While appealing at first look, we need to ask ourselves whether this solution would achieve the anticipated result. Would not a community of scholars seeking to understand corporate entrepreneurship face the same challenges of integrating insights from a range of disciplines and fields such as sociology, economics, psychology, finance and decision-making theory? Narrower topics of interest may make the task more manageable, but not fundamentally different. Entrepreneurship, however narrowly defined, will be best informed by multiple theories and disciplines.

Aldrich and Martinez are relatively optimistic about the theoretical and empirical advances that have been made and those that lie ahead, and they clearly demonstrate the advantages of applying a strong conceptual framework with a limited number of well-defined concepts. However, in our view, it would be unfortunate if the 'Evolutionary entrepreneurship researchers' were to develop into a 'club' without much contact with other entrepreneurship researchers who are interested in the same real world phenomena. We would then enter a situation that further emphasized the development of several uni-disciplinary theories of entrepreneurship in isolation. Instead, we need to encourage parallel theoretical development and to encourage opportunities for cross-fertilization. (cf. Ucbasaran, Westhead and Wright, this issue). As noted by Aldrich and Martinez: 'We understand strategic choices and environmental selection processes, but we know far less about how they interact with each other over time.' Hence, theoretical integration is pointed out as an important area for further development.

Gartner is right in suggesting that no theory, no matter how integrated, can deal with all aspects included in the Academy of Management Entrepreneurship Division Domain Statement (cf. Gartner, this issue), or with the full range of topics that appear in leading entrepreneurship journals.

However, this is perhaps more a problem of focus than of theory, which leads us to the next subsection.

### ***Focus***

Low and MacMillan emphasized the importance of examining the context and process of entrepreneurship, giving particular emphasis to the role of networks. Their emphasis on context and process was embedded in a critique of the “trait” approach of psychological determinants. In the last decade researchers have rediscovered the individual. Today however, the emphasis is on behavioral and cognitive issues rather than personality characteristics. There is a recognized need to focus on the discovery and exploitation of opportunities as a key aspect of the entrepreneurial process, which includes consideration of the influence of the individual in this process. (Shane and Venkataraman, 2000).

This rediscovery of the individual, combined with an expanded set of contexts creates new possibilities for research. Ucbasaran, Westhead and Wright argue that the definition of entrepreneurship in terms of the creation of new enterprise provides scope for future research to consider cognitive and behavioral factors relating to entrepreneurs involved in new enterprises beyond start-ups to include management buy-outs and buy-ins, franchising, corporate venturing and entrepreneurial actions associated with inherited family-owned firms.

Ucbasaran *et al.*, observe that while opportunity recognition and information search are critical first steps in the entrepreneurial process, research in this area is limited, especially in relation to how entrepreneurs use the knowledge they have acquired. Connected to the identification and exploitation of opportunities is the need for a greater understanding of entrepreneurs’ learning processes. As the number of habitual entrepreneurs grows, learning processes become even more important for understanding the behavior of different types of entrepreneurs.

This suggests a re-focusing of the field is going on that involves the parallel processes of both broadening and narrowing down. The focus seems to be broadened away from the narrow focus on stable characteristics of individuals who start and run independent business firms (Busenitz and

Barney, 1997; Wright, Hoskisson, Busenitz and Dial, 2000). At the same time, it is increasingly being emphasized that studies of small business, family firms, internal venturing etc. deal with entrepreneurship only if they explicitly address 'new enterprise', i.e., discovery and exploitation of opportunity, in these different organizational contexts.

The rediscovery of the individual raises new questions about the impact of environmental factors. Environmental factors have direct implications for how entrepreneurial opportunities are discovered and what their effects may be. In contrast to Shane and Venkataraman (2000) we take the view that consideration of environmental factors is fundamental to an understanding of the generation of and acting upon entrepreneurial opportunities. Environmental factors may also affect who is involved in entrepreneurial opportunity discovery. Aldrich and Martinez point out that, although there has been some progress in recent years, we still do not know enough about the effect of environmental factors on entrepreneurs and entrepreneurial organizations.

One specific aspect of environmental factors affecting the exploitation of entrepreneurial opportunities relates to capital market restrictions that make it difficult for entrepreneurs to fund the start-up of a new venture. Conversely, the same restrictions may make it easier for individuals in existing organizations who identify entrepreneurial opportunities to exploit them. Asymmetric information problems may, for example, be greater for new start-ups than for later stage investments (Amit, Glosten and Muller, 1990). The development of management buy-outs and buy-ins, which have been shown to involve the perception of opportunities is one manifestation of this issue (Wright, Thompson and Robbie, 1992; Zahra, 1995).

Important though entrepreneurship research that examines context and process is, it is ultimately the outcome of the exploitation of entrepreneurial opportunities that determines the contribution of entrepreneurship to wealth creation. Hence, we would argue that there is a need for performance assessment also to be a key focus of entrepreneurship research. The extent to which discovery and exploitation of opportunities contribute to firm level performance is but one aspect of that. Otherwise we agree with Venkataraman (1997) that relative performance of firms should not be the main focus of entrepreneurship research. Absolute performance of specific discovery and

exploitation processes, on the micro level as well as on the societal level is more in line with the purpose and focus of entrepreneurship research discussed above (cf. Davidsson and Wiklund).

This re-focusing has not yet reached a state of consensus. For example, Gartner suggests a focus on the creation of new organizations, thus putting less emphasis on the extent to which activity is new or innovative and excluding activity in established organizations Aldrich and Martinez can be said to take a middle position, focusing on new [populations of] organizations, but also on the extent to which they are imitators or innovators. As with purpose and theoretical perspective, the same thorny issues arise when making the decision specification about focus.

### *Level of Analysis*

The past decade appears to have seen a decline in the number of studies at the aggregate level (Davidsson and Wiklund, this issue) but this shift in emphasis has not been accompanied by a marked increase in the multi-level approaches suggested by Low and MacMillan. Chandler and Lyon (this issue) find that almost nine out of ten studies focus on one level of analysis, with the majority being at the firm level while just over a third are at the individual level of analysis. Davidsson and Wiklund show that the use of the individual as unit of analysis has remained stable but is likely to have shifted its contents. The marked rise has been in studies at the firm level.

Davidsson and Wiklund caution that this dominance of the firm as the unit of analysis is questionable both from Gartner's and Shane and Venkataraman's (2000) respective views on entrepreneurship. In particular, they call into question studies that focus on small established businesses with the implicit assumption that this somehow captures entrepreneurship. If entrepreneurship is about the creation of new organizations then the use of established firms as the 'cases' in the data matrix is awkward. If entrepreneurship is about discovery and exploitation of opportunity—or 'new enterprise' in Low and MacMillan's (1988) terminology—then the issue of how such processes occur and contribute to firm performance is but one possible avenue for entrepreneurship research. From both perspectives a more ideal study seems to be one that starts with an opportunity (or a juxtaposition of individual and opportunity) and follows its development over time, either into an organization or into new economic activity through whatever mode-of-

exploitation (Shane and Venkataraman, 2000; Ucbasaran *et al.*, this issue). On this basis, studies that seek to measure a nation's entrepreneurial behavior through the creation of new firms or the intention to create a new independent business may understate the true extent of entrepreneurship. This point has implications for international comparisons since regulatory and cultural differences between countries may mean that the pursuit of new opportunities for enterprise take forms other than the founding of a new firm.

A further area of potential mismatch identified by Davidsson and Wiklund concerns the dependent and independent variables used in entrepreneurship research. A case in point is the linking of individual characteristics and behavior to the performance of one particular venture or firm. Davidsson and Wiklund suggest that individual characteristics and behaviors be related to 'entrepreneurial career performance' or something approaching that ideal. Here, the research on serial and portfolio entrepreneurs reviewed by Ucbasaran *et al.* is a step in the right direction.

Low and MacMillan (1988) raised the question of multi-level approaches to the study of entrepreneurship. Such approaches are particularly demanding in terms of research design and data requirements, which perhaps explains why Davidsson and Wiklund found little evidence of their increased use. The pursuit of linked micro-macro level studies to identify the contribution of entrepreneurship to wealth creation may make an important contribution to establishing the credibility of entrepreneurship research. Part of this story is to find ways of capturing the contribution of all kinds of new enterprise creation, not just a focus on new, independent small firms.

### ***Methodology***

Some methodological progress has been made in entrepreneurship research in the past decade. Chandler and Lyon (2000) in their review of developments show that there has been an increase in the use of multivariate techniques and some modest but greater utilization of reliability and validity tests. They also chart a marked downward trend in the number of studies using only univariate techniques.

However, much still needs to be done to fulfil Low and MacMillan's (1988) suggestions for improving methodology in entrepreneurship research. There remains a need for further progress in theory driven research. The main specialist entrepreneurship journals are increasingly insisting on theory development in empirical studies. In order to be published in mainstream journals, entrepreneurship research probably also faces greater pressure to demonstrate theoretical underpinnings. Yet there remains a considerable section of research and researchers in the entrepreneurship field for whom empirical studies are at best atheoretical. This may be a reflection of an emphasis on the implications of research for practitioners and policymakers. However, we oppose the view that theory driven research is unlikely to be of relevance to policymakers. On the contrary, good policy relevant research needs a strong conceptual base. Otherwise, policies are likely to be based on ad hocery.

Limited use of alternative data sources continues to pose a problem for entrepreneurship research. Chandler and Lyon (2000) find that two thirds of entrepreneurship studies used paper surveys but only 5% combined primary and secondary data. There continues, therefore, to be a danger of common method variance problems. Using multiple sources is time-consuming and poses difficulties, particularly in entrepreneurship research – entrepreneurial ventures are typically dominated by one person and reliable archival data may be difficult to come by. However, in some countries, archival data are available and there may be advantages to combining this with questionnaire data that may measure variables not contained in financial reports in order to create rich data sets. The focus on process and on new enterprise rather than firm level creates additional challenges, as neither archival nor survey data are ideal for such purposes. Therefore, ethnographic case studies and simulations deserve increased emphasis in the future.

Although an individual may be dominant in an entrepreneurial venture, we know from previous studies that they often involve partners and various sources of finance. Yet, we know little about the contributions of these other actors. This problem may be the result of work at the entrepreneur level of analysis that has focused on the characteristics of the entrepreneur. A shift in focus towards the study of the creation of enterprise opens up the need to consider the role of these other actors so that the study of entrepreneurial teams may be one clear beneficiary of such a change of emphasis. For example, team start-ups are abundant in young industries, and management buy-outs and buy-ins

typically involve several individuals in equity holding positions (Robbie and Wright, 1996). This clearly suggests that multiple informants be used. As a second example, in spin-outs of inventions and innovations from universities there is a need here both to examine the role of the academic as entrepreneur but also the potential role of others who may be crucial to the exploitation of the entrepreneurial opportunity. In this context, Radosevich (1995) and Franklin and Wright (2000) analyze the roles of academic and surrogate entrepreneurs.

### *Timeframe*

There is a need for further longitudinal entrepreneurship research. Low and MacMillan (1988) emphasized the need for entrepreneurship research to chart the development of entrepreneurial ventures over time. Chandler and Lyon (2000) find that only 7% of studies were true longitudinal studies with data collected at two or more points in time, frequent interviewing or multi-year analysis of data. Why has development been so slow?

It may be that the demands of tenure dissuade researchers from investing considerable effort in long-term projects that may bear little fruit. In this case, there is some irony that the students of risky situations are themselves risk averse! But this too is a reflection of the incentives on researchers.

Longitudinal research is also difficult to do. The small and private nature of entrepreneurial ventures very often mean that in some countries, time series data are not available. While there is a problem in the US, it is not always the case elsewhere, for example in the UK and Sweden. This difference in data availability suggests scope for international collaboration.

The high failure rate of entrepreneurial ventures also means high dropout rates when ventures are revisited over time. But this feature of entrepreneurial samples can surely be turned to advantage. If researchers heed the call noted earlier about the necessity for greater understanding of the entrepreneurial process, those ventures (or individuals) that exit the sample are important observations. At the very least, this opens the way for survivor and hazard function analysis of entrepreneurial ventures over time. But it also may go some way towards addressing the selection

bias of much of entrepreneurship research that focuses solely on successful or surviving enterprises. As pointed out by Aldrich and Martinez we do not know enough about those firms that fail, yet there may be important insights to be derived from studying failure (McGrath, 1999). Similarly, following such cohorts can be the basis for charting the development of habitual entrepreneurs, arguably the most dynamic contributors to the changing business landscape but about whom we still know relatively little (Ucbasaran *et al.*).

Other problems in addition to attrition arising from failure pose serious obstacles to longitudinal research. Refusal to continue to co-operate is one obvious shortcoming. Less obvious, but at least equally challenging, is the fact that over longer periods of time the originally chosen unit of analysis may undergo such changes that it is questionable whether it is still in a meaningful manner 'the same' as that originally sampled (cf. Davidsson and Wiklund, 2000). This is where the individual has its great advantage as the unit of analysis. Unlike a 'firm' or an 'economic activity' individuals remain clearly identifiable over extended periods of time.

## **Conclusions**

Has there been progress in entrepreneurship since Low and MacMillan (1988) published their article? According to the contributors to this Special Issue this is definitely the case. As regards the individual, the focus seems to have shifted away from the relative 'dead end' of stable psychological characteristics towards behavior and teach- and learnable cognitive issues. The distinction between novice vs. habitual entrepreneurs is increasingly being recognized. Low and MacMillan stressed the need for increased emphasis on context and process. As regards the former, the field has been extended to include new economic activity in various organizational contexts. We have also seen extensive empirical research on regional characteristics conducive of entrepreneurship. Within one specific theoretical perspective—Evolutionary Theory—substantial progress is being reported (Aldrich & Martinez) and with some additional empirical research and synthesis we are likely to be able to say the same within the near future about resource-based approaches. As regards process, longitudinal studies are still in short supply. Some methodological improvement is evident, though, in the form of increased use of multivariate techniques and checks of validity and reliability.

Despite this progress there is also a shared frustration among many contributors about the remaining 'hodgepodge' or 'potpourri' character of entrepreneurship research. Conceptual contributions to the field have clearly de-emphasized 'characteristics of small business owner-managers and their firms' in favor of *behaviors* associated with *emergence*. It is not clear, however, that empirical research published in entrepreneurship journals has followed suit. This may have to do with researchers' tendencies to work with data that are readily available rather than data that are the most important (Cooper, 1995). There is a risk that the broadening (to different organizational contexts or modes of exploitation) side of the re-focusing has started to work whereas the narrowing side is lagging behind, and that the boundaries of the domain therefore are even more fuzzy today than they were twelve years ago.

One realistic way to deal with this is to accept that the basis for community in entrepreneurship is a shared interest in a set of loosely interrelated empirical phenomena such as emergence of new enterprise and new organizations, innovation, venture capital, small business and family firms. This is surrendering to 'entrepreneurship as potpourri' as discussed by Low. The situation could be somewhat improved if the members of this community, as authors and with the help of reviewers and editors, were somewhat more careful with the 'e-words'. 'Entrepreneurship' could be used as an admittedly fuzzy meta-concept whereas titles of articles and labels for empirical variables would be more precise. For example, a study of regional differences in firm start-up rates, or a study of firm size and ownership as related to rates of product innovation, would claim to study precisely that, and not differences in 'entrepreneurship'. Such studies may find their outlet in journals perceived as 'entrepreneurship journals' or in more discipline-oriented outlets. Individual researchers might build a successful career and respect within a discipline from their interest in 'entrepreneurship'. As pointed out by Low, however, the 'entrepreneurship research' label itself would likely be doomed to second class status, and as pointed out by Gartner there is no room for theory as the basis for community if entrepreneurship research remains a potpourri.

The alternative is perhaps more attractive but also more difficult to achieve. This would involve carving out a more distinctive domain, firmly anchored in the disciplines (cf. Low). For this to be a realistic route, journal editors and conference organizers would have to agree upon a narrower set of empirical phenomena and stronger demands on theoretical anchoring. The question is whether

sufficiently broad agreement can be achieved, and whether it is realistic to assume that those excluded by the agreed upon delineation of the domain would find reason to refrain from continuing to use the 'e-words' for whatever phenomenon they like.

Differences of opinion regarding what should be included in the concept of entrepreneurship typically include whether only commercial or also not-for-profit activities belong there; whether it is associated only with young and independent organizations or may appear also in old and corporate contexts; whether the essence of entrepreneurship is disposition, behavior or outcomes; how innovative an activity has to be to qualify as entrepreneurship; whether success is required and, if so, success on what level? Some of these sources of differences may be easier to reconcile if it is made explicit whether we talk about entrepreneurship as an *economic phenomenon*, as a *research domain*, or as a *teaching subject*.

There seems to be a movement towards consensus that entrepreneurship is about *emergence*. Gartner's view that it is about emergence of new organizations is narrowly focused, addresses an ecological void, and holds promise of yielding teach- and learnable knowledge. The question is why we should call this 'entrepreneurship' rather than 'organization creation' (Davidsson & Wiklund). Alternatively, entrepreneurship can be viewed as *emergence of new economic activity*, regardless of organizational context and admitting that similar processes take place in the non-commercial domain. Under this conceptualization, entrepreneurship is a matter of degree but includes both imitation and innovation (Aldrich & Martinez). Imitation means there is a new actor in the market that increases competition and may lead to incremental evolution whereas innovation—a higher degree of entrepreneurship—also leads to restructuring and revolution. It may be validly argued that entrepreneurship as *an economic phenomenon* is ultimately measured by the effect on the economy, and thus requires success. Entrepreneurship as *a research domain*, however, also needs to study failed attempts and to focus on behavior in order to explain entrepreneurial success. While micro level outcomes would be sufficient for most aspects of entrepreneurship as *a teaching subject*, entrepreneurship as *a research domain* needs to address also the societal level outcomes, because it is important for policy-makers to know what the role of entrepreneurship is in societal development, but also for reasons of legitimacy.

In our view, despite the apparent progress of the past decade, entrepreneurship research will remain vulnerable unless a stronger paradigm is developed. The real world phenomena addressed in entrepreneurship research are important and will hopefully continue to attract researcher interest regardless of the fate of 'entrepreneurship research' as a domain or community. We believe, however, that the body of cumulative knowledge about these phenomena will grow faster if carried out within a more distinctly defined domain and community, firmly anchored in the disciplines. Only on this basis may entrepreneurship emerge over time as a field in its own right.

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